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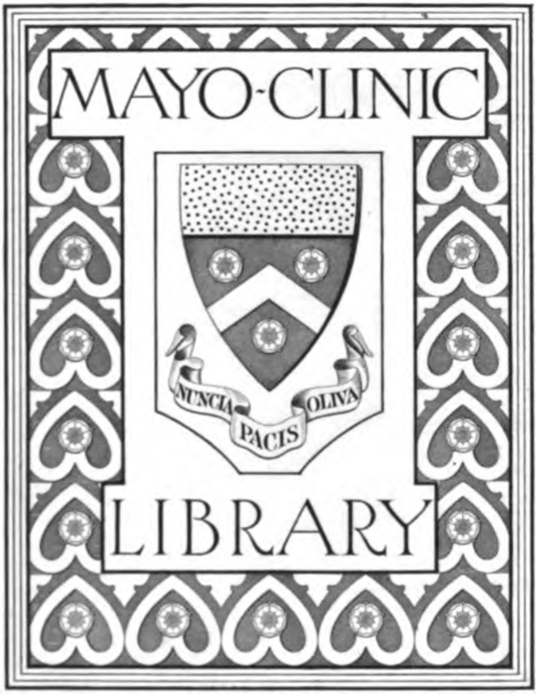
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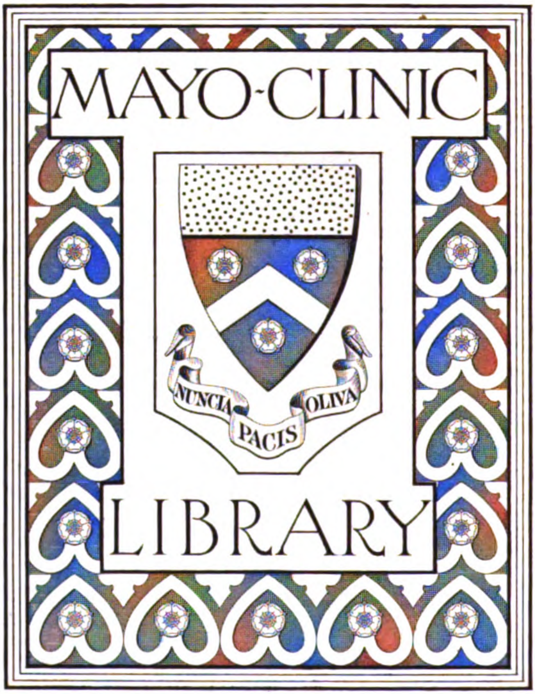
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NOTICES
OF THE
PROCEEDINGS
AT THE
MEETINGS OF THE MEMBERS
OF THE
Royal Institution of Great Britain,
WITH
ABSTRACTS OF THE DISCOURSES
DELIVERED AT
THE EVENING MEETINGS.

VOL. V.
1866—1869.

ROYAL COLLEGE OF SURGEONS OF ENGLAND.

ROYAL COLLEGE OF SURGEONS OF ENGLAND.

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CONTENTS.

1866.

Page

Nov. 5.—General Monthly Meeting 1

Dec. 3.—General Monthly Meeting 3

1867.

Jan. 18.—Professor Tyndall—On Sounding and Sensitive Flames 6

„ 25.—Professor Odling—On Mr. Graham's Recent Discoveries on the Diffusion of Gases 12

Feb. 1.—J. Scott Russell, Esq.—On the Crystal Palace Fire 18

„ 4.—General Monthly Meeting 24

„ 8.—Rev. F. W. Farrar—On some Defects in Public School Education 26

„ 15.—C. F. Varley, Esq.—On the Atlantic Telegraph 45

„ 22.—M. D. Conway, Esq.—On New England 59

Mar. 1.—Captain V. D. Majendie—On Military Breech-loading Small Arms 62

„ 4.—General Monthly Meeting 76

„ 8.—Rev. W. Greenwell—On the Yorkshire Wold Tumuli 78

„ 15.—E. B. Tylor, Esq.—On Traces of the Early Mental Condition of Man 83

1867. Page

Mar. 22.—Dr. James Bell Pettigrew—On the various modes of Flight in relation to Aeronautics 94

„ 29.—Professor Frankland—On the Water Supply of the Metropolis 109

April 1.—General Monthly Meeting 127

„ 5.—W. Pengelly, Esq.—On the Insulation of St. Michael's Mount, Cornwall 128

„ 12.—Balfour Stewart, Esq.—On the Sun as a Variable Star 138

May 1.—Annual Meeting 144

„ 3.—Professor Blackie—On the Music of Speech in the Greek and Latin Languages 145

„ 6.—General Monthly Meeting 155

„ 10.—Professor Bain—On the Doctrine of the Correlation of Force in its Bearing on Mind 157

„ 17.—Professor Odling—On the Occlusion of Gases by Metals 159

„ 24.—Professor Herschel—On the Shooting Stars of the Years 1866–67, and on the probable Source of certain Luminous Meteors in the material Substance of the Zodiacal Light 164

„ 31.—T. Sterry Hunt, Esq.—On the Chemistry of the Primeval Earth 178

June 3.—General Monthly Meeting 186

„ 7.—John Ruskin, Esq.—On the Present State of Modern Art with reference to the advisable Arrangements of a National Gallery (*no Abstract*) 187

„ 21.—Professor Tyndall—On some Experiments of Faraday, Biot, and Savart 188

July 1.—General Monthly Meeting 190

Nov. 4.—General Monthly Meeting 193

Dec. 2.—General Monthly Meeting 197

1868.

1868. Page

Jan. 17. } „ 24. } **Professor Tyndall**—On Faraday as a Discoverer 199

Parentage: Introduction to the Royal Institution: Earliest Experiments: First Royal Society Paper: Marriage .. 199

Early Researches: Magnetic Rotations: Liquefaction of Gases: Heavy Glass: Charles Anderson: Contributions to Physics 203

Discovery of Magneto-electricity: Explanation of Arago's Magnetism of Rotation: Terrestrial Magneto-electric Induction: The Extra Current 207

Points of Character 214

Identity of Electricities: First Researches on Electro-Chemistry 216

Laws of Electro-Chemical Decomposition 222

Origin of Power in the Voltaic Pile 224

Researches on Frictional Electricity: Induction: Conduction: Specific Inductive Capacity: Theory of Contiguous Particles 227

Rest needed—Visit to Switzerland 231

Magnetization of Light 233

Discovery of Diamagnetism—Researches on Magne-Crystallic Action 237

Supplementary Remarks 242

Magnetism of Flame and Gases: Atmospheric Magnetism 245

Speculations: Nature of Matter: Lines of Force 250

Unity and Convertibility of Natural Forces: Theory of the Electric Current 256

Summary 261

Illustrations of Character 262

Jan. 31.—**Rev. F. W. Farrar**—On Public School Education 273

Feb. 3.—General Monthly Meeting 276

„ 7.—**Professor Huxley**—On the Animals which are most nearly intermediate between Birds and Reptiles 278

„ 14.—**Professor H. E. Roscoe**—On Vanadium, one of the Trivalent Group of Elements 287

„ 21.—**Rev. M. W. Mayow**—On Hamlet 295

1868. Page
Feb. 28.—A. Vernon Harcourt, Esq.—On the Rate at which Chemical Actions take place 304
March 2.—General Monthly Meeting 308
„ 6.—W. Kingdon Clifford, Esq.—On some of the Conditions of Mental Development 311
„ 13.—Professor W. Stanley Jevons—On the Probable Exhaustion of our Coal Mines 328
„ 20.—Professor A. Matthiessen—On Alloys and their Uses 335
„ 27.—Dr. W. B. Carpenter—On the Unconscious Activity of the Brain 338
April 3.—Professor Frankland—On the Proposed Water Supply for the Metropolis 346
„ 6.—General Monthly Meeting 370
„ 24.—Dr. J. H. Gladstone—On some New Experiments on Light 371
May 1.—Annual Meeting 375
„ 1.—F. T. Palgrave, Esq.—How to form a Good Taste in Art 376
„ 4.—General Monthly Meeting 377
„ 8.—C. G. Williams, Esq.—On the Artificial Formation of Organic Substances 378
„ 15.—E. Deutsch, Esq.—On the Talmud 386
„ 22.—Professor Odling—On some Effects of the Heat of the Oxy-hydrogen Flame 391
„ 29.—W. E. H. Lecky, Esq.—On the Influence of the Imagination on History 394
June 1.—General Monthly Meeting 402
„ 5.—Sir S. W. Baker—On Abyssinia, or Ethiopia .. 404
„ 12.—Professor Frankland—On the Source of Light in Luminous Flames 419
July 6.—General Monthly Meeting 423
Nov. 2.—General Monthly Meeting 425
Dec. 7.—General Monthly Meeting 427

1869.

1869. Page

Jan. 15.—Professor Tyndall—On Chemical Rays, and the Light of the Sky 429

„ 22.—Professor Alexander Herschel—On the latest Eclipse of the Sun (*no Abstract*) 450

„ 29.—John Ruskin, Esq. M.R.I.—On the Flamboyant Architecture of the Valley of the Somme (*no Abstract*) 450

Feb. 1.—General Monthly Meeting 451

„ 5.—James Fergusson, Esq. F.R.S.—On Tree and Serpent Worship 453

„ 12.—Colonel W. F. Drummond Jervois—On the Coast Defences of England 458

„ 19.—C. Greville Williams, Esq.—On the Female Poisoners of the Sixteenth and Seventeenth Centuries (*no Abstract*) 470

„ 26.—Dr. John H. Bridges—On the Influence of Civilization upon Health 470

March 1.—General Monthly Meeting 474

„ 5.—W. Huggins, Esq.—On some further Results of Spectrum Analysis as applied to the Heavenly Bodies 475

„ 12.—Professor Abel—On some Applications of Electricity to Naval and Military purposes 479

„ 19.—Dr. Crum Brown—On Chemical Constitution and its relation to Physical and Physiological Properties 495

April 5.—General Monthly Meeting 501

„ 9.—Dr. William B. Carpenter, M.D. V.P.R.S.—On the Temperature and Animal Life of the Deep Sea .. 503

„ 16.—William Carruthers, Esq. F.L.S.—On the Cryptogamic Forests of the Coal Period 511

1869. Page

April 23.—E. B. Tylor, Esq.—On the Survival of Savage Thought in Modern Civilization 522

„ 30.—Robert H. Scott, Esq. M.A.—On the Work of the Meteorological Office, Past and Present .. 535

May 1.—Annual Meeting 547

„ 3.—General Monthly Meeting 548

„ 7.—Captain Moncrieff—On the Moncrieff System of Working Artillery as applied to Coast Defence .. 550

„ 14.—W. H. Perkin, Esq. F.R.S.—On the Newest Colouring Matters 566

May 21.—H. C. Fleeming Jenkin, Esq. F.R.S.—On the Submersion and Recovery of Submarine Cables .. 574

„ 28.—J. Norman Lockyer, Esq. F.R.S.—On Recent Discoveries in Solar Physics made by means of the Spectroscope 580

June 4.—Professor Odling, M.B. F.R.S.—On the Simplest Organic Compounds 598

June 7.—General Monthly Meeting 605

July 5.—General Monthly Meeting 607

Nov. 1.—General Monthly Meeting 609

Dec. 6.—General Monthly Meeting 611

Index 613

Royal Institution of Great Britain.

1866.

GENERAL MONTHLY MEETING,

Monday, November 5th, 1866.

William Pole, Esq. M.A. F.R.S. in the Chair.

Samuel Osborne Habershon, M.D.
was *admitted* a Member of the Royal Institution.

The Special Thanks of the Members were returned to the following Contributors to "the Donation Fund for the Promotion of Experimental Researches:"—

Arthur Giles Puller, Esq. (*2nd donation*) . .	£21 0 0
Benjamin Leigh Smith, Esq.	10 10 0

and to **Henry Wilde**, Esq., for his valuable present of his Magneto-Electric Machine.

The **Presents** received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

Trustees of British Museum—List of Lepidopterous Insects: Part 35. Supp. 5. 12mo. 1866.
Guide to Exhibition Rooms. 8vo. 1866.
Académie Royale de Belgique—Bulletins, 1865, 1866. 8vo. Almanach, 1866. 16to.
Actuaries, Institute of—Journal, No. 64, 65. 8vo. 1866.
Agricultural Society of England, Royal—Journal. Second Series: Vol. II. Part 2. 8vo. 1866.
American Academy of Arts and Sciences—Proceedings, Vol. VI. Nos. 39–63. Vol. VII. Nos. 1–12. 8vo. 1864–6.
American Philosophical Society—Proceedings, No. 75. 8vo. 1866.
Catalogue of Library. Part 2. 8vo. 1866.
Architects, Royal Institute of British—Proceedings. Part III. No. 3. 4to. 1866.
Asiatic Society of Bengal—Journal, Nos. 131, 132. 8vo. 1865–6.
Proceedings, 1865 and 1866. Nos. 1–3. 8vo.
Astronomical Society, Royal—Monthly Notices, 1865-6. Nos. 8, 9. 8vo.
Memoirs. Vol. XXXIV. 4to. 1866.
Bavarian Academy of Science, Royal—Sitzungsberichte, 1866. Band I. 3. 8vo.
Berthon, P. H. Esq. Secretary, Trinity House—Account of the Corporation of Trinity House, of Deptford Strond, and of Sea-marks in general. By John Whormby. 8vo. 1746. [Reprint, 1861]

Boston Society of Natural History, U.S.—Condition, &c. 8vo. 1865.
Proceedings, Vol. X. Nos. 1–18. 8vo. 1864–6.
British Association for the Advancement of Science—Report of the Thirty-fifth Meeting, held at Birmingham, September, 1865. 8vo. 1866.
Chemical Society—Journal for July to October, 1866. 8vo.
Editors—American Journal of Science, May, July, 1866. 8vo.
Artizan for July to October, 1866. 4to.
Athenæum for July to October, 1866. 4to.
British Journal of Photography for July to October, 1866. 4to.
Chemical News for July to October, 1866. 4to.
Engineer for July to October, 1866. fol.
Horological Journal for July to October, 1866. 8vo.
Journal of Gas-Lighting for July to October, 1866. 4to.
Mechanics' Magazine for July to October, 1866. 8vo.
Pharmaceutical Journal for July to October, 1866, and Calendar for 1867.
Practical Mechanics' Journal for July to October, 1866. 4to.
Foster, B. W. M.D. (the Author)—On the Use of the Sphygmograph in the Investigation of Disease. 8vo. 1866.
Francis, James B. Esq. (the Author)—On the Strength of Cast-Iron Pillars. 8vo. 1865.
Frankland, Professor E., F.R.S. (the Author)—Lecture Notes for Chemical Students: embracing Mineral and Organic Chemistry. 12mo. 1866.
Franklin Institute—Journal, Nos. 486-489. 8vo. 1866.
Geographical Society, Royal—Proceedings, Vol. X. Nos. 4, 5. 8vo. 1866.
Journal, Vol. XXXV. 8vo. 1866.
Geological Institute, Royal, Vienna—Jahrbuch, Band XV. No. 4: Band XVI. No. 1. 4to. 1865–6.
Geological Society—Quarterly Journal, No. 87. 8vo. 1866.
Geological Society of Ireland, Royal—Journal, Vol. I. Part 2. 8vo. 1866.
Greenwich Observatory (through the Royal Society)—Greenwich Observations in 1864. 4to. 1866.
Grove, W. R. Esq. Q.C. F.R.S. M.R.I. (the Author)—Address of British Association, Aug. 22, 1866. 8vo.
Hofmann, A. W. LL.D. F.R.S. (the Author)—Chemical Laboratories in the Universities of Bonn and Berlin. 4to. 1866.
Horticultural Society, Royal—Proceedings, 1866. No. 5. 8vo.
Journal, Vol. I. No. 3. 1866. 8vo.
Hull Literary and Philosophical Society—Report for 1866. 8vo. 1866.
Institution of Civil Engineers—Minutes of Proceedings. Vols. XXII. XXIII. XXIV. and XXV. 8vo. 1862–66.
Catalogue of the Library. 2nd ed. 8vo. 1866.
Irish Academy, Royal—Transactions, Vol. XXIV. Five Parts. 4to. 1865–6.
Ladd, Mr. W.—The Inductorium; by H. M. Noad. 16to. 1866.
Lankester, E. M.D. F.R.S. M.R.I. (the Author)—Cholera. 16to. 1866.
Linnean Society—Journal and Proceedings: Zoology, No. 34. 8vo. 1866.
Longmans & Co. Messrs.—St. Bartholomew's Hospital Reports. Vol. II. 8vo. 1866.
Manchester Literary and Philosophical Society—Memoirs: Third Series. Vol. II. 8vo. 1865.
Proceedings, Vols. III. and IV. 8vo. 1864-5.
Manning, Frederick, Esq. M.R.I. (the Editor)—Series of Views intended to illustrate C. Cotton's Work, entitled the Second Part of the 'Complete Angler,' and other Views. 4to. 1866.
Mechanical Engineers' Institution, Birmingham—Proceedings, Nov. 1865; Jan. and May, 1866. 8vo.
Medico-Chirurgical Society, Royal—Proceedings Vol. V. No. 5. 8vo. 1866.
Nauroji, D. Esq. M.R.I. (the Author)—Observations on Mr. Crawford's Paper on the European and Asiatic Races. (Ethnological Soc. Trans. 1866.) 8vo.
Oakes, Lieut.-Colonel W. H. A.I.A.—Table of the Reciprocals of Numbers from 1 to 100,000, with their Differences. 8vo. 1865.

Payne, Joseph, Esq. M.R.I. (*the Author*)—The Curriculum of Modern Education (L 14) 8vo. 1866.
Photographic Society—Journal, No. 171–173. 8vo. 1866.
Plateau, M. Hon. M.R.I. (*the Author*)—Recherches sur les Figures d'Equilibre. 4to. 1866.
Poor-Law Board—Dr. E. Smith's Report on Metropolitan Workhouse Infirmaries. (P 9) Fol. 1866.
Raumer, M. von Friedrich, Hon.M.R.I. (*the Author*)—Handbuch zur Geschichte der Literatur. Theil III. & IV. 8vo. 1866.
Roma, Accademia de' Nuovi Lincei—Atti: Anno XVIII. 4to. 1866.
Royal Society of London—Proceedings, No. 85, 86. 8vo. 1866.
Philosophical Transactions, 1866. Vol. CLVI. Part 1. 4to. 1866.
St. Petersburg Imperial Academy of Sciences—Mémoires: VII^e Série. Tome IX. et Tome X. Nos. 1, 2. 4to. 1865–6.
Bulletins, Tome IX. 4to. 1865.
Society of Antiquaries—Proceedings, Vol. II. No. 7. 8vo. 1864.
Catalogue of a Collection of Printed Broadsides in the possession of the Society of Antiquaries, London; by R. Lemon. 8vo. 1866.
Squire, A. Balmanno, M.B. (*the Author*)—Chromo-Lithographs of the Diseases of the Skin. 4to. 1866.
Statistical Society of London—Journal, Vol. XXIX. Parts 2 and 3. 8vo. 1866.
Surgeon-General, United States' Army—Reports on Materials for Medical and Surgical History of the Rebellion. 4to. 1866.
Teyler Museum, Haarlem—Archives. Vol. I. Fasc. 1. 8vo. 1866.
United Service Institution, Royal—Journal, No. 39. 8vo. 1866.
United States Naval Observatory—Astronomical and Meteorological Observations for 1863. 4to.
United States' Sanitary Commission—Documents. Vols. I. II. 8vo. 1866.
Bulletin. Vols. I.-III. 8vo. 1863–5.
Vereins zur Beförderung des Gewerbsfleisses in Preussen—Verhandlungen, März-Juni, 1866.
Volpicelli, Professor (*the Author*)—Richerche Analitiche sul Bifilare, &c. 4to. Roma, 1865.
Zoological Society of London—Transactions. Vol. V. Part 5. 4to. 1866.
Proceedings, 1865. 8vo. Report, 1866. 8vo.

GENERAL MONTHLY MEETING,

Monday, Dec. 3, 1866.

LIEUT.-GEN. EDWARD SABINE, R.A. D.C.L. President R.S.
Vice-President, in the Chair.

John Augustus Seymour Morse Davies, Esq.
Thomas Dell, Esq. F.R.A.S.
Frederick Thomas Elworthy, Esq.
Mrs. Ellen Hawkins.
Robert Pilkington Linton, Esq.
Alfred Sandilands, Esq.

were *elected* Members of the Royal Institution.

The Managers reported, That the following Resolution was passed at their Meeting held this day :—

> "Resolved, That the Committee of Managers have learnt with regret the decease of John Pepys, Esq. (on Nov. 13), who was for a period of sixty-six years a Member of this Institution; and in their noticing his death they cannot but remember the great interest he ever took in this Institution, and the substantial support he rendered to it." *

Whereupon it was proposed by Mr. Wm. Pole, seconded by Dr. Lewis Powell, and

> "Resolved unanimously, That this Meeting heartily concurs in the sentiments expressed in the Resolution of the Managers."

The Special Thanks of the Members were returned for the following Contribution to "the Donation Fund for the Promotion of Experimental Researches :"—

Henry Vaughan, Esq. (*2nd donation*) . . .	£20 0 0

The following Lecture Arrangements for the ensuing Season were announced :—

Christmas Lectures, 1866. (*Adapted to a Juvenile Auditory.*)

Professor Frankland, F.R.S.—Six Lectures, 'On the Chemistry of Gases.' On December 27th, 29th, 1866; January 1st, 3rd, 5th, 8th, 1867.

Before Easter, 1867.

Rev. Charles Kingsley.—Three Lectures, 'On the Ancien Régime, as it existed on the Continent before the French Revolution.' On Tuesday, Thursday, and Saturday, January 15th, 17th, and 19th.

Professor Tyndall, F.R.S.—Twelve Lectures, 'On Vibratory Motion, with Special Reference to Sound.' On Tuesdays and Thursdays, January 22nd to February 28th.

G. A. Macfarren, Esq.—Six Lectures, 'On Harmony.' On Saturdays, January 26th to March 2nd.

Rev. G. Henslow.—Six Lectures, 'On the Practical Study of Botany.' On Tuesdays, March 5th to April 9th.

Professor Frankland, F.R.S.—Six Lectures, 'On Coal Gas.' On Thursdays and Saturdays, March 7th to 23rd.

William Pengelly, Esq. F.R.S.—Six Lectures, 'On Geological Evidences in Devonshire of the Antiquity of Man.' On Thursdays and Saturdays, March 28th to April 13th.

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* In the Report of the General Monthly Meeting, June 5, 1854 (Proceedings, R.I. Vol. I. p. 455), will be found a letter from Mr. Pepys, accompanying his fifth donation of 100*l.* and the Resolution passed thereon.

The **Presents** received since the last Meeting were laid on the table, and the thanks of the Members returned for the same: viz.—

FROM

Lords of the Admiralty—Nautical Almanack for 1870. 8vo. 1866.
Trustees of the British Museum—Catalogue of Fishes. Vol. VI. 8vo. 1866.
  Catalogue of Meteorites. 8vo. 1866.
  Index to Collection of Minerals. 8vo. 1866.
  Guide to Minerals. 8vo. 1866.
  Guide to Autograph Letters, &c. 12mo. 1866.
Architects, Royal Institute of British—Proceedings, 1866–7. Part I. No. 1. 4to.
Authors—Researches in Solar Physics. Second Series. By W. De la Rue, Balfour Stewart, and Benjamin Loewy. (Phil. Trans. 1866.)
Balfour, Major-General, C.B. (the Author)—On the Budgets and Accounts of England and France. (Journal Stat. Soc. 1866.) 8vo. 1866.
Bernard, Lieut.-Col. Peter—Anacalypsis; or an Inquiry into the Origin of Languages, Nations, and Religions. By Godfrey Higgins. 2 vols. 4to. 1836.
British Pharmaceutical Conference—Proceedings. 1866. 8vo.
Chemical Society—Journal for November, 1866. 8vo.
Editors—Artizan for November, 1866. 4to.
  Athenæum for November, 1866. 4to.
  British Journal of Photography for November, 1866. 4to.
  Chemical News for November, 1866. 4to.
  Engineer for November, 1866. fol.
  Horological Journal for November, 1866. 8vo.
  Journal of Gas-Lighting for November, 1866. 4to.
  Mechanics' Magazine for November, 1866. 8vo.
  Pharmaceutical Journal for November, 1866.
  Practical Mechanics' Journal for November, 1866. 4to.
Faraday, Professor, D.C.L. F.R.S.—Archives Néerlandaises des Sciences; publiées par la Société Hollandaise des Sciences à Harlem. Tome I. Liv. 1–4. 1866.
  Abhandlungen der Akademie der Wissenschaften zu Berlin: 1864. 4to. 1865.
  Reale Accademia delle Scienze di Torino; Memorie; Serie Seconda. Tomo XXI. 4to. 1865. Atti: Disp. 1, 2. 8vo. 1866.
  Ad. Quetelet: Sciences Mathématiques et Physiques chez les Belges au Commencement du XIX^e Siècle. 8vo. Bruxelles, 1866.
  Astronomical Observations at Leyton Observatory, Essex. 1862-4. 4to. 1865.
  J. S. Stas: Nouvelles Recherches sur les Lois des Proportions Chimiques, sur les Poids Atomiques, et leurs Rapports Mutuels. 4to. Bruxelles. 1865.
  Annual of the National Academy of Sciences for 1865. Cambridge, U.S. 1866.
  Atlantic Telegraph Cable: Documents. (K 94) 8vo. 1866.
  C. N. Doughty: Glaciers of Norway. (K 94) 8vo. 1866.
  M. Seguin: Causes et Effets de la Chaleur, de la Lumière et de l'Electricité. (L 14) 8vo. 1865.
Franklin Institute—Journal, No. 490. 8vo. 1866.
Geological Society—Quarterly Journal, No. 88. 8vo. 1866.
Hope, Alex. J. B. Beresford, Esq. M.P. (the Author)—Address to Royal Institute of British Architects, Nov. 5, 1866. 4to.
Huxley, Professor T. H. F.R.S. (the Author)—Lessons in Elementary Physiology. 16to. 1866.
Linnean Society—Journal and Proceedings: Botany, No. 38. 8vo. 1866.
Medical and Chirurgical Society, Royal—Medico-Chirurgical Transactions. Vol. XLIX. 8vo. 1866. Additions to Library. No. 9. 1866. 8vo.
Meteorological Society—Proceedings, No. 26. 1866.
Photographic Society—Journal, No. 175. 8vo. 1866.
Truman, Edwin, Esq. M.R.I. (the Editor)—Archives of Dentistry. Vol. I. 8vo. 1865.
Tuke, Thos. Harrington, M.D. M.R.I.—Study of Hamlet, by J. Conolly. 16to. 1863.
Murray, Adam, Esq. M.R.I.—A Small Box of Minerals.

1867.

WEEKLY EVENING MEETING,

Friday, January 18, 1867.

JOHN TYNDALL, Esq. LL.D. F.R.S.

PROFESSOR OF NATURAL PHILOSOPHY, R.I. ETC.

On Sounding and Sensitive Flames.

HISTORICAL.

THE sounding of a hydrogen flame when enclosed within a glass tube was, I believe, first noticed by Dr. Higgins, in 1777. The subject has been since investigated by Chladni, De la Rive, Faraday, Wheatstone, Rijke, Sondhauss, and Kundt. The action of unisonant sounds on flames enclosed in tubes has been investigated by Count Schaffgotsch and myself. The jumping of a *naked* fish-tail flame, in response to musical sounds, was first noticed by Professor Leconte at a musical party in the United States. He made the important observation that the flame did not jump until it was near *flaring*. That his discovery was not further followed up by this learned investigator was probably due to too great a stretch of courtesy on his part towards myself.* Last year, while preparing the experiments for one of my "Juvenile Lectures," my late assistant, Mr. Barrett, observed the effect independently; and he afterwards succeeded in illustrating it by some very striking experiments. With a view to the present discourse, and also to the requirements of a forthcoming work on Sound, the subject of sounding and sensitive flames has been recently submitted to examination in the Laboratory of the Royal Institution. The principal results of the inquiry are embodied in the following abstract.

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* The observation of Professor Leconte is thus graphically described :—" Soon after the music commenced, I observed that the flame of the last-mentioned burner exhibited pulsations in height which were *exactly synchronous* with the audible beats. This phenomenon was very striking to every one in the room, and especially so when the strong notes of the violoncello came in. It was exceedingly interesting to observe how perfectly even the *trills* of this instrument were reflected on the sheet of flame. *A deaf man might have seen the harmony.* As the evening advanced, and the diminished consumption of gas in the city *increased the pressure*, the phenomenon became more conspicuous. The *jumping* of the flame gradually increased, became somewhat irregular, and, finally it began to flare continuously, emitting the characteristic sound indicating the escape of a greater amount of gas than could be properly consumed. I then ascertained by experiment, that the phenomenon *did not* take place unless the discharge of gas was so regulated, that the flame approximated to the condition of *flaring*. I likewise determined by experiment, that the effects *were not* produced by jarring or shaking the floor and

ABSTRACT OF LECTURE.

Pass a steadily-burning candle rapidly through the air, you obtain an indented band of light, while an almost musical sound heard at the same time announces the rhythmic character of the motion. If, on the other hand, you blow against a candle-flame, the fluttering noise produced indicates a rhythmic action.

When a fluttering of the air is produced at the embouchure of an organ-pipe, the resonance of the pipe reinforces that particular pulse of the flutter whose period of vibration coincides with its own, and raises it to a musical sound.

When a gas-flame is introduced into an open tube of suitable length and width, the current of air passing over the flame produces such a flutter, which the resonance of the tube exalts to a musical sound.

Introducing a gas-flame into this tin tube three feet long, we obtain a rich musical note; introducing it into a tube six feet long, we obtain a note an octave deeper—the pitch of the note depending on the length of the tube. Introducing the flame into this third tube, which is fifteen feet long, the sound assumes extraordinary intensity. The vibrations which produce it are sufficiently powerful to shake the pillars, floor, seats, gallery, and the five or six hundred people who occupy the seats and gallery. The flame is sometimes extinguished by its own violence, and ends its peal by an explosion as loud as a pistol shot.

The roar of a flame in a chimney is of this character: it is a rude attempt at music.

By varying the size of the flame, these tubes may be caused to emit their harmonic sounds.

Passing from large tubes to small ones, we obtain a series of musical notes, which rise in pitch as the tube diminishes in length.

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walls of the room by means of repeated concussions. Hence it is obvious that the pulsations of the flame *were not* owing to *indirect* vibrations propagated through the medium of the walls of the room to the burning apparatus, but must have been produced by the *direct* influence of aërial sonorous pulses on the burning jet."—'Phil. Mag.' 4th series, vol. xv. March, 1858, p. 235; and 'Silliman's American Journal,' Jan. 1858.

This flame, surrounded by a tube $17\frac{7}{8}$ inches long, vibrates 459 times in a second, while that contained in this tube, $10\frac{3}{8}$ inches long, vibrates 717 times in a second. Owing to the intense heat of the sounding column, these numbers are greater than those corresponding to organ-pipes of the same lengths sounding in air.

The vibrations of the flame consist of a series of partial extinctions and revivals of the flame.

The singing flame appears continuous; but if the head be moved to and fro, or if an opera-glass, directed to the flame, be caused to move to and fro; or if, after the method of Wheatstone, the flame be regarded in a mirror which is caused to rotate, the images due to the revivals of the flame are separated from each other, and form a chain of flames of great beauty.

With a longer tube and larger flame, by means of a concave mirror, I can project this chain of flames upon a screen. I first clasp my hand round the end of the tube so as to prevent the current of air which causes the flutter from passing over the flame:—the image of the flame is now steady upon the screen before you. I move the mirror to and fro, and you have this continuous luminous band: I withdraw my hand; the current of air passes over the flame, and instantly the band breaks up into a chain of images.

A position can be chosen in the tube at which the flame bursts spontaneously into song. A position may also be chosen where the flame is silent, but at which, if it could only be started, it would continue to sound. It is possible to start such a silent flame by a pitch-pipe, by the syren, or by the human voice. It is also possible to cause one flame to effect the musical ignition of another.

The sound which starts the flame must be nearly in unison with its own. Both flames must be so near unison as to produce distinct beats.

A flame may be employed to detect sonorous vibrations in air.

Thus, in front of this resonant case, which supports a large and powerful tuning-fork, I move this bright gas-flame to and fro. A continuous band of light is produced, slightly indented through the friction of the air. The fork is now sounded, and instantly this band breaks up into a series of distinct images of the flame.

Approaching the same flame, towards either end of one of our tin tubes, with the sounding flame within it, and causing it to move to and fro, the sonorous vibrations also effect the breaking up of the band of light into a chain of images.

In this glass-tube, fourteen inches long, a flame is sounding: I bring the flat flame of a fish-tail burner over the tube, the broad side of the flame being at right angles to the axis of the tube. The fish-tail flame instantly emits a musical note of the same pitch as that of the singing-flame, but of different quality. Its sound is, in fact, that of a membrane, the part of which it here plays.

Against a broad bat's-wing flame I allow a sheet of air, issuing from a thin slit, to impinge. A musical note is the consequence. The note can be produced by air or by carbonic acid; but it is produced with greater force and purity by oxygen. The pitch of the note depends on the distance of the slit from the flame.

Before you burns a bright candle-flame: I may shout, clap my hands, sound this whistle, strike this anvil with a hammer, or explode a mixture of oxygen and hydrogen. Though sonorous waves pass in each case through the air, the candle is absolutely insensible to the sound; there is no motion of the flame.

I now urge from this small blow-pipe a narrow stream of air through the flame of the candle, producing thereby an incipient flutter, and reducing the brightness of the flame. I now sound the whistle; the flame jumps visibly. Matters may be so arranged that when the whistle sounds, the flame shall be either almost restored to its pristine brightness, or that the amount of light it still possesses shall disappear.

Before you now burns a bright flame from a fish-tail burner. I may, as before, shout, clap my hands, sound a whistle, or strike an anvil; the flame remains steady and without response. I urge against the broad face of the flame a stream of air from the blow-pipe just employed. The flame is cut in two by the stream of air. It flutters slightly, and now when the whistle is sounded the flame instantly starts. A knock on the table causes the two half-flames to unite and form for an instant a flame of the ordinary shape. By a slight variation of the experiment, the two side-flames disappear when the whistle is sounded, and a central tongue of flame is thrust forth in their stead.

Passing from a fish-tail to a bat's-wing burner, I obtain this broad steady flame. It is quite insensible to the loudest sound which would be tolerable here. The flame is fed from this gas-holder, which places a power of pressure at my disposal unattainable from the gas-pipes of the Institution. I turn on more gas; the flame enlarges, but it is still insensible to sound. I enlarge it still more, and now a slight flutter of its edge answers to the sound of the whistle. Turning on a little more gas, and sounding again, the jumping of the flame is still more distinct. Finally I turn on gas until the flame is on the point of roaring, as flames do when the pressure is too great. I now sound my whistle; the flame roars and thrusts suddenly upwards eight long quivering tongues.

I strike this distant anvil with a hammer, the flame instantly responds by thrusting forth its tongues.

Another flame is now before you. It issues from a burner, formed of ordinary gas-tubing by my assistant. The flame is 18 inches long, and smokes copiously. I sound the whistle; the flame falls to a height of 9 inches, the smoke disappears, and the brilliancy of the flame is augmented.*

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* Mr. Barrett also observed the increase of light on the shortening of a flame by a musical sound; nor did the superior effect of high notes escape the attention of this acute and skilful young experimenter.

Here are two other flames, also issuing from burners formed by my assistant. The one of them is long, straight, and smoky; the other is short, forked, and brilliant. I sound the whistle; the long flame becomes short, forked, and brilliant; the forked flame becomes long and smoky. As regards, therefore, their response to the sonorous waves, the one of these flames is the exact complement of the other.

Here are various flat flames, ten inches high, and about three inches across at their widest part. They are purposely made forked flames. When the whistle sounds, the plane of each flame turns ninety degrees round, and continues in its new position as long as the whistle continues to sound.

Here again is a flame of admirable steadiness and brilliancy, issuing from a single circular orifice in a common iron nipple. I whistle, clap my hand, strike the anvil, and produce other sounds: the flame is perfectly steady. Observe the gradual change from this apathy to sensitiveness. The flame is now 4 inches high. I make its height 6 inches; it is still indifferent. I make it 10 inches; a barely perceptible quiver responds to the whistle. I make it 14 inches high, and now it jumps briskly the moment the anvil is tapped or the whistle sounded. I augment the pressure; the flame is now 16 inches long, and you observe a quivering which announces that the flame is near roaring. I increase the pressure; it now roars, and shortens at the same time to a height of 8 inches. I diminish the pressure a little; the flame is again 16 inches long, but it is on the point of roaring. It stands as it were on the brink of a precipice. *The whistle pushes it over.* Observe it shortens when the whistle sounds, exactly as it did when the pressure was in excess. The sonorous pulses, in fact, furnish the supplement of energy necessary to produce the roar and shorten the flame. This is the simple philosophy of all these sensitive flames.

The pitch of the note chosen to push the flame over the brink is not a matter of indifference. I have here a tuning-fork which vibrates 256 times in a second, emitting a clear and forcible note. It has no effect upon this flame. Here are three other forks, vibrating respectively 320, 384, and 512 times in a second. Not one of them produces the slightest impression upon the flame. But, besides their fundamental tones, these forks can be caused to sound a series of overtones of very high pitch. I sound this series of tones: the vibrations are now 1,600, 2,000, 2,400, and 3,200 per second respectively. The flame jumps in response to each of these sounds; the response to the highest tone of the series being the most prompt and energetic of all.

To the tap of a hammer upon a board the flame responds; but to the tap of the same hammer upon an anvil the response is much more brisk and animated. The reason is, that the clang of the anvil is rich in the higher tones to which the flame is most sensitive.

Here again is an inverted bell, which I cause to sound by means of a fiddle-bow, producing a powerful tone. The flame is unmoved. I bring a halfpenny into contact with the surface of the bell: the consequent rattle contains the high notes to which the flame is sensitive. It instantly shortens, flutters, and roars when the coin touches the bell.

Here is another flame, 20 inches long. I take this fiddle in my hand, and pass a bow over the three strings which emit the deepest notes. There is no response on the part of the flame. I sound the highest string: the jet instantly squats down to a tumultuous bushy flame, 8 inches long. I have here a small bell, the hammer of which is caused to descend by clock-work. I hold it at a distance of 20 yards from the flame. The strokes follow each other in rhythmic succession, and at every stroke the flame falls from a height of 20 to a height of 8 inches.

The rapidity with which sound is propagated through air is well illustrated by these experiments. There is no sensible interval between the stroke of the bell and the shortening of the flame.

Some of these flames are of marvellous sensibility; one such is at present burning before you. It is nearly 20 inches long; but the slightest tap on a distant anvil knocks it down to 8. I shake this bunch of keys or these few copper coins in my hand: the flame responds to every tinkle. I may stand at a distance of 20 yards from this flame: the dropping of a sixpence from a height of a couple of inches into a hand already containing coin, knocks the flame down. I cannot walk across the floor without affecting the flame. The creaking of my boots sets it in violent commotion. The crumpling of a bit of paper, or the rustle of a silk dress, does the same. It is startled by the plashing of a raindrop. I speak to the flame, repeating a few lines of poetry; the flame jumps at intervals, apparently picking certain sounds from my utterance to which it can respond, while it is unaffected by others.

In our experiments downstairs we have called this the vowel flame, because the different vowel-sounds affect it differently. Vowel-sounds of the same pitch are known to be readily distinguishable. Their qualities or clang-tints are different, though they have a common fundamental tone. They differ from each other through the admixture of higher tones with the fundamental. It is the presence of these higher tones in different proportions that characterizes the vowel sounds; and it is to these same tones, and not to the fundamental one, that our flame is sensitive. I utter a loud and sonorous U, the flame remains steady; I change the sound to O, the flame quivers; I sound E, and now the flame is affected strongly. I utter the words *boot*, *boat*, and *beat* in succession. To the first there is no response; to the second, the flame starts; but by the third it is thrown into violent commotion; the sound *Ah!* is still more powerful. When the vowel sounds are analysed their constituents are found to vary in accordance with the foregoing experiments; those characterized by the sharpest overtones being the most powerful excitants of the flame. (*See* Helmholtz in Pogg. Annalen, vol. cviii. p. 286.)

The flame is peculiarly sensitive to the utterance of the letter S. If the most distant person in the room were to favour me with a "hiss," the flame would be instantly shivered into tumult. The utterance of the word "hush," or "puss," produces the same effect. This hissing sound contains the precise elements that most forcibly affect the flame. The gas issues from its burner with a hiss, and an external sound of this character added to that of a gas-jet already on the point of roaring is equivalent to an augmentation of pressure on the issuing stream of gas. I hold in my hand a metal box containing compressed air. I turn the cock for a moment so as to allow a puff to escape: the flame instantly ducks down; not by any transfer of air from the box to the flame, for I stand at a distance which utterly excludes this idea; it is the *sound* of the issuing air that affects the flame. The hiss produced in one orifice precipitates the tumult at the other.*

Finally, I place this musical box on the table, and permit it to play. The flame responds like a sentient creature, curtseying to the notes to which it is sensitive.

[J. T.]

WEEKLY EVENING MEETING,

Friday, January 25, 1867.

Sir Henry Holland, Bart. M.D. D.C.L. F.R.S. President,
in the Chair.

William Odling, M.B. F.R.S.

On Mr. Graham's Recent Discoveries on the Diffusion of Gases.

I.

When atmospheric air is separated from a vacuous or partially vacuous space by a septum, partition, or bag of india-rubber, some air passes through the septum into the originally vacuous space.

This space may be conveniently maintained vacuous, and any air passing into it be simultaneously withdrawn and delivered for examination, by means of Sprengel's exhauster.

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* Those who wish to repeat these experiments would do well to bear in mind, as an essential condition of complete success, that a free way should be open for the transmission of the vibrations from the flame *backwards*, through the gaspipe which feeds it. The orifices of the stopcocks near the flame ought to be as wide as possible.

Whereas atmospheric air consists of about 21 per cent. of oxygen and 79 per cent. of nitrogen, the air transmitted through india-rubber into a vacuous space is found to contain about 40 per cent. of oxygen and 60 per cent. of nitrogen, and to have the property of re-inflaming a glowing splinter.

A transmission, therefore, takes place through the rubber septum of both constituents of the atmosphere, but there is a greater proportionate transmission of its oxygen than of its nitrogen.

Single or unmixed gases, similarly separated from a vacuous space by a septum of india-rubber, penetrate the rubber and enter the vacuous space with the following relative velocities:—

Nitrogen	1
Marsh-gas	2·15
Oxygen	2·55
Hydrogen	5·50
Carbonic acid	13·58

From these velocities the observed passage of mixed oxygen and nitrogen gases through india-rubber is deducible by calculation; and conversely, the separate velocities of oxygen and nitrogen are deducible from the transmission-results obtained with atmospheric air:—

Oxygen	21	×	2·55	=	53·55 . . .	40·4
Nitrogen	79	×	1	=	79·0 . . .	59·6
					132·55	100·0

The constituent gases of atmospheric air not only pass through an india-rubber septum into a vacuous space, but also into a space containing some other gas, such as hydrogen or carbonic acid, and at the relative velocities with which they enter a vacuous space; but the conditions of the experiment then become more complicated.

In the case of an india-rubber balloon filled with carbonic acid, for instance, not only are atmospheric oxygen and nitrogen gases continually entering the balloon, but carbonic acid gas is continually and very rapidly escaping from it.

Throughout the vacuum experiment, the conditions remain constant, the hyperoxygenized air being withdrawn as fast as transmitted; but in the balloon experiment, the oxygen is gradually accumulating within the balloon, whereby the conditions are constantly varying.

Eventually, by the rapid escape of carbonic acid, the proportion or pressure of oxygen in the internal mixture comes to exceed that in the external air; whereupon a reverse transmission through the balloon, of the excess of oxygen into the external air, at once begins.

II.

When ordinary coal gas is separated from a vacuous space by a septum, partition, or tube of platinum, some gas passes through the platinum septum into the originally vacuous space, as soon as, but not until, the metal is raised to the temperature of ignition.

Whereas coal-gas is a variable mixture of marsh-gas and hydrogen with several other gases and vapours, containing on the average about 45 per cent. of marsh-gas and 40 per cent. of hydrogen, the gas transmitted through ignited platinum is found to consist exclusively of hydrogen.

A transmission therefore of only one, and that not the most abundant of the many constituents of coal-gas, takes place into the originally vacuous space through a septum of ignited platinum.

So that while the nitrogen of the air is transmitted through a septum of india-rubber in a much smaller ratio than its oxygen, the other constituents of coal-gas are transmitted through a septum of ignited platinum in an infinitely smaller ratio than is its hydrogen.

Experimenting with single or unmixed gases, the quantity of hydrogen transmitted through a septum of ignited platinum into a vacuous space amounted to over 100 cubic centimetres in half-an-hour; whereas, under the same conditions, the quantity transmitted of oxygen, nitrogen, marsh-gas, carbonic acid, and some other gases, did not amount to ·01 cubic centimetre in half-an-hour.

Further, the transmission of hydrogen through a septum of ignited platinum, as of various gases through a septum of india-rubber, takes place into a volume of some other gas as well as into a vacuum, but with a similar complication of results.

What is the nature of these transmissions of gas through india-rubber and ignited platinum respectively? Are the phenomena in the two cases similar or dissimilar to each other; and with what class of actions are they one or both associated?

III.

By a sufficient degree of pressure, gases may be forced through the minute channels of a porous septum; or, in other words, may pass through such a septum by *transpiration.*

But transpiration takes place only through obvious channels or pores, from which india-rubber and platinum are entirely free.

Again, transpiration through a porous septum takes place only in the direction of the preponderating total pressure; but the transmission of gas through india-rubber and ignited platinum, from one gaseous space into another, can take place in the opposite direction to that of the total pressure, and in both directions at the same time, by a sort of interchange of gases through the septum.

Moreover, the composition of a mixed gas, such as atmospheric air or coal-gas, is not altered by mere transpiration; whereas the composition of these mixed gases is greatly altered by their transmissions through india-rubber and ignited platinum respectively.

Lastly, every gas and every mixture of gases has its own special velocity of transpiration, irrelative to any other property of the gas, and irreducible to any general law. These rates are altogether different from the observed rates of transmission of the same gases through india-rubber and ignited platinum, thus :—

Oxygen	1·00
Nitrogen	·87
Carbonic acid	·73
Marsh-gas	·55
Hydrogen	·44

From these differences in the character of the phenomena, as well as from another important difference hereafter to be mentioned, it is clear that the transmission of various gases through india-rubber, and of hydrogen through platinum, is not due to transpiration.

IV.

As the channels of a porous septum become more and more minute, their resistance to the bodily transmission of gas becomes greater and greater, and the quantity of gas forced through them less and less, until at length the septum becomes absolutely impermeable to transpiration, under the particular pressure.

But such a septum, of which the individual capillary channels are so small as to offer a greater resistance, or friction, to the passage of gas through them than the available pressure can overcome, may nevertheless present a considerable aggregate of interspace, through which the proper diffusive movement of gases, due to their innate molecular mobility, may take place freely.

When any volume of gas is allowed access to a vacuous space, or to an additional gaseous space, it gradually diffuses itself throughout the space afforded it, at a rate inversely proportionate for each gas to the square root of its specific gravity.

In so far as the aggregate area of interspace available for *diffusion* is greatly diminished, by the introduction of a porous non-transpiring septum between the diffusing gas and the additional space afforded it, so is the amount of diffusion within a given time proportionably diminished; but in no other respect does the septum appear to take any part in the action; it neither promotes nor retards the diffusion, but simply allows it to take place in proportion to the aggregate area of the interspace which it affords.

The experimental determination by means of Bunsen's diffusionometer of the relative diffusion-velocities of different gases through a thin plate of compressed graphite—a septum without obvious pores and quite impermeable to transpiration—has given numbers which are almost identical with the reciprocals of the square roots of the specific gravities of the several gases :—

Hydrogen	3·80
Marsh-gas	1·34
Nitrogen	1·01
Oxygen	·95
Carbonic acid	·81

Interdiffusion of different gases takes place in proportion to their respective diffusion-velocities. Thus with air and hydrogen separated from each other by a graphite septum, for every 1 volume of air which passes into the hydrogen-space, 3·8 volumes of hydrogen pass into the air-space.

Mixed gases also diffuse away from one another according to their respective diffusion-velocities. As a result of even the small superior diffusiveness of nitrogen over that of oxygen, the proportion of oxygen in atmospheric air has been increased from 21 to 24·5 per cent., by the diffusion of nitrogen away from it, during its conveyance through several lengths of porous tobacco-pipe enclosed in a vacuous space.

The acts of gas-diffusion through porous septa and of gas-transmission through india-rubber and ignited platinum resemble each other in several points. They both take place through septa free from obvious pores; they both take place as well in the direction as against the direction of the preponderating pressure, and also in opposite directions at the same time by a sort of interchange; and they both effect an alteration in the composition of any mixed gas subjected to their operations.

But they differ altogether from one another in the relative velocities with which the gas-movements in each case are effected—the transmissions of gás through india-rubber and ignited platinum being at special rates, while the diffusions of gas through porous septa are inversely as the square roots of the specific gravities of the particular gases.

Thus the specific gravity of nitrogen being somewhat less than that of oxygen, its rate of diffusion is accordingly somewhat higher, in the proportion of 101 to 95. Hence, if the passage of air through the rubber septum were due to diffusion, the transmitted air should be rather richer in nitrogen and poorer in oxygen than the original air; whereas the transmitted air is actually found to be very much richer in oxygen and poorer in nitrogen than the original air.

Again, hydrogen having a far lower specific gravity than marsh-gas, its diffusion rate is very much higher, in the ratio of 380 to 134. Hence, taking the proportion of marsh-gas to hydrogen in coal-gas, as 1 to 1, and it is usually rather greater, if the passage of coal-gas through ignited platinum were due to diffusion, for every 380 volumes of hydrogen transmitted there should be 134 volumes of marsh-gas: but in reality no marsh-gas whatever is transmitted; so that neither with the rubber septum nor with the platinum septum are the results due to diffusion.

It is rare to have phenomena of diffusion undisturbed by phenomena of transpiration, or phenomena of transpiration undisturbed by phenomena of diffusion; but since the alteration in the composition of a mixed gas by its passage through a transpiring-diffusing septum is effected solely by diffusion, the results obtained with the rubber and platinum septa are not due to joint transpiration-diffusion.

V.

A septum may be quite free from pores of any kind or degree of minuteness, and so far be absolutely impermeable to the passage of gas through it in the form of gas, but may nevertheless permit a considerable transmission of certain gases by their prior solution or *liquefaction* in the substance of the septum.

The merest film of water, as of a soap-bubble for instance, is quite impermeable to gas as gas; but allows the ready transmission of a soluble gas, such as ammonia, through it, by reason of a prior solution or liquefaction of the ammonia in the film of water.

The film of water may be replaced by a moist membrane of any degree of thinness or thickness, with a similar result.

In this case the phenomenon consists in a solution of the gas in the moist material of the septum—in a diffusion of the liquefied gas as a liquid through the thickness of the septum—in an evaporation of the liquefied gas from the remote surface of the septum—and lastly, in a diffusion of the evaporated gas into the adjoining space.

Of the many circumstances affecting the final result, the influence of the solubility of the gas in the liquid of the septum would so far predominate over all other influences as to allow of their being left out of consideration. Whence it may be affirmed that the transmission of any gas through a film of liquid, or a moist septum, will be found proportionate to the solubility of the gas in the liquid.

But gases are absorbable not only by liquids, but also by certain solids, and especially by charcoal.

The gases absorbed by charcoal are probably liquefied in the charcoal; at any rate, the more absorbable of them occupy a bulk considerably less than if reduced to the liquid state by pressure.

All charcoal is more or less porous; but its absorption of gases is not proportionate to, or a mere physical effect of, its porosity; since other similarly porous substances do not manifest the same absorptive power; and since the absorbability by charcoal of any gas is as special a property of that gas as is its solubility in water, or alcohol.

The transmission of an absorbable gas through a septum of compact charcoal, such as the cocoa-nut charcoal used by Mr. Hunter of Belfast, which absorbs about $\frac{8}{10}$ of its volume of mercury, and 111 times its volume of ammonia, would take place in two ways.

A portion of the gas would pass through the fine pores of the charcoal as gas, by diffusion, at a rate inversely proportionate to the square root of its specific gravity; while another portion would become liquefied in the charcoal by capillary condensation, pass through the charcoal as a liquid, and evaporate from the other side, just as would a gas liquefied by solution in a moist membrane; and it is conceivable that, in some compact forms of charcoal, the transmission of gas by gaseous diffusion might be inappreciable as compared with its transmission by liquefaction and evaporation.

VI.

Whereas the mere passage of gas through a transpiring or diffusing septum takes place in thorough independence of the nature of the material of the septum, in these last considered actions the transmission takes place by virtue of a sort of chemical affinity between the gas and the material of the septum—the selective absorption of the gas by the septum being a necessary antecedent of its transmission; whence it may be said that the gas is transmitted because it is first absorbed.

Is, then, the transmission of oxygen, &c., through india-rubber, and of hydrogen through ignited platinum, effected by a process, at all allied to that of gas-liquefaction by solution or capillary condensation?

That septa of india-rubber and platinum differ from merely diffusive and transpiring septa, in effecting or allowing a selective transmission of certain gases through them, is evident; but do they first exert a selective or, in other words, a *chemical absorption* of these particular gases?

Experiment answers that they do; oxygen proving to be more than twice as absorbable by india-rubber as by water, and hydrogen proving to be fully three times as absorbable by wrought-platinum as by charcoal.

The statements of fact and interpretation contained in this abstract are based upon the investigations of Mr. Graham, spread over a long period of years; and especially upon the investigations described in his more recent memoirs 'On the Molecular Mobility of Gases' (Phil. Trans., 1863), and 'On the Absorption and Dialytic Separation of Gases by Colloid Septa' (Phil. Trans., 1866).

[W. O.]

WEEKLY EVENING MEETING,

Friday, February 1, 1867.

Sir HENRY HOLLAND, Bart. M.D. D.C.L. F.R.S. President, in the chair.

J. SCOTT RUSSELL, Esq. F.R.S.

On the Crystal Palace Fire.

MR. RUSSELL commenced by stating that he appeared as a substitute for Professor Max Müller, and that he had much pleasure in undertaking the task which had been imposed upon him. As one of the first of those officially connected with the Exhibition of All

Nations, in Hyde Park, he, and the other promoters of it, regarded the Crystal Palace as an institution;—as one of a series of efforts which had long been in progress for the purpose of advancing the education of the English people, and more especially the large mass of people concentrated in the metropolis; as an institution for educating the multitude by their eyes;—for cultivating their tastes by means of the best works of the greatest men of all times, and for substituting pure and refined pleasures for the coarse and somewhat vulgar recreations which too often filled up the leisure hours of crowded masses of men. Regarding the Crystal Palace in that light, his audience, like himself, would take a deep and painful interest in the catastrophe which had overtaken one of the most interesting portions of the building.

He proposed to divide his remarks into three divisions: first, to consider the structural and mechanical character of the building, its stability and power of resistance to mechanical force and to fire; secondly, to consider the fire itself; and thirdly, to consider what was best to be done for its restoration.

With regard to the building itself, it might be superfluous to say much. They knew its splendid site on Sydenham hill, and the general beauty of its aspect, but there were peculiarities in its structure not generally understood, which affected the relations of the fire to the palace. They would remember that the Crystal Palace was the infant of the original building in Hyde Park,—that fairy palace more wonderful than their dreams had been,—which first burst upon their view on the memorable 1st of May, 1851,—that building which itself was said to be the most beautiful thing in the Exhibition. That 1st of May could never be forgotten, when All Nations first met together in friendly rivalry. One whose name they would all remember with respect and gratitude, the late Chevalier Bunsen, on that day and in that palace, threw his arms around the neck of one of his countrymen, and exclaimed,—alas! too prematurely, "That happy period we have all longed for of universal peace has at last arrived." For the purposes of the present discourse, it was necessary to describe how that beautiful building was made strong, durable, and relatively fireproof. It was the invention of Sir Joseph Paxton, whose experiments at Chatsworth induced him to submit to the Royal Commissioners the plan of this great and beautiful glasshouse. It had happened to be his, Mr. Russell's, duty to take Sir Joseph Paxton's designs to His Royal Highness the late Prince Consort, and the result was that the Commissioners were induced to forego their accepted brick and mortar plan, and to adopt that of Sir Joseph Paxton. That building, however (drawings of which were before the meeting), could hardly be recognized as the present Crystal Palace, so much had its exterior been changed by progressive improvements in the design. The design, as at first submitted, was of the simplest form; the roof being flat from end to end. When the drawings were submitted to the Building Committee by Messrs. Fox and Henderson, who had worked out Sir Joseph

Paxton's design, they were required to add a transept to be in conformity with the plan of the Building Committee, which had a transept.

There were a number of trees on the site of the building, and it had been intended to cut openings in the roof for the trees to grow through, and the next suggestion was that the proposed transept should be placed over the trees; then it was found that the trees were taller than the transept. In this fix, Mr. Henderson said, "Happy thought. Take the circular roof of the Great Conservatory at Chatsworth, and put it upon the transept instead of a flat roof." "Happy thought," indeed! A happier thought still was that of Sir Charles Barry, to continue the arched roof along the whole length of the building, and thus to complete an entirely new style of edifice in glass and iron. Time, however, was wanting, and the first building went without this great crowning feature. The elevation of the building derived great advantage from the suggestion of Sir Charles Barry, who suggested the horizontal panels or string courses with circular openings in each, above and between the ranges of windows. Mr. Scott Russell illustrated this portion of his discourse by some interesting models and drawings.

He next proceeded to explain, with the assistance of drawings and diagrams, the peculiar arrangements which constituted the strength and stability of a building so apparently fragile, and to render it a marvellous combination of lightness with elegance and strength. Supported by columns only eight inches in diameter, the strength of the structure consisted in the two qualities of accumulation and rigidity. Arranged in sets of four, these columns, which might in that position be compared to the legs of a table, were firmly braced and united together by cross-girders of triangular pattern, which acted as the top of the table to its legs, and hence the quality of rigidity and strength. Each of these sets of four columns and girders was firmly connected with a second set, so that it was impossible to upset one of these tables without upsetting the second; the second was connected with a third, and so on; and thus, by the principle of accumulation, the whole building was able to resist any pressure that might come against the front of it. Hence, the essential parts of the Crystal Palace had never suffered, as it had been predicted it would, from any of the violent storms of wind to which it had been exposed; and the merit of this admirable arrangement of mechanical powers rested between Sir Charles Fox and Mr. Henderson. The beauty of detail and colour which gave grace and homogeneity to the interior, was due to Mr. Owen Jones. Not the least ingenious portion of the design was the construction of the roof by the repetition of ridge and furrow ribs, in sections of 24 feet long by 8 feet wide; and nothing but this simplicity could have made it possible for all these parts to be constructed with the greatest accuracy, in different parts of the country simultaneously, and the whole building put together in such an incredibly short space of time.

How then could such a building as Mr. Russell had described be burnt? In the first place, such a building would not burn of itself; secondly, if you set it on fire it would not keep alight; thirdly, he thought it was pretty clear that the late fire at the Crystal Palace went out by itself, though some of them took a great deal of credit to themselves for trying to put it out; and fourthly, it must not be forgotten that any building could be burnt by putting sufficient combustible matter in it. It was to be feared that there was enough combustible matter in that portion of the Palace which had been destroyed to burn fiercely, and do great injury to the building, but he was as satisfied as ever that the building was one of the safest that could be conceived to resist fire. With regard to the late fire, it was very difficult to ascertain the actual facts, nor did he know that he could distinctly recollect, or could put into language which might not be misunderstood, that which he had actually seen of it.

The fire, it would be remembered, occurred at the north end of the palace, in the beautiful Tropical Department where they used to sit and dream of bygone ages and distant climates, very unlike their own. There stood the wonderful court illustrative of that ancient Nineveh which Layard did so much to discover and Ferguson to restore. There also was the exquisite Alhambra Court, which, as it said itself in one of its inscriptions, taught all men who would gaze carefully at it what were the deep and hidden and mysterious principles of beauty, and how that beauty could be created with certainty, and worked out as if with magical art, but after all with that true science which was the most wonderful of all art;—that court which taught the principles by which not only could Alhambras be made beautiful, but by which all other works of art could be made beautiful to the eye, comely to look at, ravishing to the senses. That court was the realization of the ideal dreams of Mr. Owen Jones, whose first love was the Alhambra, who had left his own country and exiled himself there to worship it, who brought home models and drawings of it, but was unable to display its beauties to the British public till the Crystal Palace afforded him the opportunity of reproducing it. He (Mr. Scott Russell) remembered how Mr. Owen Jones revelled in the thought of doing so, when the proposition was laid before him. Then there was the beautiful Byzantine court of Mr. Digby Wyatt, who at this point stood between the architecture of the ancients and that of the moderns, and represented that extraordinary jumble of the oriental, the antique, and the renaissance (in the proper, not the architectural sense), between the old world and the new, to show what a remarkable combination could be formed by men who were acquainted with different styles of architecture, but had no time to make one of their own. That these admirable works of art should in one brief afternoon vanish from their vision was indeed a sad calamity, for the sake of their producers, and for the public.*

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* It has been said that these courts ought not to be reconstructed, as the state of knowledge and views on which they were originally made, have materially changed; but after consulting the authors, I find that no ground exists for such an assertion.—J. S. R.

At two o'clock on the last Sunday of 1866 there was no fire. At five, or ten, or twenty minutes past two (overwhelming evidence was to be had to each of these as the precise time of its commencement) —there was fire in the Crystal Palace. Where did it come from? Ten minutes after two one of the firemen saw a singular column of what he called fire, in the building. It was in fact gas, and consisted of the products of combustible materials, heated into a gaseous state. He immediately applied water by a hose, both to this and the opposite side of the transept, but in vain. A cloud of smoke and a volume of flame rose to and spread under the floor of the gallery, which was immediately in flames, around both the end and side of the Tropical Department. In the transept there stood the Mammoth tree, the Wellingtonia Gigantea, 20 feet in diameter and 100 feet high, consisting not of wood, but of dry bark, ready to light a fire. The moment the gases spread from the wooden floor of the palace to the Mammoth tree, that became a huge chimney, an immense tunnel full of inflamed gas. On reaching the building, at twenty minutes to three, he (Mr. Scott Russell) found the whole of this transept in flames; and within ten minutes he saw that all chance of saving it was gone. He had been burnt out three times. He had never yet got the money he had insured for, and the Crystal Palace was unfortunately in the same predicament; for under what was termed the "average clause," in the policy, although insured for 80,000*l.*, the company would probably only recover one fourth or one fifth of that amount.* This clause was considered fair by the Insurance Companies, and therefore there must be some justice in it; but for his own part he could not see it. Seeing the transept lost, he, together with many brave and zealous volunteers, set himself to work to draw a line, across which the flames should not pass; and, by applying water freely to the part beyond this mathematical line, the flames were stopped. This was indeed the whole theory of stopping a fire. It was useless to waste water on anything which fire had obtained firm possession of. As to the commencement of the fire, the probability was that a boiler in the basement became overheated; it had not, he understood, blown up, but he believed that, it being Sunday, the stoker wanted to go to church, and put as much coal on the furnace fire as he thought would keep the boiler hot till he returned; but in doing so he converted a moderate fire into a splendid gas manufactory.

This gas escaped, and, undoubtedly, ascended in formidable quantities to the building, and became ignited, being indeed in an excellent condition for combustion. This was the origin of the fire, and it was a thing to be guarded against in future. As soon as the combustible matter piled together in this portion of the building was consumed, the fire died out; and, although it was quite possible for the palace to have been altogether destroyed if there had been less help from the parish engines and those of the Fire Brigade, he was convinced that with certain precautions the Crystal Palace, *quâ* the building itself, might be regarded as fireproof; but not if filled with highly combustible matter.

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* I am happy to acknowledge that, since the date of this discourse, 38,500*l.* has been paid by the Insurance Companies to the Crystal Palace Company.

Mr. Scott Russell then proceeded to consider the question of reconstruction.

In 1851 the building in Hyde Park was about to be swept off the surface of the earth. At that crisis there came to its aid some of the old and tried friends of the Exhibition of All Nations. Amongst them was a gentleman whose efforts were at the present time being energetically used to restore, if possible, the Crystal Palace to its original beauty and its original usefulness. Mr. Francis Fuller was one of the three original promoters of the original Crystal Palace—the Exhibition of 1851—one of those three who, under its great founder, the late Prince Consort, first conceived the idea that it would be possible to erect the Crystal Palace in Hyde Park; and indeed it was by his enthusiastic exertions that the necessary funds were originally obtained to start the project, and bring the first Crystal Palace into existence. The merit of the first institution of Exhibitions of All Nations was due to His Royal Highness Prince Albert, and although princes were often deprived of the credit due to them by the claims set up by others, he (Mr. Scott Russell), who knew all the facts, was in possession of time, date, and circumstance, in the Prince Consort's own handwriting, to prove that to him alone was due the enunciation of this great idea. Under him Mr. Fuller was one of the first who founded and made possible the Crystal Palace. To Mr. Fuller also the public were mainly indebted for its reconstruction on Sydenham hill. The first idea of purchasing the building in Hyde Park, and re-erecting it at Sydenham, originated between Mr. Leech and his partner, the late Mr. Farquhar, of the firm of Johnston, Farquhar, and Leech. Next to them it was imparted to Mr. Fuller, and next to himself; and they, with one or two other gentlemen, purchased the building accordingly. At Sydenham the arched roof throughout the length of the palace was introduced. Many other improvements were made upon the original design, and the whole presented a great combination of the united talents of Sir Joseph Paxton, Sir Charles Barry, Sir Charles Fox, Mr. Henderson, Mr. Owen Jones, and Mr. Digby Wyatt. Surely such a building ought not to be allowed to go down. If the company were not rich enough to do it,—if the Insurance companies would not pay the full insurance, the public ought somehow to help them. In other countries, a great educational institution like this would be helped by the government. In England, in such a matter as this, the people were the government. Let them therefore help both the directors and themselves to restore and maintain so grand a temple of art, education, and refinement: for to do so was a matter affecting the national reputation. "If," said the speaker, "you will do what I have done to-night,—say how fond you are of the Crystal Palace; tell everybody how much good you think of it; explain to them what a wonderful building it is, and how admirably it is constructed to stand and resist fire; and say that we shall have our Crystal Palace restored again, with all its glorious features unimpaired; if you will do that, I shall be almost glad that Professor Max Müller did not give his discourse."

[J. S. R.]

GENERAL MONTHLY MEETING,

Monday, Feb. 4, 1867.

The Earl Stanhope, D.C.L. F.R.S. Pres. Soc. Antiq. Vice-President, in the Chair.

John Blacker, Esq.
Adam F. Blandy, Esq.
John Clarke, M.D.
William David, Esq.
Robert Ward Jackson, Esq.
Edward Smith, M.D. F.R.S.
Captain the Hon. John Robert Vesey.

were *elected* Members of the Royal Institution.

John Augustus Seymour Morse Davies, Esq.

was *admitted* a Member of the Royal Institution.

The special thanks of the Members were returned for the following contributions to "the Donation Fund for the Promotion of Experimental Researches."

John P. Gassiot, Esq. F.R.S. (4th Annual Donation)	£20	0	0
Samuel Scott, Esq.	5	5	0
William Dell. Esq.	5	5	0
Edward H. Moscrop, Esq.	25	0	0
Alfred Davis, Esq.	21	0	0

The special thanks of the Members were presented to Edmund Pepys, Esq. M.R.I. for his present of the Portrait of his Father, the late John Pepys, Esq. for sixty-six years a Member of the Royal Institution.

The following Arrangements for the Lectures after Easter were announced:—

Professor Blackie.—Two Lectures, 'On Plato.' On Tuesdays, April 30th and May 7th.

Professor W. A. Miller, Treas. R.A.—Four Lectures, 'On Spectrum Analysis, including its application to Astronomy.' On Tuesdays, May 14th to June 4th.

Professor Huxley, F.R.S. — Twelve Lectures, 'On Ethnology.' On Thursdays and Saturdays, May 2nd to June 8th.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

Secretary of War (through Sir Henry James, F.R.S.)—Comparisons of the Standards of Length of England, France, &c., made at the Ordnance Survey Office, Southampton. 4to. 1866.

Government of New South Wales—R. P. Whitworth: New South Wales Gazetteer. 8vo. Sydney, 1866.

Actuaries, Institute of—Journal, No. 66. 8vo. 1867.

Architects, Royal Institute of British—Proceedings, 1867. Part I. Nos. 1, 2, 3, 4. 4to.

Asiatic Society, Royal—Journal. New Series. Vol. II. Part 2. 8vo. 1866.

Asiatic Society of Bengal—Journal, Nos. 133, 135. 8vo. 1866.

Astronomical Society, Royal—Monthly Notices, Vol. XXVII. Nos. 1, 2. 8vo. 1866–7.

Barlow, Rev. John, M.A. F.R.S. M.R.I.—The Alps of Hannibal. By Wm. John Law. 2 vols. 8vo. 1866.

Bavarian Academy of Science, Royal—Sitzungsberichte, 1866. Band I. 4. Band II. 1. 8vo.

Annalen der Münchener Sternwarte. V. Supplementband. 8vo. 1866.

Reden von Justus von Liebig und H. Bauernfeind. 4to. 1866.

Bodde, D. Esq. (the Author)—Essay on the Use of Petroleum. (L 14) 8vo. 1866.

Bremen Naturwissenschaftliche Vereins—Abhandlungen, Band I. Heft. 1. 8vo. 1866.

Chemical Society—Journal for Dec. 1866, and Jan. 1867. 8vo.

Cialdi, Alessandro (the Author), through the Royal Society—Sul Moto Ondoso del Mare e su le Correnti di esso specialmente su quelle Littorali. 8vo. Roma, 1866.

Les Ports-Canaux, &c. 8vo. 1866.

Devonshire Association for Advancement of Science, &c.—Report and Transactions. Part 5. 8vo. 1866.

Dickson, E. W. D.C.L. F.R.S.—Reports of the International Sanitary Conference at Constantinople. 4to. 1866.

Dublin Society, Royal—Journal, No. 35. 8vo. 1866.

Editors—American Journal of Science, November, 1866. 8vo.

Artizan for Dec. 1866, Jan. 1867. 4to.

Athenæum for Dec. 1866, Jan. 1867. 4to.

British Journal of Photography for Dec. 1866, Jan. 1867. 4to.

Chemical News for Dec. 1866, Jan. 1867. 4to.

Engineer for Dec. 1866, Jan. 1867. fol.

Horological Journal for Dec. 1866, Jan. 1867. 8vo.

Journal of Gas-Lighting for Dec. 1866, Jan. 1867. 4to.

Mechanics' Magazine for Dec. 1866, Jan. 1867. 8vo.

Pharmaceutical Journal for Dec. 1866, Jan. 1867.

Enderby, Charles, Esq. F.R.S. M.R.I.—Thos. Beale: Natural History of the Whale. 16to. 1839.

Faraday, Professor, D.C.L. F.R.S.—Kais. Akademie der Wissenschaften, Wien. Math. Nat. Classe:—Denkschriften, Band XXV. 4to. 1866.

Sitzungsberichte. Abth. I. 1865. Nos. 8, 9, 10. 1866. Nos. 1–6. Abth. II. 1865. Nos. 9, 10. 1866. Nos. 1-5. 8vo.

Almanach, 1866. 16to.

Franklin Institute—Journal, Nos. 491, 492. 8vo. 1866.
Genève, Société de Physique, &c.—Mémoires, Tome XVIII. 2e Partie. 4to. 1866.
Geographical Society, Royal—Proceedings, Vol. X. No. 6. 8vo. 1866.
Geological Institute, Royal, Vienna—Jahrbuch, Band XVI. Nos. 2, 3. 4to. 1866.
Glasgow Philosophical Society—Proceedings, Vol. IV. No. 2. Vol. VI. Nos. 1, 2. 8vo. 1860–66.
Haynes, Stanley L. M.D. (the Author)—Ramble in New Zealand Bush. 12mo. 1866.
Horticultural Society, Royal—Proceedings, Vol. I. No. 6. 8vo.
Journal, Vol. I. No. 4. 1867. 8vo.
Ladd, Mr. Wm. (the Publisher)—Results of Spectrum Analysis applied to the Heavenly Bodies. By W. Huggins. 16to. 1866.
Linnean Society—Journal and Proceedings: Zoology, No. 35. 8vo. 1867.
Lubbock, Sir John, Bart. F.R.S. M.R.I. (the Author)—Development of the Chloëon (Ephemera) dimidiatum. Part II. (Linn. Soc. Trans. XXV.)
Mechanical Engineers' Institution, Birmingham—Proceedings, August, 1866. Part 1. 8vo.
Photographic Society—Journal, No. 176, 177. 8vo. 1866.
Royal Society of Edinburgh—Transactions, Vol. XXIV. Part 2. 4to. 1866.
Proceedings, No. 68–70. 8vo. 1865–66.
Royal Society of London—Proceedings, Nos. 87, 88. 8vo. 1866.
Saxon Society of Sciences—Abhandlungen, Band V. Two Parts. 8vo. 1866.
Berichte. Three parts. 1865-6. 8vo.
Scharf, George, Esq. F.S.A. (the Author)—Remarks on a Portrait of the Duchess of Milan. (Archæologia, Vol. XL.)
Scottish Society of Arts, Royal—Transactions. Vol. VII. Part 2. 8vo. 1866.
Statistical Society of London—Journal, Vol. XXIX. Part 4. 8vo. 1866.
United Service Institution, Royal—Journal, No. 40. 8vo. 1866.
Vereins zur Beförderung des Gewerbsfleisses in Preussen—Verhandlungen, Juli und August, 1866. 4to.

Mayall, John J. E. Esq.—Portrait of Professor Faraday.
Pepys, Edmund, Esq.—Portrait of John Pepys, Esq. by Lonsdale.

WEEKLY EVENING MEETING,

Friday, February 8, 1867.

Sir Henry Holland, Bart. M.D. D.C.L. F.R.S. President, in the Chair.

The Rev. Frederic W. Farrar, M.A. F.R.S.

On some Defects in Public School Education.

When I had the honour of being invited to deliver a discourse before the Royal Institution, my subject was at the same time assigned to me. Yet even if this had not been the case, it would perhaps have been unnecessary to apologise for speaking to you on a topic of great importance, of which I know so much from personal experience. Although, therefore, my discourse must be of an interest immeasurably inferior to those fairy tales of science, illustrated by delicate and wonderful experiments, to which you are accustomed in this place, it will, at least, be so far scientific in its spirit, that it will deal with no facts which have not been derived from first-hand observation.

Thirteen full years of labour spent in the heart of Public Schools, and devoted, to the utmost of my poor ability, to their service, are the credentials which I offer to save me from the charge of presumption if I deal with their shortcomings. My position differs widely—nay, absolutely—from that of a rude, uncompromising, and unsympathetic assailant. The fact that I have myself toiled for years at a task which, in many instances, has filled me with the misgiving that it would be as barren of all obvious results as if I had ploughed the sand of the sea-shore and sown salt in its furrows, will show, I trust, that I am no arrogant critic with an eye blind to every merit, but keen as that of an eagle to every fault. Honouring the body of public schoolmasters with a sincere honour,—believing that, though their usefulness is often impaired by the trammels of an unprofitable routine, there may be found among them men of conscience the most enlightened, of intellect at once solid and brilliant, of indomitable energy and noble purpose,—I can hardly be suspected of desiring to cast a slur on a profession, to which, however lightly it may be estimated by the outer world, it is my pride and pleasure to belong. If, then, a hand so feeble as mine can inflict the slightest wound on our present system, I ask to be believed when I say that it is the faithful wound of a friend, and that my spear, like the spear of Achilles, is meant to heal as well as smite. If I criticize Public Schools fearlessly, it is because I love them deeply, and because I would not willingly see them fall into that gradual neglect and disesteem, which *must* be the consequence of a refusal on their part to progress with a progressive age, and to widen the narrow horizon of their studies with the widening of an epoch, whose researches have thrown light on every region of nature, from its minutest organism to its most distant nebulæ and stars.

Not, then, from any wish to conciliate favour for my future remarks, still less because I shrink from the full brunt of any anger they may bring upon me, let me say as the hearty tribute of my genuine admiration, that judged by the nobility and serviceableness of the manhood they have trained, the Public Schools have no cause to blush; and that though they may have fallen far short of that splendid ideal which is entertained for them in the aspirations of those who love them best, they may yet put forth an irrefragable claim to the respect and honour of their bitterest enemies:—

> "Great men have been among them,—hands that penned,
> And tongues that uttered wisdom."

And although they have no right to claim the entire credit of such names, because the greatest men are often the accidents rather than the results of a system, and they have often been great in *spite* of it rather than *because* of it, yet, when a school (like that which I have at this moment the honour to serve) may boast to have produced in one half-century among its five Prime Ministers, a Palmerston, a Peel, a Spencer Percival, and an Aberdeen; and among its statesmen, a Dalhousie and a Sidney Herbert; and among its soldiers and sailors, a Rodney and a Codrington; and among its poets, a Byron and a Proctor; and among its scholars, a Parr and a Sir William Jones; and among its divines, a Trench and a Manning; and among its common crowd of unknown *alumni*, so vast a multitude of honourable and useful men;—such a school (and it is but an illustrious type of many more) has no cause to fear that a system which, in other days, has borne "such fruit in all its branches," will be *hastily* or *indiscriminately* condemned.

1. To proceed, then, to my review of our present system in its workings and results, I would state my belief that the *social* education offered by our schools is one of immense value. I am not now alluding to the despicable advantage of making fashionable acquaintances, but to something much deeper and more indisputable. It is, I think, a distinct benefit to the growth and development of English society, that boys of families and of professions the most widely diverse, should be thrown together at our Public Schools. It is a benefit, I think, to the harmony and to the breadth of our statesmanship, that the poor curate working obscurely in his remote country parish, and the brilliant journalist who wields the force of public opinion, and the college don in his quiet quadrangle, and the Manchester man building up his vast fortune in the counting-house, and the eloquent Radical member for some city or borough, and the silent millionaire nobleman who flings the whole weight of his influence on the side of Conservatism, should still have this friendly bond of sympathy between them, that at school they each found their own level or realized their own worth, and that one was the other's fag, and that they joined in shouting for the victory of their common house in the green and sunny cricket-field, and passed under the influence of the same associations and interests, and that, however widely they may now be separated, they loved and esteemed each other in those days as equals and friends. In broadening the views, in knitting together the sympathies of brother Englishmen, even amid such din of controversies and strife of parties as may be raging at this moment, or will soon be raging, within the walls of St. Stephen's, such ameliorating influences are not, I think, to be despised.

2. And, again, as far as regards *moral* education, I give my most sincere and honest opinion when I say we have good reason to be thankful. Be it remembered that we have to deal with a difficult and impetuous period of life; with that perilous age—

> "When young Dionysus seems
> All joyous as he burst upon the East
> A jocund and a welcome conqueror;
> And Aphrodite sweet as from the sea
> She rose, and floated in her pearly shell
> A laughing girl."

Let any one consider how difficult must be the task of governing that wilful age, and then let him visit some great school, and note the admirable discipline, the cordial relations between master and scholar, the manly bearing, free at once from presumptuous forwardness and servile timidity; and then, further, the reverent attention to the religious services, and the number of hushed and youthful partakers of the Lord's Supper,—and few, I think, will refuse to admit that, by God's blessing, we have in large measure ennobled and purified the once unhealthy moral atmosphere of our Public Schools. Some, indeed, there are, and will ever be, who, in spite of the many kindly and solemn warnings they receive, learn there but few lessons save those of sin and sorrow; but the majority, by their high tone of honour and principle, and by that deeply encouraging growth of Christian character which marks their progress from form to form, show that the most high-spirited boys may be guided by a thread, when they are guided by gentlemen and by Christians, as well as by scholars, and when the sincere and simple spirit of religion is made to bear—not in the shape of dead dogmas, but in the shape of living principles—upon the whole tenour of their lives.

3. Nor—once more—if we turn to the *physical* training of our boys, shall we find any errors—at any rate on the side of omission. If the end and aim of physical training be health, vigour, and activity, few will think it neglected when they see the healthy colour, and high spirits, and well-knit frames, which among our Public School boys are not the exception, but the rule. The only question could be whether in this direction we have not gone too far. I would leave *ample* margin for our hours of play; I look with the heartiest sympathy on the flourishing of our manly sports; I know the value and beauty of a keen glance, a strong arm, and swift feet, and I rejoice to see the racquet-court, the cricket-field, and the rifle range thronged with emulous competitors; but I must not hesitate to say, that in some instances we have pushed our admiration for these things to extravagant and disastrous lengths. When we commonly see boys ready to sacrifice everything to cricket; when we see them devoting to it a number of hours and an amount of enthusiasm, out of all proportion to that expended on their work; when we find their thoughts so thoroughly coloured and moulded by it that they talk cricket, think cricket, and dream cricket, morning, noon, and night; it is hardly surprising to find many who complain that this mania of muscularity has its share in the hunger-bitten poverty of our intellectual results, and tends to account for the fact that the value of the work we get out of the mass of boys is, as Mr. Gladstone puts it, "scandalously small." Let us by all means make our boys good animals if we can; let us train them to patient endurance and hardy strength; let us teach them (if we *can* teach them) to scorn the ever-growing tendency to luxury and extravagance; but let us at the same time impress on them, that to be good animals is a contemptible result if it does not conduce to their being better, more thoughtful, and nobler men. It is hardly satisfactory that

the child of nineteen Christian centuries, "the heir of all the ages in the foremost files of time," should spend *all* his energies, and *all* his admiration on the attainment of those corporeal attributes in which, let him do his best, the brute and the savage will beat him still.

4. I now turn to that part of my task which is at once the most difficult and the most immediately important—I mean the *Intellectual* Education of our Public Schools. And here I shall doubtless cut against the grain of a hundred prejudices, and draw upon my head a storm of opposition. Be it so, if I thereby hasten the victorious purpose which so many have at heart. About my own insignificance the storm may roar as loudly as it will, if thereby it "lash into motion those lazy clouds" that have stagnated so long on our educational horizon. Nor will I deprecate it further than by saying that no one has a right to resent the straightforward avowal of a conviction derived from long experience; and that, as I speak without arrogance and without censoriousness, I claim the privilege, as indeed I have earned the right, to speak also with perfect plainness.

I must, then, avow my own deliberate opinion,—arrived at in the teeth of the strongest possible bias and prejudice in the opposite direction,—arrived at with the fullest possible knowledge of every single argument which may be urged on the other side,—I must avow my distinct conviction that our present system of exclusively classical education as a whole, and carried out as we do carry it out, is a *deplorable failure.* I say it, knowing that the words are strong words, but not without having considered them well; and I say it, because that system has been "weighed in the balances and found wanting." It is no epigram, but a simple fact to say, that Classical Education neglects all the powers of some minds, and some of the powers of all minds. In the case of the few it has a value, which being partial, is unsatisfactory; in the case of the vast multitude, it ends in utter and irremediable waste. On the theory of the convertibility of force, something, I suppose, must come of the energies expended on our ordinary teaching; but at present a large portion of them seems to me as entirely wasted, as the sunbeams which waste their vivifying influence in scorching the desert sands. "We pour this kind of knowledge," says Mr. Ruskin, "on one and all alike, like snow upon the Alps, and are proud if here and there a river descends from their crests into the valleys, forgetting that we have made the loaded hills themselves *barren for ever.*"

The proofs of the fact are now but too patent in the faithful report of eminent and most friendly commissioners; for after diligent, anxious, and repeated study of the four thick blue volumes in which their laborious investigations lie buried from the public ken, I can draw from them no other conclusion than that which may be summed up in these few words: That but a small proportion of our boys, say twenty-five per cent., go to the Universities; that yet the entire curriculum of our Public Schools is framed with a view to the Universities; and that even of this poor twenty-five per cent., who are as it were the very flower and fruit of the system, and if I may so phrase it, its *raison d'être*, a *considerable* number (many would be inclined to say the *larger* number) leave school at the age of eighteen or nineteen, not only ignorant of history, both ancient and modern, ignorant of geography and chronology, ignorant of every single modern language, ignorant of their own language and often of its mere spelling, ignorant of every single science, ignorant of the merest elements of geometry and mathematics, ignorant of music, ignorant of drawing, profoundly ignorant of that Greek and Latin to which the long ineffectual years of their aimless teaching have been professedly devoted; and we may add, besides all this, and perhaps worst of all, completely ignorant of—altogether content with—their own astonishing and consummate ignorance. Or, in other words—for here I am but translating into a little plainer language the courteous euphemisms of the commissioners—we have *this* fact:—During ten or twelve, or even more, of the best, the most vigorous, the most plastic, and the brightest years of life, a multitude of boys have been *mainly*, at some schools almost *exclusively*, occupied with Greek and Latin, who yet at the end of those years not only know nothing else, and not only are wholly careless to learn anything else, but have profited so little even in their Greek and Latin that they can neither write a single correct sentence in either language, nor stumble correctly through a single page of their simplest authors without special previous preparation. On such a topic it would be useless to amplify; there, whether we like it or not, is the plain, naked, unvarnished fact. If it startle us, I can only say it *ought* to startle us; if it is painful, I say that it *ought* to be painful to any mind on which custom is not lying with a weight "heavy as frost, and deep almost as death." If any one be prepared to question it, the Commissioners will supply him with ample proofs in both hands; and any public schoolmaster could quadruple those proofs, if his eyes are open, out of the experiences of a single year. The Commissioners quote tutor after tutor of the Oxford Colleges, and tutor after tutor of the Cambridge Colleges, who come forward with dreary iteration to say that the men are mostly men of excellent principles and manners, but to numbers of whom they freely apply the epithets "indolent," "unawakened," "inaccurate," "men of idle habits, and empty uncultivated minds." And this, be it remembered, is the verdict on boys who go to the University, the *only* body of whom the Commissioners had even an opportunity of forming a judgment; and it suggests these two reflections:—If these be the results in the case of boys for whom the system was specially framed, what are we to think of the rest? And if these boys know nothing of Greek and Latin in which for years they have been assiduously taught, how unfathomable may be their ignorance in subjects which they have never been taught at all, or (as is the case with many noble branches of knowledge) taught by an ignorant tradition to neglect and to despise?

Facts like these may have been unknown to all but professional teachers at schools and universities until the Commission revealed them, and until the whole press of the United Kingdom, whether friendly or unfriendly, whether religious, political, educational, or scientific, had with appalling unanimity summed up the general result of the Report of the Commissioners in the one ugly word, FAILURE. There was no escape from the plain conclusion; yet the apathy with which it was accepted strikes one with amazement. We know what the years of boyhood are—how keen, how inquiring, how full of life: we know what education can do; how it can stimulate exertion and store up knowledge, and give extraordinary energy to every faculty and every sense. Is it then a matter of no consequence that the intellectual powers of so many fine and noble English boys should be suffered to run to seed? Is it the will of England that her sons should grow up good oars, and good cricketers, and profoundly ignorant men? While science commands its thousands of eager, devoted, enthusiastic workers, will England remain content that the main effort of her education should end so often in an atrophy of intelligence and knowledge? Is education a mere trifling experiment made *in corpore vili?* Is the mighty development, the magnificent heritage of this and many centuries to be left with an influence either *nil* or insignificant in the teaching of our boys? If people believe in a *classical* education, do they believe in one which may be *nominally* classical, but which ends with such extreme frequency in a gigantic negation? Will they listen to idle and flourishing rhetoric about the graceful and godlike literature of Greece and Rome, and then deceive themselves with the illusion that *this* is the reward of lads who, after an indefinite term of years, could not speak two Latin sentences, or construe Xenophon without a crib? Are we, in the nineteenth century, to learn no more and to teach no more—nay, to attempt and to achieve actually less—than was learnt by young Romans in the school of Quintilian, or at best by Gregory and Basil in the retirement of Athens? The young Greek learnt something of geometry; the young Roman something of law; even the young monk of the Middle Ages learnt in his meagre quadrivium some scraps of such science as was then to be had. Are we alone to follow the example of the Chinese in a changeless imitation of our ancestors, and to confine our eager boys for ever between the blank walls of an ancient cemetery, which contains only the sepulchres of two dead tongues?

Such questions crowd indignantly upon the mind; and that they should admit of no answer is a subject of simple astonishment. If English people do not really care about the question—if they are indifferent to knowledge, scorn ideas, and despise *Geist* as a continental importation—then there is an end of the matter; but if, undeceived at length, they begin to realize that a solely classical education even for the few who succeed in it is not the best, while for the multitude who fail, it ends in no Latin, no Greek, and nothing else: then it is full time for their voice to be heard. The mighty stream of public opinion must be brought to bear upon universities and schools, and if they cannot be *aided* from within they must be *coerced* from without, to modify a curriculum which has long been too narrow and antiquated, and is now demonstrably unsuccessful. In a scientific age their studies ought *not* to be *solely* literary: in a progressive, practical, and earnest age they must not be suffered to remain stationary, fantastic, and preeminently pagan.

That Greek and Latin—taught in a shorter period, and in a more comprehensive manner—should remain as the solid basis of a liberal education, we are all (or nearly all) agreed: none can hold such an opinion more strongly than myself: but why can it not be frankly recognized that an education *confined* to Greek and Latin is a failure, because it is an anachronism? It has outlived its time. It is utterly out of harmony with the spirit of the age. It may have been all very well three centuries ago, but is it to remain unaltered after three centuries, which in the history of the human race have the importance of thirty? * This is an age of progress, and we keep spinning round and round on the same pivot; an age of observation and experiment, and we keep bowing and scraping to mere authority; an age, as Professor Huxley has said, "full of modern artillery, and we turn out our boys to do battle in it, equipped with the sword and shield of an ancient gladiator." Its continuance is due, not to its importance, but mainly, as the Commissioners admit, to custom and prescription; and now the new wine is bursting the old bottles. The days when men were grateful to a literature which had unshackled them from the fetters of scholasticism—that heroic age of classical studies when in spite of bad food, bad lodging, straw beds, and eternal horrible floggings, youths came on foot vast distances to crowd the school of a Tempête—the days when such men as Erasmus and Stondonck, after toiling out the daylight, mounted a clock-tower to study still by the gratuitous moonbeams—the days when Ronsard and Baïf occupied one bedroom, and rose, one after the other, in turns, long after midnight, to go shares in their single candle and keep the same spot warm; those days have gone by for ever, and we can neither reproduce their acquisitions, nor galvanize them into life. People see now, as Montaigne's father saw more than three hundred years ago, that a scholar may cost much too dear; and they are *beginning* to see that to produce your scholar here and there, you are apt to sacrifice multitudes of minds no less gifted, it may be far more gifted, than his. The complaint is no new one. "It is deplorable," says the poet Cowley, himself a brilliant scholar, "to consider the loss which children make at most schools, employing, or rather casting away, six or seven years in the learning of words only, and that very imperfectly." "We do amiss," says John Milton—a man whose opinion is of infinite importance, not only for his immense learning and splendid imagination, but also because he stands out of nineteen Christian centuries as one of the grandest ideals of a noble and cultivated manhood—"We do amiss to spend seven or eight years merely in scraping together so much miserable Greek and Latin, as might be learnt otherwise easily and delightfully in one year." "Would not a Chinese who took notice of our way of breeding," says John Locke, "be apt to imagine that all our young gentlemen were designed to be teachers and professors of the dead languages of foreign countries, and not to be men of business in their own?" After three such testimonies I need hardly add more; but I *could* easily produce a catena of overwhelming testimony in the same direction of our best and greatest men; from these down to Whewell and Macaulay, and that great, good Prince, who has many a time sat in that chair, and whose wisdom and foresight we only then began to acknowledge, when he was far beyond the reach of our ungrudging censure and our niggardly applause. And that voice would be swelled not only by the all but unanimous testimony of our greatest men of science—the Herschels, the Tyndalls, the Huxleys, the Faradays—but also by a vast crowd of our living statesmen, orators, philosophers, and poets; nay more, from both our Universities, by some of the very best and profoundest scholars of the present day. It should be a significant sign to our educational conservatives that within this very year two such statesmen and thinkers as Mr. Lowe and Mr. Mill—the one eminent as a scholar, the other as a philosopher—should both have spoken of the main staple of our Public School education with scathing and undisguised contempt. Impenetrable as the deaf and sluggish majesty of prescription, and serene as the dull self-complacency of routine may be, it is impossible not to hope that, assaulted by such batteries as these, routine and prescription are beginning to totter to their fall.

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* See H. Rigault, Œuvres Complètes, ii. p. 150.

I have less, however, to do with any *general* cause for the failure of our system, than with two special ones of which I wish particularly to speak. I mean, first, the fact that some of our existing methods are so disagreeable and illogical as to clog all progress, even in our narrow path, with difficulties all but insuperable, even when we have absorbed an inordinate length of time; and secondly, that we have hitherto coldly refused, or but partially admitted, an alliance with those fruitful scientific studies, which can put forth a claim to reverence far prouder than our own, and which would have given our own studies material assistance by the very act of making room for more.

First, then, I believe that one of the reasons why classical studies lie across the path of education, unprogressive themselves and a hindrance to all other progress, is the present superstitious devotion to Greek and Latin composition, and the present irrational mode of studying grammar. It is in this direction that our reform must be most radical and most imperative.

That nebulous halo of admiration which for many years has so densely enshrouded Greek and Latin composition, and which has given to their proportions as seen through the mist a sort of indefinite and colossal grandeur, was perhaps the cause why the Royal Commissioners expressed no word of reprobation against the pursuit, and neither sought nor elicited any evidence condemnatory of their practice. To myself, trained in the system for years, and training others in it for years—being one of those who succeeded in it, if that amount of progress which has been thought worthy of high classical honours in two Universities may be called success—influenced therefore by every conceivable prejudice of authority, experience, and personal vanity in its favour—I can only give my emphatic conclusion that every year the practice of it appears to me increasingly deplorable, and the theory of it every year increasingly absurd. Any facility which I may myself have attained in it I hold exceedingly cheap; I should estimate its value as simply nought in any inventory of my intellectual possessions. The utterly extravagant value attached to Greek and Latin verse, and the utterly untenable arguments urged in its favour, are irritating enough; but with me all minor irritation is lost in deep pain and regret, when year after year I see boys of eighteen and nineteen who have been working for ten years or more at Latin verses under conscientious and able teachers, and who at the end of that time are unable to produce one single line that is not flagrantly incorrect and intolerably odious to every reasonable mind. Almost daily it is my fortune to see poor boys ploughing barren poetic fields in the shape of verse-books with a grammar and dictionary "unequally yoked together like ox and ass." This is the kind of thing they have to turn into Latin elegaics—for instance (to give only a favourable specimen), this lucid address to the sun:—

> "Thou, midmost of our world, I narrate wonders,
> Rulest stars, lest they should wander, laws being broken."

Or this:—

> "The fiery steed, his tail in air proudly cocked,
> Not without much neighing, traverses glad pastures."

This is the sort of "kelp and brickdust" used to polish the cogs of their mental machinery! And when for a good decade of human life, and those its most invaluable years, a boy has stumbled on this dreadful mill-round without progressing a single step, and is plucked at his matriculation for Latin prose, we flatter ourselves, forsooth, that we have been giving him the best means for learning Latin quantities, for improving taste (or what passes for such!) and—*credite posteri!*—for acquiring the *niceties* of Greek and Latin scholarship! We resent the nickname of the Chinese of Europe, yet our education offers the closest possible analogue to that which reigns in the Celestial empire, and for centuries we have continued, and are continuing, a system to which (so far as I know) no other civilised nation attaches any importance, yet which leaves us to borrow our scholarship second-hand from them; which is now necessary for the very highest classical honours at the University of Cambridge alone; in which only *one* has a partial glimmering of success for hundreds and hundreds who inevitably fail; and in which the few exceptional successes are so flagrantly useless, that they can only be regarded at the best as a somewhat trivial and fantastic accomplishment,—an accomplishment so singularly barren of all results, that it has scarcely produced a dozen original poems on which the world sets the most trifling value, or which (as I believe) even a Bavius or a Mævius could have owned without a blush. While we waste years in thus perniciously fostering idle verbal imitations, and in neglecting the rich fruit of ancient learning for its bitter, useless, and unwholesome husk—while we thus dwarf many a vigorous intellect, and disgust many a manly mind—while a great University, neglecting in great measure the literature and the philosophy of two great nations, contents itself with being, in the words of one of its greatest sons, "a Bestower of Rewards for Schoolboy Merit"—while thousands of despairing boys thus waste their precious hours in "contracting their own views and deadening their own sensibilities" by a failure in the acquisition of the useless—while we apply this inconceivably irrational process to Greek and Latin, and to no other language ever yet taught under the sun—while we thus accumulate instruction without education, and feel no shame or compunction if at the end of many years we thrust our youth in all their unarmed ignorance through the open gate of life—while, I say, such a system as this continues and flourishes, which most practical men have long scorned with an immeasurable contempt, do not let us consider that we have advanced a single step in reforming education, to reform which (in the words of Leibnitz) is to reform society and to reform mankind.

Do not imagine that any of these convictions are new. If time permitted I could corroborate them by multitudes of facts, maintain them with most cogent arguments, and support them by the most splendid authority,—if indeed authority be needed to prove that an unique absurdity, condemned by its unique failure, is irrational and wrong. Phillips compared it to going from point to point in curved lines. It is a "preposterous exaction," said Milton, the greatest of all our Latin versifiers, no less than one of our greatest men; "these are not matters to be wrung from poor striplings, like blood from the nose, or the plucking of untimely fruit." "See that your son be not employed in making themes, neither verses of any kind," says the strong common-sense and manly wisdom of John Locke; "it is a sort of Egyptian tyranny," and "if he have no poetic taste 'tis the most unreasonable thing in the world to torment him and waste his time about that which can never succeed." "Versification in a dead language," says another eminent scholar—Lord Macaulay—"is an exotic, a *far-fetched*, *costly*, *sickly* exotic. The soils on which the rarity flourishes are in general as ill suited to the production of native poetry as the flower-pots of a hothouse to the growth of oaks." "It appears to me a *cruel absurdity*," says Bishop Thirlwall, "to attempt to forestall an imitative instinct . . . by forcing young boys through the hardest drudgery and at a great expense of time, to wrap the vacancy of thought in Ciceronian phrases, and to hammer nonsense into Horatian metres." "To what purpose," asks Mr. John Stuart Mill, in his recent great address at St. Andrew's,* which I should like to see framed and glazed in every schoolmaster's study, "should the most precious years of early life be irreparably squandered in learning to write bad Latin and Greek verses? I do not see that we are much the better even for those who end by writing good ones. Can our favourites of fortune find no better or more serious employment than these *nugæ difficiles?* Are we to pay this extravagant price for acquiring the pernicious faculty of stringing together borrowed phrases—a habit which a teacher should consider it one of his first duties to repress?" For myself, as one who has seen the thing in actual working, I will only add, that if one could but show the world what the teaching of Latin verse practically amounts to, and the kind of paltry *poésie épithétique* in which at the best it practically ends,—if the verses written in any one verse examination by any one school in any one day were but laid before the world, with the ages of the boys appended, and the number of hours which the boys have spent for years in thus not progressing a step in this enervating drudgery, *I firmly believe that the system would not last a week longer*, because then Englishmen would see—as clearly as I know, that an ever increasing number of scholars and of schoolmasters have long seen—that in sacrificing so much time and so many branches of study to the non-achievement of this puny accomplishment, is to make our sons slave in the service of a huge gilt empty idol—it is to worship a fly or a beetle, and daily to offer a hecatomb of costly oxen in sacrifice thereto.

I pass from our empty infructuous years of Greek and Latin verse-making, to another blot upon our system no less pernicious,—I mean the illogical and indefensible way in which we teach grammar. Here too, I believe, we have another instance of—

> "Blind authority beating with his staff
> The child that might have led him."

Nothing can be more certain than that the comprehension of grammar comes *after* the mastery of language; that the science of grammar (for there is such a science, and a noble one it is) is at once abstruse and difficult, and that its deeply-seated metaphysical principles are best attained by an analysis of abundant linguistic facts already appreciated. Yet what do we do? we try to build up a boy's knowledge synthetically by plunging him at once into a bewildering mass of intricate rules and anomalous exceptions; and instead of making him understand these, we effectually prevent him from ever learning them in any real sense by making him learn them by rote:† and then, as though it had been our express object to paralyse his own intellectual powers, we shroud these mysterious instructions in the very language which he is supposed not to understand! Well may Mr. Herbert Spencer speak of "that intensely stupid practice, the teaching of grammar to children." "Grammar," says Horne Tooke (who surely was a good judge, if any one was), "is among the first things taught, and the latest understood." Yet what happens? what is happening at this moment to your little sons? They are being "dragged through grammar as through a cactus-bush,"—being taught it in a way which always reminds me of Judges viii. 16, where it says that Gideon "took thorns of the wilderness, and briers, and with these he *taught* the men of Succoth." They have been sent to a preparatory school, where the two main implements of education put into their innocent and unsuspecting hands are a primer and a verse-book. The verse-book is the kind of thing of which I have given you a specimen; the primer—that utterly disastrous legacy of the commission which, in spite of the strenuous opposition of many of us, is now forced as a standard grammar upon nine great public, and countless private schools—is a delightful manual in which the little victim, not without amazement, learns by heart in Latin a multitude of such lucid empiricisms as that "factitive verbs have two accusatives, one of the object, the other of the oblique complement!"

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* I may perhaps be allowed to observe that the whole of my discourse (except this clause) was written before the delivery of Mr. Mill's address.

† "Sçavoir par cœur n'est pas sçavoir."—*Montaigne.*

Here too, at the tender age of eight or nine his young imagination is terrified, often by ignorant men, with such incubi and succubi as "quid-quale verbs," "gerundive attractions," "suboblique clauses," "spirants," "receptive complements," "relations circumstantive and prolative," "quasi-passives," "semi-deponents," and I know not what, —which are hard enough for grown men to understand, even if they do not despise this clatter of pedantic (because needless) polysyllables, but which to a child must be worse than "Gorgons, and Hydras, and Chimeras dire." Imagine, ladies and gentlemen, that at this moment you yourselves were desirous to learn Arabic; imagine an Arabic grammar, with rules in Arabic, put into your hands; imagine these Arabic rules clothed in a scholastic terminology, and bristling with philosophical abstractions, interspersed here and there with the castanet music of an abhorrent doggrel; imagine that the Arabic verb, like the Greek verb, had twelve hundred synthetic forms, and that you had to learn them every one by heart before proceeding a step; imagine that this amazing sum-total were forced on you in a solid and amorphous form, perhaps by a wholly incompetent teacher who repressed all questions at the point of the ferula; imagine this, and you have the very photograph of what in very many cases is being done with your little sons in Greek and Latin. Can "the theory of elementary unintelligibility" go farther than this? Would it be possible to be more ingenuously out of harmony with all that is natural? After such a grievous waste of time, are you astonished at failure? Are you surprised if your son, thus suddenly introduced from the mid-sunlight of his boyhood into these "yawning caves where glaring monsters lie," and where, like the Indian hunter, he is forced into chronic indigestion by feeding on dry Greek roots,—are you astonished if he revolts and succumbs altogether? Or, should he be courageous and lucky enough to emerge undazed, retires for life into what Sidney Smith calls "the safe and elegant imbecility of classical learning," with a confirmed habit for "credulously swallowing millstones with passive obedience," —crammed with dead words, but unapt for living inquiry,—with plenty of second-hand knowledge reflected and refracted through the semi-opaque medium of books, but with a sight too bedimmed in this long darkness to gaze on the sun-bright and unveiled countenance of truth. "There is no study," says Professor Halford Vaughan, "that could prove more successful in producing often thorough idleness and vacancy of mind, parrot-like repetition and sing-song knowledge, to the abeyance and destruction of the intellectual powers, as well as to the loss and paralysis of the outward senses, than our traditional study and idolatry of language."

But, if this be so with the successful, what are the results with the unsuccessful? Ask our modern writers of genius, and they will point to the ambition, the dissipation, the restlessness of our wealthier classes. Ask our parents, and they will sigh over the vacant hours spent in lounging in the billiard-room and the stable-yard. Ask our schoolmasters, and they will deplore the number of dunces and idlers whom they produce. Ask our most zealous and earnest college tutors, and they will tell of undergraduates who regard their royal and sacred seats of learning as luxurious and fashionable clubs,—of torpid minds that either do not care to read enough for the most elementary examinations, or only cram through them with infinite difficulty and disgust. Contrast this languishing inefficiency of unprogressive studies with the keen, passionate, eager, undaunted enthusiasm wherewith thousands of minds, hitherto untrained, are flinging their whole energy into the toils of science, and thereby adding year by year to the fair sum of human knowledge; and then ask if the nature of our studies be not to blame? "I do not wonder," says Tanaquil Faber, who, at least, succeeded in making his own daughter, Madame Dacier, one of the first scholars of her day, "that one-half of our boys who go to schools, do become downright asses rather than learned men." While, if you turn once more to Milton's opinion, you will find that he sets down barren hearts—a tendency "to live in ease and luxury," and "an ambitious, mercenary, or ignorantly zealous divinity"—to the fact of boys "misspending their prime youth at the schools and universities in learning mere words," and being "deluded with ragged notions and babblements, while they expected worthy and delightful knowledge."

With facts like these before us, how long, I ask, are we to leave our education "sickening in this muddy pool of conformity and tradition?" Are we to go on for ever conjugating and declining, and gerund-grinding, and Latin-verse manufacturing, "while the great world spins for ever down the ringing grooves of change?" I speak, be it observed, in the highest interest of Classics. I would not abandon them as the basis of a liberal education; I feel their abuse somewhat bitterly, only because I know how great may be their proper use. I am not one of that large and increasing multitude who say that classical education is a barren tree, and that the axe must be laid mercilessly at its very root. I say, on the contrary, that it is a fair tree and a strong, and that if we cut off its dead and unsightly branches, we may still leave its stem in the tender grass of the field, and that men's hopes and fears may still—

"Take shelter in
The fragrance of its complicated glooms,
And cool impleachèd twilights."

I know that the Classics introduce us into a region of virtues in which our modern life is meanest and most meagre. I should hold it disastrous to disintegrate ourselves altogether from the past, and to break the chain of its noble associations. But this is the precise effect which is now being produced by the wretched baldness and poverty of our system. The worst enemies of classical education are those who would stereotype its present imbecility. I am no enemy, but a sincere and humble supporter of classical education *properly supplemented and properly understood.* That against which I have been pleading is not knowledge of the classics, but ignorance of their entire spirit; not classics, but the degradation of the classics; not the thoughts of the ancients enshrined in their noblest literature, but a paltry stringing together of the artificial phrases of their rhetoric; not a sound learning, but a shallow simulacrum of superiority; not mental training, but a mere knack acquired by desultory reading and incessant practice; a peacock's feather, which, though it has often been a proof of intellectual rank, yet often waves over very empty skulls—a trick so difficult and so useless that it averts robust minds from *all* classical study, and is capable (on the published confession of some of its best representatives)* of coexisting with a profound ignorance on all subjects, ancient as well as modern. *These* are not the results of classical teaching in any high and noble sense; but of its fantastic abuse by methods which our best and wisest men have combined in denouncing as glaringly irrational and curiously bad.

But further, and lastly, I would ask, if this idol of the theatre is still to be worshipped, how long shall its service be so exclusive as now it is? *However* high we are to place classics, are we still to act as though they were the sole end and aim of education, and as though men had neither the faculties nor the thirst for any other kind of knowledge? Are we never to get rid of this bed of Procrustes, which for some is inordinately long, for some intolerably short? I allude of course to that second special cause for the failure of our system, in the short-sighted neglect which has suffered our boys to grow up in total ignorance of, sometimes in disgraceful contempt for, every scientific pursuit. In an age which is emphatically the age of science,

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* See Public Schools Commission Report, ii. 43, 44, 50; Cambridge Commission Report, p. 293.

"Mid the mighty march of mind,
The steamship, and the railway, and the thoughts that shake mankind,"

a boy has been suffered to know nothing of the world of wonder, of beauty, and of power, in which his lot is cast. What has science achieved within this century? She has made the shattering force of the electric spark obediently speed her messages through the heart of iron mountains, and under the waves of raging seas; she has kindled her silver beacons on the wave-tormented crags, as though to light up an avenue to her palace front; she has enabled the sailor to steer in safety amid the breaker's wintry surge, and the miner to work in safety amid the blasting fire-damp of the mine; she has drawn the forked lightning in harmless splendour out of the purple cloud; she has discovered the precious anodyne which lulls the senses into calm and dreamless sleep, while the work of agony, agonising no longer, is wrought upon the human frame; with a scratch of her lancet she has stayed the loathsome ravages of disease; she has forced upon reluctant selfishness, and branded into the brain of invincible ignorance, those beneficent laws which paralyse the fury of the pestilence, and restore health and buoyancy to the factory and the hut; all this and more, she is doing, and has done; and the history of her discoveries, and the knowledge of the methods she has used in all this majestic sorcery, would, I take it, be *almost* as useful, and would effect as much for the human race, as the most delicate appreciation of the particle γε, or even as an approximate knowledge of the uses of ἂν with the moods! Oh, if the world is to be transformed for our boys into the cave I have described, at least suffer them to look round upon it, and enter it torch in hand:—

"And bid with lifted torch its starry walls
Sparkle, as erst they sparkled to the glow
Of odorous lamp tended by saint and sage!"

It would give *reality*, it would give *utility*, it would give *happiness* to their education. It would give reality. Our present system does not represent the existing state of knowledge. It deals with names, not things; with grammar, not facts; with books, not phenomena. It learns, but does not acquire; it imitates, but does not observe. With a Paradise open before us, it fumbles at the old and costly key of a second-hand knowledge. It acts as though God had turned His creatures into a world in which there was no such thing as education until Greece and Rome emerged. What wonder that our boys have ceased to feel all relation between these dead and barren vocables and their bright and living world? We have not enlisted among them the services of what Dr. Brown calls "that resident teacher within the skin," who is for ever giving his lessons while we are giving ours. Our boys are getting weary of Horace and Ovid:—

> "Earth outgrows the mythic fancies
> Sung beside her in her youth;
> And those debonnaire romances
> Sound but dull beside the truth;
> Phœbus' chariot-course is run!
> Look up, poets, to the sun;—
> Pan, Pan, is dead!"

Then it would give our education more *usefulness*. I use the word in no vulgar or sordid sense. I do not only mean professionally—though surely that is not wholly unimportant; nor do I mean by the stimulus to great philanthropic discoveries, though that too ought not to be despised; but I mean *morally*, and *socially*, and *intellectually* as well. To speak of the benefit of scientific knowledge to our physicians, our barristers, our engineers, our soldiers, our country gentlemen, would be waste of time, and owing to a deeply-seated stupidity I have never been able to see anything specially glorious in inutility *per se*. But it is worth while to take the single instance of the use of science to our clergy. Seeing that the Bible, in page after page, to say nothing of whole books of it, is constantly occupied in directing profound attention to the power of God as proved by the magnificence of His Creation,—seeing that the Saviour of the world points, as the special proofs of God's love, to His care for the mountain lily, and the falling sparrow, and the raven's callow brood,—is not our education, and especially that of our clergy, distinctly *irreligious* in neglecting these things, and in elevating the poor words of man, as an instrument of training, unmeasurably above the mighty works of God? And with what results? It would be hardly possible to exaggerate their disastrous importance. Not only do the clergy, who should be the leaders of thought, lose the advantage of assisting in a thousand ways their poorer parishioners, but they find themselves actually inferior in these great fields of knowledge to many clerks and artisans in their own congregations, before whom they cannot venture to speak of them without the danger of raising a contemptuous smile. This, however, is the least part of the evil. Science has interpenetrated to a wonderful degree the thoughts, the speculations, nay, even the common literature of the age, and yet the clergy are wholly out of sympathy with it; in many instances are suspicious of it; in many more are its bitter and ignorant opponents. Scarcely has there been an eminent philosopher, from Roger Bacon down to Comte,—scarcely an eminent discoverer, from Galileo down to Darwin, who has not counted the clergy among his most ruthless opponents. I challenge denial of the fact. Against astronomy, against zoology, against chemistry, against geology, against ethnology, against philology,—against well-nigh *every* nascent science in its turn—has theological arrogance and self-styled orthodoxy marshalled their menacing array of misinterpreted or inapplicable fragments of Holy Writ. Just as of old "fops refuted

Berkeley with a sneer," so now some young ordained B.A. finds it easy to crush Darwin with a text. Is it, I ask, uncommon to hear some ignorant clergyman who has laboriously scraped into a poll degree, lay down the law as though he held the keys of all knowledge in his hand, and could afford to pity and look down upon those splendid students, whose lives have been one long-continued heroism of candour and research? You may say that an opposition of this calibre usually ends in some complacent avowal of the ardent friendship between science and theology, and in the acceptance as axiomatic truisms of what had previously been denounced as atheistical and absurd. But meanwhile, what happens? Men of science, confounding religion with the anachronisms of its most feeble and most violent expounders, too often hold aloof from a Church whose inmost heart is intensely truthful,—a Church which well knows the delight that deeply religious minds have ever felt in reverent inquiry into the laws of God, and which sees more of her own real spirit in the patient labours of science than in unprogressive idleness and theological hate.

The remedy is simple. Let the boys who are to become our clergy be trained, if it be but in one single science; let them see the stern and simple accuracy of its methods; let them observe the singleness of its eye for truth, and truth alone;—let them mark its inevitable progress over triumphant errors;—let them be initiated in the labour, the sincerity, the patience, the self-devotion, the precision of thought which it requires,—and then we shall hear no more of the preposterous falsehood that science is inimical to true religion, and see no more of men who prefer anathema to inquiry,—who rather than sit at God's feet, and learn the great laws which He reveals to the humble and to the patient, prefer to gyrate round and round in the petty circle of one-thousandth-hand expositions, or, if I may borrow an old simile, to make long voyages in the belly of Jonah's whale,—traversing immense distances but seeing nothing in the world. It is my solemn belief, and a belief that I am neither afraid nor ashamed to avow, that the scepticism so often laid to the charge of science, is in reality the *necessary* result of its neglect.

In conclusion, I confidently look to an education in science for a considerable increase of youthful happiness. How do many boys regard their school-hours? Chiefly as time spent in the close atmosphere of a dull room over verbal disquisitions for which they care little, and verbal imitations for which they care still less. If you would have them progress in such studies, and believe in them, you must superadd *another* education, which by enlisting their sympathies, shall awake their dormant faculties and save their decaying self-respect. You must teach the boy who fails in classics that the authority of long words in crabbed books is not the *only* source of knowledge; that he can use his untrained senses as the gateways for a thousand forms of instruction; that the theory which strove to make teaching unpleasant was an odious and unnatural heresy; and that his education, so far from ceasing, is but being continued (if he so wills it) with greater intensity when with open eyes, and senses keenly observant, he is living under the blue temple-roof of heaven,—storing his cabinet with the gorgeous and delicate loveliness of shells and butterflies and eggs,—or following the game on the mountain-side, ancle-deep in purple heather, knee-deep in the tall green ferns. Teach him that in every dewdrop brushed away by his careless feet there slumbers the electric flame; teach him that there is no flower which he can pluck, in the study of which he may not soon reach those "flammantia mœnia mundi" over which not even the eagle wing of science can soar; teach him that there is not the simplest phenomenon of his daily life which does not involve for its explanation the agency of the most marvellous and eternal laws. Show him too that, when once taught to read and write, and think, and use his senses, he has been equipped in *all* the panoply wherewith the giants of human intellect have made the elements of the physical world their slaves; that with no more potent instrument than his own kite he may take the lightning by the wing; that in the falling of an apple, and the swinging of a lamp, and the thoughts of an idle boy as he watched the steam condensing on the bowl of his silver spoon and the twitching of a dead frog's leg when it was touched by a scalpel as it lay upon a plate, lay hid the secrets of the rolling of the planets, and the mode in which we mark the flight of time, and the rushing of the railway engine, and the means whereby with the simplest possible materials we can put a girdle in a few seconds round the globe. Add this to your classical system, and the frequency of dunces will *cease* to be a commonplace of schoolmasters. I dare not indeed use the strong language of Milton, when he says, "I doubt not that ye shall have more ado to drive our dullest and laziest youth, our stocks and stubs, from the infinite desire of such a happy nurture, than we have now to hale and drag our hopefullest and choicest wits to that asinine feast of sow-thistles and brambles which is commonly set before them as the food and entertainment of their tenderest and most docile age;"—but I *do* say with him that the path of a virtuous and noble education is "so smooth, so green, so full of goodly prospects and melodious sounds on every side, that the harp of Orpheus was not more charming,"—I do say that the kind of education which here I advocate will be fruitful of the mightiest advantages both to England as a nation and to thousands of her individual sons,—and that it is

"Not harsh and rugged, as dull fools suppose,
But musical as is Apollo's lute,
And a perpetual feast of nectared sweets,
Where no crude surfeit reigns."

[F. W. F.]

WEEKLY EVENING MEETING,

Friday, February 15, 1867.

Sir Henry Holland, Bart. M.D. D.C.L. F.R.S. President,
in the Chair.

Cromwell F. Varley, Esq. M.I.C.E. M.R.I.

On the Atlantic Telegraph.

The object the speaker had in view was to demonstrate (for the first time in public) a few of the more subtle phenomena, which present themselves when attempting to work long submarine cables, and to show how the disturbances arising from earth currents are sufficiently neutralized to prevent them from interfering with the telegraphic signals.

For this purpose he had two artificial telegraph cables, the one representing the Atlantic Telegraph cable, the second one being made 40 times slower, so as to represent the phenomena which would present themselves on a cable of the same dimensions per nautical mile as the Atlantic Telegraph, but 13,000 miles in length, long enough to reach from England to Australia.

The former artificial cable consisted of 11 coils of fine German silver wire, having together the same resistance as the actual Atlantic cable. The phenomena of electro-static induction were given to this conductor, by attaching at each of the junctions between the coils a large condenser. (*Vide* Fig. 14, p. 49.)

The second or slow artificial line consisted of 11 glass tubes; and the apparatus was so constructed that by turning a handle the condensers could be removed simultaneously from the resistance coils, and reapplied as often as necessary. A galvanometer was inserted between each of the tubes in the latter circuit.

The glass tubes were filled with a solution consisting of 98 parts pure water and 2 parts sulphate of zinc.

The metal poles dipping into the solution were composed of zinc amalgamated; amalgamated zinc electrodes in a solution of sulphate zinc being almost entirely free from polarization.

The reflecting galvanometers differed from those hitherto used in the following respects:—the mirrors consisted of lenses ground and polished, instead of flat microscope glass, the tubes containing the mirrors had glass ends and were filled with pure water, which cut off the tremors of the room, and brought the instruments to rest in the fraction of a second.

Each mirror was half-an-inch in diameter, and with its magnet weighed 2 grains, and was suspended by 3 cocoons of silk $\frac{1}{16}$th of an inch in length.

The time allowed for this discourse only permitted of a cursory examination into two of the many interesting inquiries connected with the Atlantic Telegraph.

The speaker observed:—"The press had (imperfectly) familiarized the public with the mechanical operations of manufacturing and laying the cables, as well as with the means adopted for navigating the ship across the ocean.

"The method adopted for testing the integrity of the insulation of the cable as well as of its conducting power were partly brought under the notice of this Institution by Sir William Thomson not long since."

Light and radiant heat travel through space with a definite velocity; electricity does not. The waves of light do not flatten out or elongate during their flight over millions and millions of miles. For example, suppose the star Sirius were suddenly covered by a screen for ten minutes and then uncovered again, the star would still appear on this earth to shine continuously for about twenty years after the screen was applied, when it would be suddenly extinguished for just ten minutes, and then re-appear.

Electricity in passing through a cable begins instantly to appear at the distant end, but in strength far too weak to be measured; after the lapse of a certain time definite for each particular cable, it begins rapidly to augment in power and continues to approach to a definite limit of strength.

The reason for this will appear when the construction of the artificial Atlantic cable is considered.

Electricity is popularly supposed to have a definite velocity, like light; this is not so. The question—what is the velocity of electricity? cannot be answered unless other conditions are given; for instance, it begins to arrive at the distant end instantaneously, but to reach its maximum strength would, strictly speaking, require eternity and a day; while to reach half its maximum strength would have occupied a time of 6 a, this quantity a being a definite time dependent upon the dimensions of the cable.

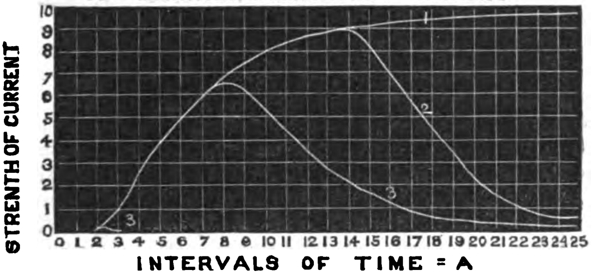
Fig. 1, curve No. 1, represents the strength of the current at the distant end of the cable, after the lapse of periods of time represented by the figures under the diagram, the maximum strength which the battery can produce through the cable being represented by 10.

The figures on the horizontal line represent equal periods of time: a.

FIG. I.

THE LITTLE CURVE 3' REPRESENTS THE SIGNAL OBTAINED BY CURBING

10 = MAXIMUM-STRENGTH-OF-CURRENT



The figures on the vertical line represent the strength of the current. On connecting one end of the cable to a battery whose other pole is to earth, no sensible current is visible until after the lapse of the period *a*, when the current has a strength of a little more than a thousandth part of that which the battery is capable of producing.

When the cable is fully charged, the current attains its maximum strength, which is represented in the diagram by the line opposite the vertical figure 10. After the lapse of 4 *a*, the strength of the current is about one-fourth of this maximum power. After the lapse of 6 *a* it has a strength equal to half the maximum, and for greater periods of time the strength of the current goes on augmenting if the battery be continually applied, as shown by curve No. 1. But although it approaches to, it never, in strict language, actually attains the absolute maximum strength 10. For instance, after 20 *a*, the strength of the current is about 98 per cent. of the maximum.

The curves 2 and 3 show the rise and fall of electric currents at the distant end after the cable has been connected to the battery for intervals of time corresponding to 12 *a* and 6 *a*; and after such contact with the battery, the cable has been connected to the earth again. This is about as quick as it is possible to signal through a submarine cable with the ordinary Morse instrument. No. 2 represents a "dash;" No. 3, a dot.

The little curve 3 represents a signal whose strength is only 1 per cent. of the maximum strength. Such signals are only to be obtained either by the curb-key or the use of condensers at the end of the cable, as explained further on.

It will be seen that considerable time must be allowed to elapse after the impulse has been given for a dot or dash before the cable has sufficiently discharged itself to permit of a second intelligible signal. It is evident from the above diagram that in order to get rapid signalling through an Atlantic or other long cable the apparatus must first of all be very sensitive, so as to give as early indication as possible of the arrival of the electric current, and the moment such indication begins to be produced the line must be discharged as quickly as possible, in order that a second signal may be made to follow quickly afterwards.

On January 20, 1854, Dr. Faraday, in a discourse at the Royal Institution,* described a number of experiments which he had made with a hundred miles of gutta percha-covered wire, and also some experiments upon 1500 miles of line between London and Manchester. But on the table before the speaker was an artificial representation of a cable 13,000 miles in length, by the aid of which the same phenomena amplified and many others were exhibited.

A telegraph cable is a long Leyden jar, one end of which is attached to the earth, whilst the other is attached to a source of electricity each time a signal is to be produced. If the cable be connected to a battery for a long time, the strength of the charge in the different parts of the cable will be shown by the diagonal line, Fig. 2, being nothing at the end connected to the earth, and equal to the full power of the battery at the other.

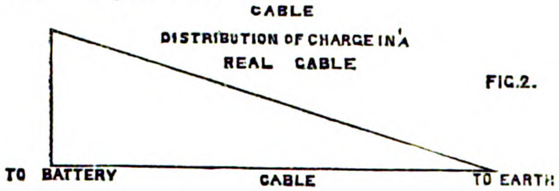


FIG. 2.

Fig. 3 shows the distribution of the charge in the artificial line.

FIG: 3.

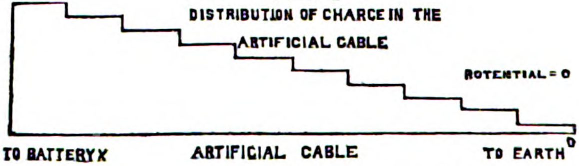


FIG. 6

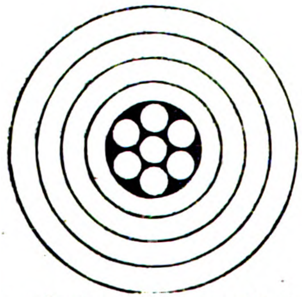


Fig. 6 represents a section of the core of the Atlantic Telegraph cable. The seven small circles are sections of the seven copper wires constituting the conductor—the interstices between the wires are filled with a mixture of gutta percha and wood pitch. Four coatings (or tubes) of gutta percha united together by pitch and gutta percha form the insulating medium. The reasons for using seven wires instead of one solid conductor, are first, to avoid the risk of a flaw or break in the conductor, which would be fatal to the line; secondly, to reduce the rigidity of the copper.

The weights per nautical mile (2029 yards) are—copper, 300 lbs. avoirdupois; gutta percha, 400 lbs.

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* Proceedings of the Royal Institution, Vol. I., p. 345.

When such a core is submerged the copper conductor forms the interior coating; the gutta percha, the insulating medium; the water, the exterior coating of a Leyden jar.

Figs. 13 and 14 represent the artificial cables.

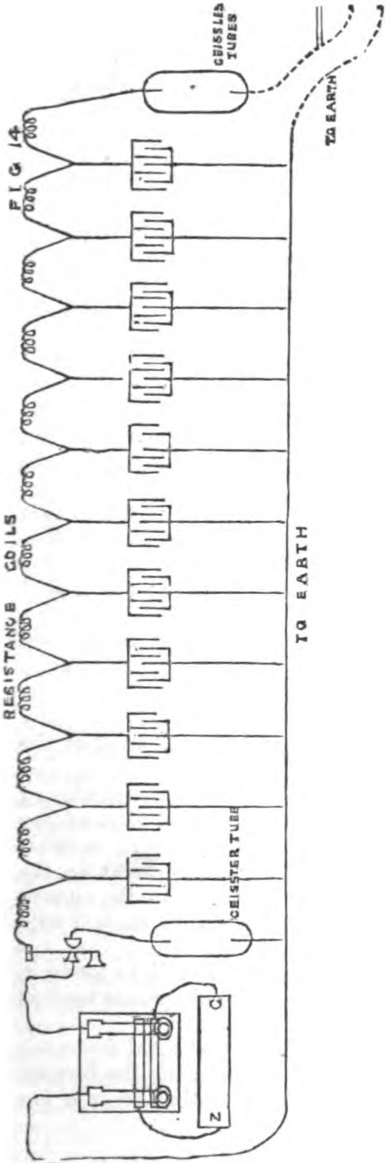


FIG 14
GEISSLER TUBES
TO EARTH
RESISTANCE COILS
TO EARTH
GEISSLER TUBE
C
Z

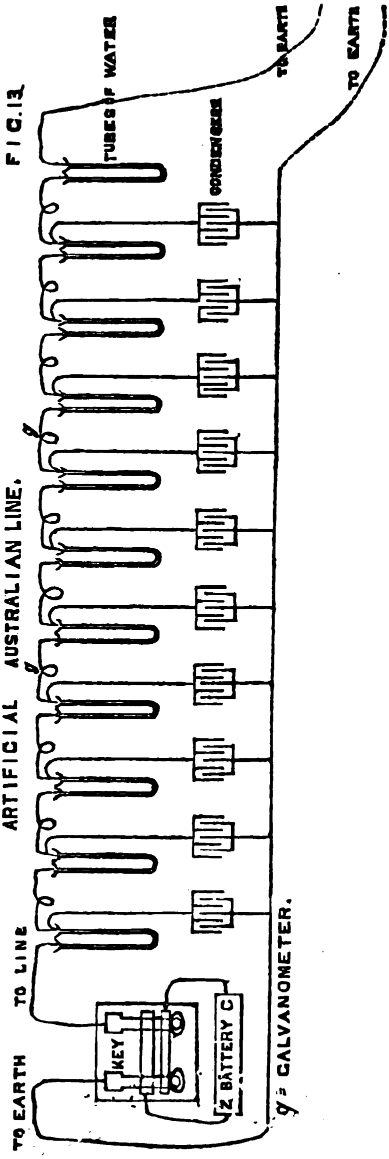


FIG. 13

ARTIFICIAL AUSTRALIAN LINE.
TUBES OF WATER
TO EARTH
TO EARTH
CONDENSERS
g
g
TO LINE
TO EARTH
KEY
Z BATTERY C
g = GALVANOMETER.

When the cable is at rest, the two ends are connected to the earth; on depressing the telegraphic key, a battery is inserted between the earth and the cable, and a current immediately flows in at the sending end; the keys in the diagram are double ones; on depressing the right-hand key, the positive or copper pole of the battery is connected with the cable, while the negative or zinc pole is left in connection with the earth; if the left-hand key be depressed, the right-hand key being up, the negative or zinc pole of the battery is connected with the cable, while the positive or copper pole remains connected to the earth. The second key shown in Fig. 14 was used as a switch to connect the cable at pleasure either to the battery key or to the Geissler's tube, to show the escape of the charge in the cable from both ends of the circuit.

On depressing one of the keys, the battery is inserted between the earth and the cable; a current immediately flows; after passing through the first resistance it meets with the first condenser; here it finds two channels, namely, the condenser and the other resistance coils, forming the remainder of the circuit.

At the first moment of time the condenser being empty, it offers no appreciable opposition to the electric current which commences charging it, and therefore at the first instant of time nearly the whole of the current is spent in charging the condenser. No sooner, however, has the charging commenced than this charge begins to oppose the further flow of electricity into it; a current then flows through the second coil; its strength is dependent upon the potential for the time being of the charge in the first condenser. Example: suppose the potential of the battery to be 100 (and its resistance practically nil), let the resistance of each coil be 1 at the first moment of time; as the first condenser offers no sensible resistance, there will be a resistance of 1 only in circuit. By Ohm's law the strength or volume of the current is—

$$I = \frac{E}{R} = \frac{100}{1}$$

Where I equals the volume of the current, E the potential of the battery, R the resistance in circuit.

After the lapse of a very short interval of time, the first condenser will have become charged to a potential of $\frac{1}{100}$th part that of the battery, *viz.* 1, at that moment of time, as there will be no sensible charge in the second condenser; the strength of the current in the second resistance coil will be $\frac{1}{100}$th part the strength of the current that flowed through the first coil at the first moment of contact with the battery.

Thus it will be seen that no appreciable current will be found in any of the other resistance coils until an appreciable charge has reached the condenser immediately preceding it.

If ten galvanometers were inserted in a long cable at equal distances, it would be seen that at the first moment of contact with the battery, the current rushing into the cable is vastly greater than it is a few moments afterwards: this was illustrated by the speaker on the large artificial cable represented in Fig. 13, p. 49. The ten galvanometers were placed one above the other; they were carefully adjusted so that the ten images which they reflected from the electric lamp upon a large white screen formed a vertical line when no current was passing (*vide* Fig. 4, p. 52). The galvanometers were all of equal sensibility.

To familiarize the audience with the relative positions of the different galvanometers, the speaker considered the upper part of the screen to be England, the lower part the Antipodes.

The 1st station was named Gibraltar, the 2nd Malta, the 3rd Suez, the 4th Aden, the 5th Bombay, the 6th Calcutta, the 7th Rangoon, the 8th Singapore, the 9th Java, the 10th Australia.

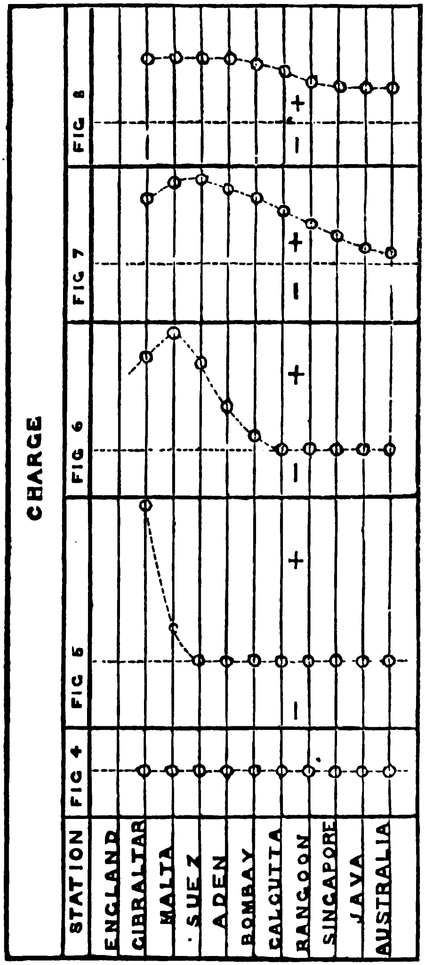
No galvanometer was inserted at England, because the discharge from the cable through it to the earth was so powerful that the magnetism of the needle was reversed by it, and the indications rendered alternately the opposite of the truth.

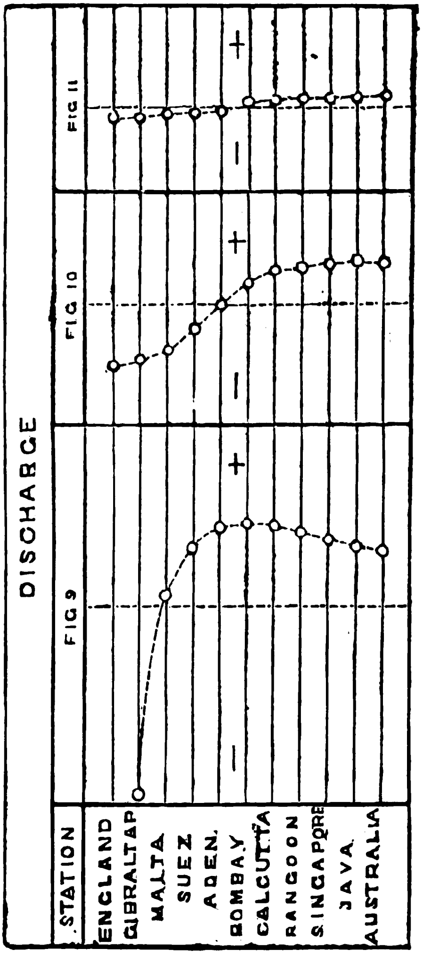
The speaker first of all connected the condensers of the artificial line all together, and charged them with a battery of 800 cells, Daniel's battery. On discharging them by means of a sheet of tinfoil, a loud report, a brilliant flash, and an irregular hole five-eighths of an inch in diameter were the result.

The second experiment consisted of connecting the 800 cells with the artificial Atlantic cable (Fig. 14, p. 49), the condensers being removed from the resistance coils. The instrument used to show the passage of the electric current was an exhausted glass tube (a Geissler's tube), whose resistance was such that it would just allow a current of 400 cells, Daniel's battery, to pass from wire to wire. This tube formed a ready method of showing when the charge at the Newfoundland end reached half the potential of the battery. The condensers being removed from the artificial line, the current appeared instantly at the Newfoundland end, and disappeared instantly upon the connection between the battery and the cable being broken at the English end. When the condensers were applied, an interval of 3 or 4 seconds elapsed before the current appeared; and on breaking the connection between the battery and the cable at the English end, the current still continued to flow out of the Newfoundland end for many seconds afterwards.

The experiment was varied by connecting the cable to the 800 cells, until, by the brilliancy of the Geissler's tube at Newfoundland, the cable was seen to be nearly fully charged; the battery connection was then broken at the English end, and the cable connected to the earth by a second Geissler's tube, the latter shone with a more brilliant light than that at Newfoundland, because the charge in the cable near the battery was greater than at the distant end, thus forming a rough but very pretty illustration of Figs. 1 and 2, pp. 47, 48. The tube at the English end continued to shine for several seconds after the Newfoundland end had gone out, because the charge at the end of the cable near the battery was greater than at the other end.

A smaller battery was then connected with the long, or Australian, artificial line. A bundle of rays from the electric lamp was thrown upon the 10 galvanometers, each of which reflected a little sun-like spot upon a large white screen, forming, when no current was passing through the cable, a straight vertical line of luminous points (*vide* Fig. 4). On depressing the right-hand key, Gibraltar almost immediately responded, and when it had travelled about 6 feet over the screen (*vide* Fig. 5), Malta began visibly to move.





Later still the current* at Gibraltar decreased considerably in strength (Figs. 6, 7, and 8), owing to that end of the line becoming charged. Fig. 7 shows approximately the appearance presented after the cable had been connected to the battery 14 seconds; and Fig. 8 shows the appearance after the lapse of nearly a minute, when a powerful current was rushing out at the Australian end. The English end was then removed from the battery and connected with the earth; and quickly after the Gibraltar spot rushed across to the other side of the screen, indicating the rolling back of a powerful current to the earth. This was followed shortly afterwards by Malta, Suez, and Aden. Bombay came only as far as the zero line, at which time the currents in the different parts of the cable were flowing out at each end, leaving Bombay neutral. It was some minutes before the cable was sufficiently discharged to allow the ten spots to come near enough to the zero line to admit of a second experiment.

The curves produced at successive intervals of time while the cable was discharging are shown by Figs. 9, 10, and 11. Fig. 9 giving the position of the spots a second after the English end was connected to earth; Fig. 10, the appearance shown at a still later period: Fig. 11, after the lapse of about a minute. Here it will be seen the cable having become charged had to discharge itself by pouring its electric charge out at each end.

When a succession of signals was sent into the cable by alternately depressing and elevating the key, for periods of five seconds each, these impulses produced waves which could be distinctly traced as far as Aden, where their individuality became lost. The little spots beyond this station showed the presence of a current resulting from the combination of these successive waves.

The speaker then pointed out his methods of clearing the line after each impulse. In 1853–54 he invented a plan which consisted of sending after each positive current, a negative current—a plan which is now generally used in working submarine lines.

In 1856 he invented a plan which consisted of sending a strong positive current of definite strength and duration into the line, followed by a weak positive current to produce a signal; this being followed by a strong negative retreat, succeeded by a weak negative current to clear the line. This system was a great improvement upon the former.

In 1858 Professor Sir William Thomson proposed the use of three currents of *equal duration but irregular strength* and alternate signs, which produced a still more rapid result.

In 1863 the speaker found, by experiments on his artificial line, that by using a succession of four or five currents, *all of the same strength but varying in duration*, greater rapidity could be secured.

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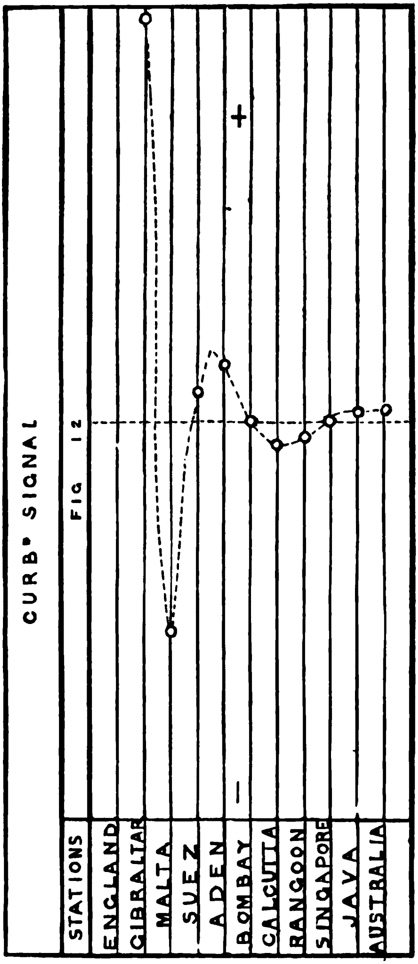
* In Figs. 10 and 11 the engraver has made a mistake, and inserted one galvanometer too many.

For example, first a positive current, followed by a negative of longer duration, followed again by a positive current of much less duration; then a shorter negative current, and that again by a very short positive current, produced a succession of positive and negative waves throughout the line: the result of all of which was at the Australian end one very small positive wave, perfectly distinct, the rest of the line being left almost immediately entirely free from all traces of electricity, ready for the production of a second signal.

This is shown by Fig. 12.



In the previous experiments, when an impulse had been given the charge had to find its way to the earth through the two ends of the cable.

By experiments on actual cables the speaker had at an early date ascertained that the rate of transmission through a cable is independent of the potential or intensity of the battery, and that it varies inversely as the square of the length of the cable, excepting in the case of very short cables thickly covered with iron, where the retardation caused by the magnetic inertia of the iron forms an item of some importance; and in the foregoing experiments it has been assumed, which is really the case with such a cable as the Atlantic, that this retardation is too small to be worthy of consideration.

Professor Thomson, by mathematical reasoning, has calculated, and the artificial cable has demonstrated, that the strength of the current at the distant end varies, as shown in Fig. 1, p. 47; and that with a given potential or intensity there is no possible means of expediting this rate of arrival: and therefore, in order to get high speed, highly sensitive instruments must be used, and so soon as a visible indication has been produced at the distant end, the cable must be cleared of its charge, so as to admit of a second impulse or signal being given.

The instrument known as the curb key seems to carry this to the utmost limit, and its success depends upon the following consideration:—If the line be cut in half, the speed of transmission is increased four times. If now one-half the cable were made positive while the other half was made negative, it is clear that while the first quarter of the cable which was negative is discharging to earth, and while the last quarter of the cable which was positive is discharging to earth, the two intermediate quarters would discharge by rushing into each other: consequently the electricity having a shorter distance to travel, the discharge is effected much more quickly.

Fig. 12 represents the currents in the cable, after a curbed signal of five currents producing three positive and two negative waves in the cable, the first positive current to produce the signal was followed by a longer negative current to cut off the upper portion of the curve No. 1, Fig. 1, p. 47. This would have produced a powerful negative signal, therefore a shorter positive current was sent into the line to curb it, and that followed by a still shorter negative, followed by a very short positive.

These five alternate currents were all sent into the line before any signal was visible at Australia.

Almost immediately after the appearance of the current at the Australian end the alternate waves neutralized each other, and the ten spots fell rapidly upon the zero line, showing that the cable was discharged throughout.

The little curve 3′ Fig. 1, p. 47, represents a signal curbed down to nearly 1 per cent. of the original wave, while the big curve 3 represents the short signal of the ordinary Morse key, the latter requires at least 18 *a* for its formation, while the former can be made in from 2½ to 3 *a*.

There seems a definite limit beyond which the curbing cannot be carried with advantage. The eye of the operator can read the signals even when considerably distorted; and when the curbing is carried below the $\frac{1}{100}$th part of the original wave, which the speaker has done upon the Atlantic cable itself, an interval of time has to be left between each signal to admit of their being individualized; *i.e.* the signals will not bear distortion, and the line must be more thoroughly cleared before a second signal which shall be distinct can be made to follow.

The speaker explained the plan he had invented in 1862 for

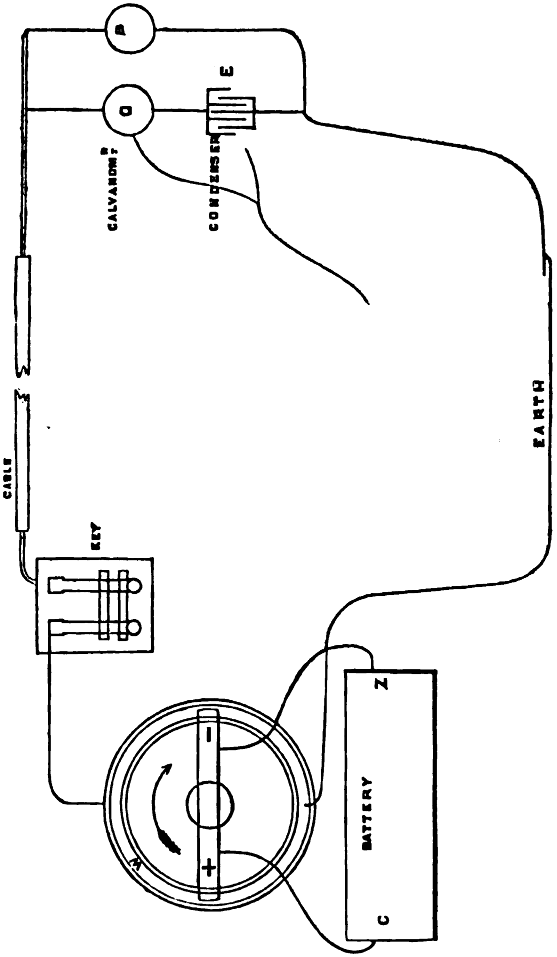


FIG. 15

expediting the signals through the cable, and for cutting off the disturbances arising from the Aurora Borealis, and which were generally designated "magnetic storms."

These phenomena are not unfrequently experienced simultaneously over the greater part of the earth, and are sometimes so powerful that the earth at Ipswich has been found to be + or − to the earth in London to the extent of 140 cells Daniel's battery. These currents do not change suddenly like electric signals, but go *gradually* over from positive to negative. The speaker had constructed an apparatus (*vide* Fig. 15) for imitating these currents. An annular trough containing sulphate of zinc solution was connected at opposite points to the earth and to the cable (through the telegraph key). A fly-clock rotated the crossbar as shown by the arrow in the diagram. From this crossbar depended two pieces of amalgamated zinc connected with the two poles of the battery, when the latter were at right angles to the line connecting the cable with the earth, *viz.* as in the figure, no current flowed into the cable. When this crossbar had moved 90° from the position shown in the diagram, a positive current was flowing into the cable; on its moving 180° more the current had changed sign and reached its maximum negative. This apparatus performed the half revolution in 40 seconds. The speaker stated that he had never seen, in all his experience, an earth current change from maximum positive to maximum negative in a shorter period than 60 seconds, and that only once; the change was often spread over an interval varying from five to ten or more minutes.

At the Newfoundland end of the cable, a reflecting galvanometer was placed between the earth and the cable, whilst a second galvanometer was placed, on Mr. Varley's plan, between a condenser and the cable, the other pole of the condenser being connected with the earth. The moment a current began to arrive at the distant end, it was shown upon both galvanometers; but as soon as it had acquired a uniform strength, the cendenser, being charged to that strength, cut off all further electricity, and the second galvanometer returned to zero, while the first one remained deflected. The amount that the second galvanometer was deflected did not depend at all upon the amount of current passing through the cable, but simply upon the rate at which its potential varied, and according to which the condenser charged or discharged itself. The earth currents sent the ordinary galvanometer image running from right to left twenty or thirty feet, right off the screen in fact, but the other one simply moved three inches, because the rate of variation of potential being slow the condenser charged slowly.

The effect of the condenser on the latter galvanometer is at the first moment to offer no resistance to the passage of the electric current. Suppose a current of a force 1 to arrive at the distant end, the current splits, part running through the galvanometer D to the condenser, the other through the galvanometer or resistance B to the earth.

As soon as the condenser becomes charged to the power 1 the current in D ceases, if now the current increase from 1 to 2 the condenser E will become charged to 2, and while charging the galvanometer D will indicate the presence of a current; but as soon as the condenser is charged with the same power as the cable, the current in D ceases. Thus then the galvanometer and condenser D, E do not measure the strength of the current flowing through the cable, but simply indicates alterations of the potential.

Suppose the cable and condenser charged to a potential of 100, D would show no current; if anything suddenly augmented this potential to 101 the condenser would be charged to 101, and the variation of potential would be indicated. Suppose now that the charge were decreased from 100 to 99 the potential of the condenser would be reduced to 99 by discharging itself into the cable, producing a current in the opposite direction of a power of 1.

Thus then the strength of the current in D is entirely dependent upon the increment or decrement of potential, and not upon the strength of the current flowing through the cable. Suppose now an earth current of a power of 100 to pass from + to − in 60 seconds. A signal through the Atlantic cable is produced in about a quarter of a second.

If the strength of the signal be but $\frac{1}{10}$ of that of the earth current, yet as the rate of variation is 240 times greater, the signal while it lasts will be 24 times as strong as the earth current, and so the weak rapid current produces a signal, while the strong, by slowly changing earth currents, produces one too feeble to interfere.

The same disposition of apparatus greatly accelerates the speed of signalling, and affords a near approach to the rapidity attained by the curb key; this contrivance, however, can be used with advantage in conjunction with the curb key.

The speaker demonstrated this by sending signals through the artificial Atlantic cable, from a very much weaker battery than that used to produce the earth-currents; however, *the rate of augmentation of potential* of these small signal waves being much greater than the rate of variation of potential of the great earth current wave, sharp clear signals were produced upon the second galvanometer. So by this extremely simple contrivance the small ripples upon the back of the big earth current wave were, practically speaking, entirely detached, and clear signals produced, which despised altogether the great swell which had rendered the first instrument altogether useless. A number of words were spelt through the cable to illustrate the fact.

In concluding, the speaker remarked that it was upon the data furnished by this artificial cable that he designed the present Atlantic cables, and that without it he could not then have guaranteed eight words per minute without a core whose conductor and insulator were each 60 per cent. greater than the present, which consisted of 300lb. of copper and 400lb. of gutta percha to the mile; and he added that it was at least *some* reward for the years of arduous labour he had had in connection with this great enterprise to find that everything he had predicted as to the capabilities of the cables had been entirely verified.

[Samples of the 1865 and 1866 cables were exhibited, also a piece of the cable which had been for more than a year two miles below the surface of the Atlantic Ocean.]

The speaker gratefully acknowledged the kindness of Dr. Tyndall, who had provided him with the electric light and his assistant to manage it.

[C. F. V.]

WEEKLY EVENING MEETING,

Friday, February 22, 1867.

Sir Henry Holland, Bart. M.D. D.C.L. F.R.S. President, in the Chair.

Moncure D. Conway, Esq.

On New England.

The men and women who left England in the dawn of the seventeenth century and landed on Plymouth Rock represented the filtered strength of the English people. It was because they feared they would in Holland, where they first sought refuge from persecution, lose their English name and speech, that they resolved to seek the shores of America, where their wild home was rightly named New England. They were 102 in number—28 women—who landed at Plymouth in 1620, and there was not a feeble heart nor a blockhead among them. Their wildly-cradled infant colony was born on Christmas Day, but they toiled through every hour of it, though the bitter cold could not induce them to work on their cabins the previous day, it being Sunday. The first years of the colony were lowly enough, the hand to hand struggle with nature for subsistence; but from this was evolved national character.

The section in which the pilgrims settled has a distinctive physical character. After describing at considerable length the features of the country and its great beauties, the speaker referred to the Indians, of whom the pilgrims found about 50,000 in New England when they landed. The first impression of the Indian is attractive, on account of his fine physical appearance; but the near view reveals his repulsive characteristics. The power of his senses has not been exaggerated.

He is, however, a brainless savage, by no means the right figure for a central position in America. The Puritans began by being scrupulously just to the Indians; but the Indian could not be made to understand an English bargain. He thought that a thing reverted to him when what he had received for it was exhausted. This led to the Indian wars. But the Indians were hardly so heavy a trial to the Puritans as the power from which they had fled in England, but which found them out so soon as they had any success. The king gave their lands to his favourites, and they had to share whatever they could wring from their hard soil; and even this was not so hard on them as the effort made to crush their religion; the noblemen thinking it a disgrace that their American estates should be worked by a party of vulgar "separatists." Neither they nor their religion was crushed, however. At the end of twenty years there had been drawn thither 20,000 people, and they were building good ships, and had a good trade. They were distributed into the settlements which were the germs of the six New England States by natural forces. The barrenness and the fine water-power of Massachusetts—the mother state—decided that it should be a manufacturing state; these parties went out to the grazing uplands of New Hampshire and Vermont, and to the meadows of Connecticut, to get wool and grain for those mills of Massachusetts. Rhode Island was planted by Roger Williams, an eloquent Oxonian, who fled from the persecutions of Laud, and was too much of a Radical for the Puritans to tolerate. The State of Maine was settled through the free trade that was opened by the French of *Nouvelle France* in the north with the Boston colony; the first of the long column of benefactions on which rests what is called the traditional friendship between America and France. The hostility of the Indians made every inland migration a costly one.

In 1643 the various settlements formed the first American Confederation. It was partly a mutually defensive union, but mainly a religious one. The body which represented it was a Council in which each settlement, however large or small, had two members; and in this respect it was the original senate. There was no President, but only a Moderator chosen by the Council. They adopted the Mosaic code as their constitution; but it is not true, as is generally supposed, that their laws were unusually rigorous. Whilst England and Scotland had as yet thirty offences punishable with death, New England had only ten. It was in small affairs, and in the interference with private conduct, that Puritan rigour was felt chiefly, although they were guilty of cruelties in the instances of the Quakers and other heretics. However, those "heretics" were never punished for *heresy* with anything beyond banishment from the colony; the scourges, and the four memorable executions of Quakers, were punishments for the defiance of magistrates involved in repeated returns, when death had been named as the penalty for the final return. At this time New England was a religious aristocracy. The preachers were also the magistrates; only Church members could vote. This had hardened into a despotic regime. But a silent power—the free school—had even at that early period been set in motion, which, when the persecution of the witches came on, was shown to have undermined the clerical power. The people released the condemned witches, and swept out of pulpits and from benches the judges and preachers concerned in their prosecution. Amid this revulsion at the close of the seventeenth century, the authority of the clergy in New England closed, and the era of religious liberty began.

The period stretching from the year in which the last witch was executed—1693—to the Revolution of Independence was one of great intellectual and moral growth. Under the masterly neglect of the Duke of Newcastle, to whom the administration of that region was so long committed, the New World matured its powers, and the colonies silently became Republics. The Revolution was but the publication of a fact. It was a victory in which both sides won, and decided that the New World is not to be a duplicate of the Old.

Independence came upon New England like the breath of a tropic. Its industry had always been great. The cultivable land is only about the size of England, but its products for 1866 may be represented by 250,000,000*l.* The single state of Massachusetts is a flint not larger than three English counties, yet out of it came last year 100,000,000*l.*, which is thrice as much as in 1845. The taxable property of that state, apart from all public institutions, is over a thousand millions of dollars.

One-third of the American people are of New England descent. Another third is of other English settlements. It is undeniable that the races of the old world have been modified in the new, and the English more than others. "The full and florid habit," says Dr. Palfrey, "the moist skin, the curly hair, and sanguine temperament so general in Great Britain, have in America been replaced by a comparatively slender form, dry skin, straight hair, and bilious or nervous temperament." With more flesh than the American, the Englishman has not quite so large a skeleton. Agassiz has admitted this fact, though it is unfriendly to his theories. Haller gave the average height of the European man as 5 feet 5 inches; the measurements of New England soldiers in the late war show them to be 5 feet 8 inches. The New Englander reaches his maximum of size later in life than the European. There is doubtless a slight infusion of the Indian characteristics in the American. These traits are possibly physiognomical. In religion, in education, the skeleton or type is large, though not yet completely filled out. With less scholarship than Europe, America has a better school-system. New England may be called a large university. Her system of education has been filtered through 230 years, and is so complete that it is impossible to find a healthy native of the country uneducated. In coming in contact with the people fresh from work and reality the colleges have lost much of their monasticism, and science has become paramount in them. This enthusiasm for scientific studies has influenced literature.

Nearly all the men of letters in America are of New England. Forty years ago American thought began to cease to be the refrain of English thought, and to assume those distinctive characters which are best represented in transcendentalism. The transcendental movement has revolutionized the beliefs and politics and institutions of America more than anything else. It was a kind of spiritual Positivism. When Emerson—whom the future will call the father of his country rather than Washington, whose birthday America is now (Feb. 22) celebrating—first held up his torch, the old imported order still stood in mountains of ice around him; but his torch was multiplied, conflagrations came of it, and the hills melted. Something like a deluge has followed; but those who shall see the solid land again, shall find that this was only the needed irrigation, before it should put forth faiths fresh as prairie grass, and states stabler than mountains.

[M. D. C.]

WEEKLY EVENING MEETING,

Friday, March 1, 1867.

Sir Henry Holland, Bart. M.D. D.C.L. F.R.S. President,
in the Chair.

Captain V. D. Majendie, R.A.

ASSISTANT SUPERINTENDENT, ROYAL LABORATORY.

On Military Breech-Loading Small Arms.

Those who believe that breech-loading rifles formed no part of the equipment of the British army until the present or the past year—that they sprang directly out of the performances of the needle-gun in the late Bohemian campaign, will learn with surprise that not only had breech-loading been determined upon for our infantry nearly three years ago, and the actual pattern of arm that is now in the hands of a large proportion of our troops decided upon a full month before Königgratz was fought, but that two regiments of English cavalry were provided with a breech-loading carbine ten years ago. If we examine one of these Sharp's carbines, as they are called, we notice at once an objectionable feature, *viz.* that the ignition is effected by means of a percussion cap. Then again, the arm is provided with no *effective* arrangement for checking the escape of gas. So great is this escape, that a handkerchief laid over the breech at the moment of firing will be burnt through and through; whereas with a close-fitting arm or cartridge the handkerchief should not be even soiled by the discharge. This defect is aggravated by the quantity of powder, which in damp weather adheres outside the breech after loading. The back end of the cartridge is cut off thus, and the powder sprinkled about. From these two causes, the firer's face is generally flecked and burnt in firing; while the escape of gas tends also to foul and clog the breech action, which exposes large friction surfaces, and to render the arm more difficult to open as each round is fired. For these reasons: the retention of the percussion cap, the liability to an escape of gas, the flash occasioned also by the spilt powder, and the difficulties which often arise in loading, this system is extremely imperfect. And yet this system did undoubtedly reflect to some extent the state of feeling which prevailed in this country on the subject of military breech-loading arms a few years ago. For at that time this question was regarded exclusively as a cavalry question, and quick shooting was scarcely thought desirable for a cavalry soldier. . . . .

Between 1857 and 1861 three other breech-loaders were introduced for experimental cavalry use. One of these, the Green's carbine, even in that easy age, never obtained a footing as a recognized service arm. But the Terry's carbine, which I have here, was introduced to some extent, and is not yet entirely obsolete; and the Westley Richards' carbine has found justly more favour. In 1861 this arm was definitively adopted. It is a great improvement on the others which I have named. It is an accurate arm; it fires six or seven rounds a minute with comparative ease and certainty; it spills no powder; it does not burn the firer's face or facings. Until the recent competition, Westley Richards' was accepted as the best type of military breech-loading arm known in this country, and, with its many objections, I would still pronounce it one of the most effective capping breech-loaders which has been produced.

But we have now to consider the subject in a broader light. It had been treated hitherto from one narrow point of view—as almost exclusively a cavalry question. We had considered only arms of which the percussion cap formed a material element, and a capping arm may be pronounced, with our present knowledge and for our present purpose, only half a breech-loader. Thus far, then, we may justly say that breech-loading had been cramped within contracted limits and misunderstood. But a couple of years or so ago the whole aspect of the case altered. As by a revelation we learnt *then* what breech-loading really meant, and of what development the system was capable. It was a great epoch for breech-loading, its Hegira, it might be called, when it was discovered that the objections to cartridges containing within themselves the means of ignition had in reality no force. If we are to trace the stagnation which had hung over the question to a definite cause, we may confidently place our finger upon these objections.

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Between 1857 and 1861 three other breech-loaders were introduced for experimental cavalry use. One of these, the Green's carbine, even in that easy age, never obtained a footing as a recognized service arm. But the Terry's carbine, which I have here, was introduced to some extent, and is not yet entirely obsolete; and the Westley Richards' carbine has found justly more favour. In 1861 this arm was definitively adopted. It is a great improvement on the others which I have named. It is an accurate arm; it fires six or seven rounds a minute with comparative ease and certainty; it spills no powder; it does not burn the firer's face or facings. Until the recent competition, Westley Richards' was accepted as the best type of military breech-loading arm known in this country, and, with its many objections, I would still pronounce it one of the most effective capping breech-loaders which has been produced.

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When men said, and the ablest and most experienced military men did say, and our military authorities laid it down as a fundamental axiom, that cartridges containing their own ignition were not admissible for military use, what they meant was: 1st, That such cartridges were more liable than others to accidental explosion; and 2nd, That in the event of the explosion of one cartridge, the contents of the barrel were liable to be all exploded *en masse*, and so to communicate from barrel to barrel. If we grant these premises, the conclusion is just. Ammunition which is liable to explode in bulk, and thus not only to commit injury and dangerous havoc for the moment, but by its explosion to deprive the troops dependent upon it of their supplies, is clearly not admissible for military use. But when, mainly, I believe, by reason of the explosion of gunpowder at Erith two years and a half ago, men's minds were directed towards explosions generally, an experiment was made to determine the liability of small-arm cartridges — not breech-loading cartridges in this case, but the ordinary muzzle-loading Enfield rifle cartridges, ball and blank—to explode in bulk, and it was established not only that the explosion of a single cartridge in a barrel was not communicated to the rest, but that the explosion of a number of cartridges, or even of ½ lb. of loose powder, although it might burst open and destroy the barrel, would not occasion a general explosion. Although the bearing of this fact upon the question of breech-loading was not immediately perceived, it furnished the opening through which presently a flood of strong new light rushed in upon the subject.

This effect proceeds from the same cause as that which operates in rendering the well-known Gale gunpowder inexplosive. Mr. Gale enclosed each grain of his powder in an incombustible envelope of finely-powdered glass or bone-dust.* In a barrel of cartridges we have not each grain enveloped, but a number of grains, so many as compose one charge, in a non-combustible case. I have tried over and over again to explode a barrel of the service breech-loading cartridges without success. I have several times fired one cartridge in a barrel without igniting the remainder. I have fired ten cartridges at once, and no more have fired. I have gone further, and placed the barrel inside an iron cylinder, tightly screwed down, and have exploded ½lb. of powder in the midst of the 700 cartridges which it contained; and although the screws were broken and the lid of the cylinder was blown off with violence, and some of the cartridges were strangely distorted, none of the cartridges were ignited. Experiments not less exhaustive have established also the non-liability of these cartridges to separate accidental ignition, by concussion or otherwise. In this way we dispose of the objections which have been entertained against cartridges containing their own ignition, and establish their admissibility for military service. . . . . .

Between 1859 and 1864, no less than twenty-six different plans of breech-loading were proposed. For sporting purposes, too, breech-loading guns had found their way into very general use, and in America they were being largely applied experimentally for military service. But always this shadow hung over the question; that cartridges containing their own ignition were not generally considered admissible for military use. And as breech-loaders not adapted for such ammunition presented comparatively few advantages except for cavalry, for whom as I have explained quick firing was not specially desired, and as the development of the system was thus stunted, the question was in fact argued in a circle, and the subject remained up to this period practically at a stand-still.

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* The Gale process for rendering gunpowder non-explosive by mixing it with four parts of powdered glass, was described and illustrated by experiments. See Professor Abel's lecture on gunpowder, R. I. Proceedings, vol. iv. p. 618.

But when once this fatal restriction and limitation were removed—we began at once to make real progress in the matter. It was no longer a question merely of an arm which presented certain minor facilities of manipulation—of an arm somewhat handier to load on horseback—but it became obviously a question of an arm, which would multiply the fire of an army three or fourfold—which, properly considered, would place as it were three or four rifles in each soldier's hands.

In the course of the Dano-German war the value of the famous needle-gun, or rather of the system of which the needle-gun was an indifferent exponent, became in the eyes of observant men fully established. So obvious was the teaching of this campaign, that our then Secretary of State for War, Lord De Grey, appointed forthwith a committee, with General Russell as president, to "report upon the advisability of arming the infantry, either in whole or in part, with breech-loaders." After four meetings this committee reported, abstractedly and without reference to any particular system, that it would be desirable to arm the whole of the infantry with breech-loading rifles; and on the day on which this report was drawn up, the 11th July, 1864, the death-warrant of muzzle-loading rifles for the use of the British soldiers may be regarded as having been signed.

The next question to be considered was, how to give effect to the recommendations of the committee. This question admitted of consideration in two ways: either by itself with a view to ascertain the speediest and cheapest mode of placing a breech-loader in the hands of the troops, or in combination with other questions connected with rifles, such as the best size of the bore and the best mode of igniting the cartridge, and with a view to determine what would be, in all respects, the most perfect arm for the use of the infantry. The first mode of dealing with the question would have the advantage of celerity, the second of completeness. But why not have combined the two, by arming the British army immediately with the needle-gun? Here was a complete system—a good system perhaps, some will say, ready-made to our hand; a system too which had had the advantage of being tried on actual service, and which, if economical considerations were to have any weight, had been adopted by a nation very much less able to bear heavy expenditure than ourselves. I have here a needle-gun, which will I think answer this question for itself. Take the gun as it stands, and tell me—those of you at least, and I doubt not there are many who are accustomed to handle the beautiful weapons which England puts into the hands of her soldiers and her volunteers and her sportsmen—tell me if this clumsy rifle is one which you would have cared to see issued to our troops. But why not, you will say, improve the workmanship of the piece—make it lighter, balance it better, alter its bore if need be, or its mode of rifling, improve its mechanism, and when you have suited it to the more advanced requirements of this country, adopt it?

Need I point out to you that such a measure would have disposed of the first argument for the adoption of the arm. We are no longer in this case adopting a system ready made to our hands. We are in fact creating one. We are adopting a breech mechanism, merely one element of a system, nothing more. And but little knowledge of the subject was needed to instruct us as to the extremely defective nature or principle of this breech mechanism. If I handle the needle-gun you will see that it is comparatively slow. It is clumsy and imperfect, in other ways. The needle with which the cartridge is ignited is very liable to become bent or injured. I shall be reminded that these needles are easily replaced, that each Prussian soldier carries two or three. But this injury and replacing of a needle temporarily disables the arm, and constitutes, it must be admitted, an objection. Then again, the gas-check is not permanently reliable. The whole escape is thrown upon the arm, at the junction of the breech, none upon the cartridge; and even if we set out with a tight fit, the fit will become less and less close, and in time, if the arm be not carefully looked to and repaired, an inconvenient escape of gas will occur,—an escape which sometimes induces the Prussian soldier to deliver his fire by preference from the hip. Of the ammunition it will be sufficient to say that it is rude, and for technical reasons ill-adapted for the requirements of military service. The egg-shaped bullet is embedded in a small papier-maché wad, which serves the double purpose of rifling the bullet, which never touches the grooves, and of containing the fulminate, which the needle, penetrating the powder and the thin paper envelope which encloses the whole, has to pierce. In short the needle-gun, however superior even to a *good* muzzle-loader, was not such an arm as we should have been justified in adopting, except in an emergency more pressing than that which now presented itself.

Of the other systems of which we had any experience, and which you have seen, there was none which seemed calculated to satisfy our requirements, and their adoption was accordingly not entertained.

Under these circumstances, having no complete system to begin upon, we determined to take so much of a known and reliable system as we approved, and apply it. By this course we should get, as it were, a lift upon our road, and start somewhat in advance of the point from which we must set out, if we elected to consider the question *ab ovo.* Therefore, and without prejudicing the ultimate and more leisurely investigation which the question as a whole demanded, we resolved to take so much of the existing arm—the Enfield rifle—as seemed to us good, and to revolutionize that part of it which we deemed bad. We determined, in short, to convert our Enfield rifles into breech-loaders. In speaking of the Enfield rifle as a good arm, I am anxious not to be misunderstood. It is an arm doubtless with many defects, if we judge it by the rigid standard of more modern requirements. A steel barrel, for example, would probably be preferable to a wrought-iron barrel; the Enfield twist is undoubtedly too slow for extreme accuracy; and the calibre may be considered as unnecessarily large. The refinements and progress of gunmaking have left the Enfield rifle to a certain extent behindhand, just as the refinements and progress of breech-loaders have left the needle-gun behind; and yet in the main it is an excellent weapon for military use, and I know no power whose soldiers possess a muzzle-loading rifle which can compare with it. We do not, recollect, require a match-rifle for military purposes. Except, on a few rare occasions, such an arm would have no special value. It was Lord Palmerston, I think, who pointed out that what the soldier is required to do is, not to hit a particular button upon his enemy's coat, but generally to drive home an effective fire into an opposing body of infantry or cavalry. . . . . Instructed then by the sober teaching of actual warfare, we were justified, I think, in assuming that if to this Enfield rifle, without subtracting from its accuracy and other qualities, we could apply an effective breech-loading arrangement, we should be at once relieved of all pressing anxiety, and in a position to select leisurely a new and superior breech-loader for future manufacture. Moreover, there were more than half-a-million of these muskets available for conversion; and the step which was proposed had the secondary, but by no means inconsiderable, advantage of economy. It had other subordinate advantages, such as the experience in breech-loading equipment to be derived in the course of the inquiry, and which might stand us in good stead when the more complete and difficult question should come before us, and finally the avoidance of the danger of hurrying without due consideration into the adoption of a possibly unsatisfactory arm.

In August, 1864, an advertisement was issued, inviting gunmakers and others to submit propositions for the conversion of the Enfield rifle. From the fifty systems which were sent in in reply to this advertisement, those which were obviously unsuitable were eliminated, and eight systems were selected for trial, of which five only, for reasons which it is unnecessary for me to detail, ultimately came to the post. Of these, four were capping arms, one only a non-capping arm. The selection for trial of these capping arms indicates that we were not then quite as advanced or decided upon this point as we are now, and had not yet absolutely learned to regard non-capping as a *sina quâ non.* So Westley Richards', Mont-Storm's, Wilson's, and

Green's converted Enfield rifles entered the lists against the solitary representative of the non-capping system—the Snider rifle.

Westley Richards' converted Enfield was substantially the same in respect to its breech mechanism as the cavalry carbine which I have shown you, with the addition of a small hook at the end of the plunger for withdrawing the wad after firing.

In Wilson's arm the breech of the original rifle is removed, and the barrel prolonged for some inches in the form of an open slot. The cartridge is inserted here and pushed forward into the barrel, and is followed by a sliding plunger, which is fixed after loading by a stout bolt which passes through stock and plunger. There is an india-rubber ring to diminish the escape of gas.

The Green rifle resembled the Wilson, except in the manner of securing the plunger, which is furnished with a small knob, and is turned round after loading a quarter circle, and so locked.

The Montgomery Storm or Mont-Storm arm is one of that class of breech-loaders known as "chamber-loaders." A chamber-loader is in fact a sort of muzzle-loader cut short, or so arranged that the arm can be conveniently loaded by hand, without the assistance of a ramrod. The charge is deposited in a short chamber, instead of being rammed all the way down a long barrel; and the chamber is then replaced in the position for firing in the prolongation of the barrel. Of breech-loaders on this plan there are many modifications. In this arm the hinge is in front of the chamber; in many rifles (the Swedish rifle for example) it is behind, and that would probably be the most primitive form of chamber-loading. Colt and Deane and Adam's revolvers are examples of chamber-loading arms having several chambers. In these arms, the chambers are not hinged but revolve, and are brought in succession into prolongation of the barrel. I have several other examples of chamber-loading arms here, to which I would invite your attention after this discourse. The chamber of the Storm rifle can be turned completely over the breech, and loaded; or it may be swivelled at right angles to the barrel for loading with loose powder. When the chamber is returned it is secured by a bolt worked by the lock, and an escape is prevented by an expanding ring or thimble on the pan of the chamber.

In the first arms made on the Snider system, about two inches of the upper side of the breech end of the barrel were cut away, leaving a wide open slot or breech for the admission of the cartridge. When the cartridge had been pushed forward into a taper chamber formed by enlarging what was now the hind part of the barrel, the slot was closed by a lump of steel, hinged on the right side of the barrel, and forming a false breech. It was afterwards found more convenient to remove the back part of the barrel bodily, and to replace it with a "shoe," in which the whole of the breech arrangement was comprised; other modifications followed, until we got at last the more perfect arm, which I hold in my hand. The breech-block, you will see, hinges upon this pin and works thus. It is kept in its place by a small spring stud; and the fit of the block tends also to hold it. In order to meet a possible objection that this arrangement is liable to wear and get out of order, I have brought a "shoe" which has never even been hardened, but which has fired at least 30,000 rounds, and still remains, as you will see if you care to inspect it presently, perfectly serviceable.

The ignition is effected by means of a small piston or striker, which passes through the breech-block, and which, when in repose, is flush with the face of the block. A blow of the hammer causes it to dart forward about a tenth of an inch into the cap, which is fixed, as I shall presently more particularly explain, in the base of the cartridge. The piston is returned by a spiral spring. To withdraw the empty cartridge case, a claw or extractor forms part of the breech-block. When I withdraw the block, the empty cartridge is necessarily drawn with it, and by canting the rifle sideways the case is thrown out. The extractor is returned by another spiral spring. With regard to this spring, to which objection has been taken, I would point out that it is negative, not positive, in its action. The spring in the needle-gun is positive, and the action depends upon it. In this gun, the spring is an auxiliary merely, not an essential. We can do without the spring. It is better to have it; but if we have it not, if it becomes damaged or inert, the extractor can be pushed back by hand. And lastly, the spring in this case is never repressed to a greater extent than about one-tenth of an inch.

It has been stated lately that the Snider system was not really invented by Mr. Snider at all, but by M. François Eugene Schneider; or to go further back, by Mr. John Poad Drake, a Cornishman. But if we are really to trace the system to its source, we must go back to a time which places the invention quite beyond the reach of living men. I have thus far not ventured on the archæology of breech-loading; but if you will permit me, I will make one dive into antiquity for the purpose of bringing under your notice two breech-loading firearms of the reign of Henry VIII., on the Snider system! By the kindness of Admiral Caffin, these interesting arms have been lent to me from the museum of the Tower, for the purpose of exhibition this evening.

In the course of the competition the Snider gun proved about 50 per cent. quicker than its rivals; it was stronger too; it was simple, and apparently durable, the breech arrangement being well adapted to sustain the shock of any number of discharges, from the fact of those shocks being sensible only in a direction at right angles to that in which a force must be exerted to open the breech; and lastly—I should perhaps have placed this advantage first, not last, —it was adapted for a cartridge containing its own ignition.

Among the capping arms, the Mont-Storm rifle was ranked first, and was recommended for experimental application to a certain number of rifles. The system failed subsequently at proof, and the recommendation was cancelled. It failed partly on account of the unsuitability of the skin cartridge which formed part of the system; and this reminds me of another objection to which all capping arms are open. Such arms need a cartridge so thin, that the fire from the cap shall pierce it, and at the same time the cartridge must be so entirely consumed or carried out by the discharge, as to leave no residue to endanger or interfere with loading. These requirements make it difficult to satisfy another not less important point in a military cartridge, viz.: that it shall be strong enough to stand the knocking about to which it will inevitably be exposed in transport and on service. Moreover, a thin cartridge is evidently less well adapted than a stout one to resist the effects of an accidental adjacent explosion. But the Mont-Storm rifle failed also in the arm itself. Under proof charges, the hinge and small bolt by which the chamber is locked were broken.

The Snider rifle, while satisfying many requirements, failed in one important respect. It was so inaccurate as to be quite unsuitable for adoption as it stood. . . . .

As the fault could not lie in the barrel, which the accuracy obtained under the other systems showed could be converted without detriment in this respect, it was due obviously to the ammunition which Mr. Snider had submitted. At this point then, the question of providing more suitable ammunition for the arm was referred to Colonel Boxer, by whom, after a year's experiments, the present service ammunition was designed. Discarding the papier-maché case submitted by Mr. Snider, the objections to which, especially in damp weather, are well known, Colonel Boxer made the case of his cartridge of very thin sheet brass, ·003° thick. The cartridge has a little over two turns of this brass.

Five important advantages result from the employment of this case:—1st. Being uncoiled slightly by the explosion, instead of depending upon the mere stretch of the material, the case can be used in a chamber considerably larger than itself, with little danger of breakage and consequent escape of gas. 2nd, It is not liable to swell with damp, and so to interfere either with loading or withdrawal. 3rd, The difference between the size of the case and of the chamber is so considerable, as to permit of loading even when the case has become considerably enlarged or disfigured by rough usage. Nor under these circumstances again is there any danger of leakage or escape. 4th, Such a cartridge may be made, like the present service cartridges, practically waterproof. 5th, The reaction, or tendency of the case to recoil, which arises after the pressure of gas is removed, tends to render a case of this sort easier of extraction than any other. All of these advantages hold good against a papier-maché case; many of them hold good also against a case of simple copper or other metal.

. . . . .

The last point to be noticed in the ammunition for the Snider rifle is the bullet. On this the accuracy of fire of the arm depends. The bullet is not made, as in the majority of breech-loading arms, slightly larger than the bore, but depends for its action upon the system of expansion which Colonel Minié was I believe the first practically to adopt, viz. a hollow on the base, together with a plug, by which the original Minié iron cap has been superseded. This plug, which is now made of baked clay, plays the double part of expanding and supporting agent. The expansion of the bullet would be effected, it is true, to a great extent by the simple action of the powder-gas upon the sides of the hollow. But a plug makes that expansion more instantaneous and more uniform; and above all it supports the sides of the bullet after expansion. Thus with a plug the passage of the gas is prevented and fouling diminished, in the first place; and in the second, even when fouling has been established, its effect upon the accuracy of a plugged bullet, whose sides do not collapse when they come into contact with the obstructing deposit, will be much less than upon an expanded bullet which has no plug.

Another important feature of the bullet is the wood plug in the head. By this plug we obtain three advantages: greater length, and so a broader bearing and lubricating surface; secondly, the centre of gravity is more favourably adjusted with reference to the requirements of the projectile and the slow twist of the piece; thirdly, the weight is disposed, as in the fly-wheel, away from the axis of rotation. . . . .

Round the bullet are disposed grooves or cannelures, which serve to carry the bees' wax lubricant, by which means a layer of wax is always interposed between the lead of the bullet and the sides of the bore; and fouling is so completely got rid of that one of the best targets ever made with this arm was made after 1000 rounds had been fired from it without cleaning. This point is one of extreme importance, for the accuracy of a military arm is to be measured by its average accuracy during a long sustained fire, and not by its performances when perfectly clean. The measure of efficiency of a military arm in this, as in other respects, is to be obtained by taking the arm, not at its best, but at its worst. In other respects the ammunition satisfied the tests which were imposed. It stood an extraordinary amount of rough usage. It was waterproof to an extent which enabled it to be kept for a whole week in wet sawdust without injury; it was easy of extraction, not liable to escape or explosion; and its expense is very little greater than that of a paper cartridge, even if we take its first cost merely, and immeasurably less if we spread the cost over the periods during which the two ammunitions would respectively remain serviceable—if, indeed, the paper cartridge could ever be considered serviceable. The extreme rapidity of fire in the arm is fifteen shots per minute. Of this ammunition about eight millions of rounds have been issued up to the end of last week, and of the arms in round numbers 100,000. To deal with the false and exaggerated reports which have been circulated respecting the "failure" of the Snider system is a task which I cannot undertake, except in a general way. I can only say that in essence and in substance these reports are false. There have been difficulties of detail, it is true, but fewer and less serious difficulties, I believe, than have ever attended the introduction of any new system of guns or small arms; and whereas the general tenour of the misstatements to which I refer has been that the system has failed, the general tenour of the reports of the troops, in Canada, at Hythe, and at Aldershot, to whom the arms have been issued, has been that the system has proved, on the whole, and specially with reference to the cartridge and the breech action, admirably successful. It may tend, perhaps, to restore confidence, also, if I state that the percentage of failures out of a total of about 50,000 rounds of ammunition, which, in the course of my duty, it has fallen to my lot to fire since the arms were introduced, has amounted to little over one failure in every three hundred cartridges, including every defect, however slight—every miss upon the target at 500 yards, every misfire, every cartridge which has split or failed from any cause. And out of more than 20,000 rounds fired by the Ordnance Select Committee, the percentage of failure has been, I believe, even less. . . . .

Breech-loaders may be divided into two great classes:*—(*a*) breech-loaders, in the ordinary acceptation of the term; and (*b*) repeaters.

The first class may be further subdivided into (1) chamber-loaders, and (2) breech-loaders proper. Of *Chamber-loaders* I have already shown you two examples, the Mont-Storm and the Hägstrom. I have here several others, such as a Spanish arm (Garcia's), Bergstrom's, and another Norwegian system; Leetch's, Mackenzie and Wentworth's, &c.

The more conspicuous defects of this class of arm would seem to be their liability to injury by the explosion, which generally acts directly upon the breech mechanism. On the other hand, they are generally perfectly free from escape of gas, except sometimes at the junction of chamber and barrel. In Sweden and Norway chamber-loaders find more favour than in this country.

Breech-loaders proper include more numerous types of arms. The Snider rifle is an arm of this class; so is the Westley Richards, the Terry, the Sharp, the Green, the needle-gun, all of which you have seen. I have here several others, some good, some bad, some celebrated in their way, and some which exhibit the imperfections of which the system is capable. The Amsler-Milbank system (adopted by the Swiss Government for conversion), Joslyn's, Bayliss's, Restell's, Beard's, Bruton's, Prince's, the Starr carbine (now in use in Canada), Fosbery's, Laidley's (adopted in Russia), are all examples of breech-loaders proper. In these arms, as a rule, very much more work is thrown upon the cartridge. Where, as in the needle-gun and the Sharp carbine, the escape is sustained by the gun, and not by the cartridge, the system will generally be open to objection on the score of excessive escape of gas. With secure ammunition, this defect may be entirely got rid of; and to me it seems sounder in principle and more reliable in practice to throw the burden of the escape upon the cartridge, which only has to sustain it once, than upon the arm, which must sustain it many hundreds or thousands of times.

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* A number of characteristic specimens of British and foreign breech-loaders were exhibited. Their special peculiarities were referred to in the discourse, and discussed in the library afterwards.

(*b*) *Repeaters* may be subdivided into (1) revolvers, and (2) magazine arms. Of *Revolvers* the best examples are furnished by revolving pistols, of which, by the addition of a long stock and barrel, rifles have sometimes been made. I have here also a revolving carbine, by a Colonel Porter. The system is open to many objections, among which, except for pistols, the weight of the several chambers is conspicuous.

Of *Magazine arms* there are two important varieties: The simple repeater, such as Henry's, in which the cartridges are constantly drawn from a magazine under the barrel, which must be replenished from time to time as it becomes exhausted; and secondly, an arm which, like the Spenser and Lamson rifles, can be used as repeaters or simple breech-loaders at will. In these arms I can, if I wish, shut off the magazine, and load them as ordinary breech-loaders. This plan presents several important advantages over the ordinary repeater, which entails distinct intervals of inefficiency while the magazine is being replenished, and which directly tempts the soldier to indulge in an excessive rapidity of fire while his magazine supply lasts. But in the improved repeaters these objections are obviated. I have here, for example, the Lamson rifle, with its magazine full. I need not, however, call upon the magazine, but I may load as if the arm were an ordinary breech-loader. Then when pressed I can, so to speak, turn on the tap of the magazine, and pour forth such a fire as no simple breech-loader can deliver. When my magazine is exhausted, I am not, as with an ordinary repeater, temporarily disabled; for I can fall back upon the simple breech-loading action, until an opportunity presents itself for replenishing the magazine against another emergency. So with the Spenser rifle, in which, as you will see from this diagram, the magazine is situated in the stock.

The direction in which repeaters generally err is in complexity of construction; but if this defect can be overcome, a magazine-rifle would present immense advantages over the simple breech-loader, not merely for those services, such as the navy, the cavalry, and artillery, in which an intensely rapid fire is generally required for a few decisive moments, but for the universal equipment of troops. It is in arms of this class that breech-loading tends towards its highest development; and to this principle of action I believe we must look for the complete and ultimate solution of the breech-loading question. . . . .

The advantages which breech-loading presents in a military rifle are—

First, *Rapidity of Fire.* We give each soldier, so to speak, and as I have before expressed it, three or four rifles, with the inconveniences only of one. At close quarters no troops, however brave, devoted, or disciplined, could stand with muzzle-loaders against a corresponding force armed with breech-loaders. It amounts to being opposed to a force whose numbers are practically multiplied by the figure which expresses the ratio of rapidity of fire of the breech-loaders to the muzzle-loaders. We must not press this argument too far. It will hold generally when the fighting is quick and close and decisive, and when the conditions of the contest on both sides are the same. But breech-loaders will not do everything; and we must avoid the error of supposing that they did everything in the Danish or Bohemian campaigns. The needle-gun was but the embodiment of that spirit of "*geist*" and progress which animated the Prussian army and its leaders, which dictated the execution of their rapid movements, and which was the soul and essence of their superior organization. Especially must we avoid the too close application of mere abstract reasoning when the element of artillery fire comes into play. Eleven years ago an experiment was made at Hythe with life-size dummy figures of men and horses, which went to prove conclusively that artillery would be beaten off the field by infantry armed with rifles. This drawing shows what was then proved, that a detachment coming into action would be annihilated in three minutes by thirty file of riflemen. But two important considerations were overlooked: that in actual warfare infantry would scarcely deliver so effective a fire as then served to plant at 800 yards' range thirty-four shots in a single gun detachment,—just as two men standing opposite one another at twenty paces to fight a duel often fail to hit one another, while at a very much greater distance they can each easily break a bottle's neck in a pistol gallery. And, secondly, the necessity for artillery coming within this range at all was not established; so that the reasoning which was based upon these premises fell in practice to the ground; and notwithstanding improved musketry instruction and improvements in the arms, the use and importance of artillery have in nowise diminished since the introduction of the rifle for general service: so those also would be mistaken who might argue from this diagram that if so much could be done with muzzle-loading rifles, firing as as they did on this occasion one round per minute only, breech-loaders firing seven or eight rounds per minute would produce a corresponding effect, and that the predominance of artillery fire in an action must henceforth cease to exist. But within reasonable limits, the effects of breech-loaders, as opposed to muzzle-loaders, can hardly be over-estimated. It must be remembered too, that there is always attendant upon the employment of a more effective arm a moral as well as a physical effect. As Marshal Marmont said, a battle is decided after all not by the number of men killed, but by the number frightened.

By a converse application of this argument, we reach the second great advantage of breech-loading, *increased confidence*, a point upon which I need not dwell, but of which all military men will recognize the importance.

Thirdly, we obtain greater *facility in loading.* On horseback, breech-loading, even of an imperfect kind, is vastly superior mechanically to muzzle-loading. For the infantry man, if we reflect a moment, the advantages will appear quite as great. Whatever the soldier's position, whether lying behind some sheltering mound, cramped in a rifle-pit, working in close squares, with his bayonet presented to resist cavalry, or running forward as a skirmisher, he can load a breech-loader as he could never load a muzzle-loader—without exposing himself, without changing his position, without inconvenience or loss of time or effect.

We have, in the fourth place, *improved shooting.* The arm is always loaded in a position which favours the subsequent delivery of a low and effective fire. The eye is never removed from the object; and no part of the powder can be spilt, no part can be lodged in the grooves of the rifle; while the increased confidence of which I have spoken tends to steady men's arms and improve their aim. It has been objected that the rapid firing of the breech-loader will tend to tire and unsteady men's arms, but surely this objection, if it has any force, may be met by the consideration that the operation of loading a breech-loader is very much less fatiguing.

Fifthly, *the possibility of overloading is avoided.* This in the hurry and excitement of action is no uncommon accident. A man loads and as he thinks fires. His cap misses fire, or he even neglects to cap at all. But he does not at once recognize it. He rams down another charge; perhaps another, &c. &c. After one of the American battles several arms were picked up loaded with two charges, others with three; some with four, and a few even with eight!

Among the minor advantages of breech-loaders, I may name the completeness and compactness of the ammunition; the facilities for cleaning and inspecting the arms; the ease with which the drill may be acquired; the diminished danger in loading; and the possibility of the arm being rendered inefficient by the loss of a ramrod.

It has been said that breech-loaders will entail an excessive expenditure of ammunition. But in the first place, we have no grounds for supposing that the ammunition expended will be *wasted*: and a similar argument would have held against the supersession of the old flint-lock by the percussion cap. If the fire be delivered at such ranges that the shots tell, it merely amounts to this, that the work is done with the new system so much quicker and more effectually than with the old; and experience teaches us that the requisite supply can be kept up without difficulty, even in a long hot general action, and that this objection has been much overrated. It is stated indeed that the greater coolness and confidence of the men tends rather to a less expenditure; and the number of rounds fired by any individual Prussian soldier in the late campaign, if figures are to be relied upon, would seem to favour this view. The supply of ammunition, whatever its expenditure, is, however, only a question of organization; its *efficient* expenditure is a matter of instruction. . . .

[V. D. M.]

GENERAL MONTHLY MEETING,

Monday, March 4, 1867.

Sir Henry Holland, Bart. M.D. D.C.L. F.R.S. President, in the Chair.

Jerry Barrett, Esq.
John Brunskill, Esq.
Lord Sackville Cecil.
Frederick James Chester, Esq.
Henry Collinson, Esq.
Charles Newton, Esq.
Algernon Perkins, Esq.
Captain Richard Phelips.
James Rankin, Esq. B.A.
T. A. Rochussen, Esq. M.I.C.E.
George Lake Russell, Esq. M.A.
Edward Sartoris, Esq.
Mrs. William Spottiswoode.
John Williams, Esq.
James Christopher Wilson, Esq.

were *elected* Members of the Royal Institution.

Edward Smith, M.D. F.R.S.

was *admitted* a Member of the Royal Institution.

The special thanks of the Members were returned for the following additions to "The Donation Fund for the Promotion of Experimental Researches."

Thomas Williams Helps, Esq. (2nd Donation) . .	£10 0 0
T. Carrick Moore, Esq. (2nd Donation) . . .	10 0 0

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM
Lords of the Committee of Council on Education—Dr. A. W. Hofmann's Report on Chemical Laboratories at Bonn and Berlin. 4to. 1866.
Ansell, George F. Esq. (the Author)—Treatise on Coining. 8vo. 1862.
Architects, Royal Institute of British—Proceedings, 1867. Part II. Nos. 1, 2. 4to.
Astronomical Society, Royal—Monthly Notices, Vol. XXVII. No. 3. 8vo. 1867.

Basel Naturforschende Gesellschaft—Verhandlungen. Theil IV. Heft 3. 8vo. 1866.
Chemical Society—Journal for Feb. 1867. 8vo.
Davies, J. Seymour, Lieut. R.A. M.R.I. (*the Author*)—The Meteoric Theory of Saturn's Rings. 16to. 1867.
Editors—Artizan for Feb. 1867. 4to.
Athenæum for Feb. 1867. 4to.
British Journal of Photography for Feb. 1867. 4to.
Chemical News for Feb. 1867. 4to.
Engineer for Feb. 1867. fol.
Horological Journal for Feb. 1867. 8vo.
Journal of Gas-Lighting for Feb. 1867. 4to.
Mechanics' Magazine for Feb. 1867. 8vo.
Pharmaceutical Journal for Feb. 1867.
Faraday, Professor, D.C.L. F.R.S.—Abhandlungen der Kön Akademie der Wissenschaften, Berlin: 1865. 4to. 1866.
Franklin Institute—Journal, Nos. 489–493. 8vo. 1866–67.
Geographical Society, Royal—Proceedings, Vol. XI. No. 1. 1867.
Horticultural Society, Royal—Report of Proceedings of International Horticultural Exhibition and Botanical Congress, held in London, May 22-31, 1866. 8vo. 1867.
Lee, Rev. A. T. M.A. (*the Author*)—The Irish Episcopal Succession: the Recent Statements of Mr. Froude and Dr. Brady examined. (K 94) 8vo. 1867.
Linton, Robert P. Esq. M.R.I.—Charts of Ancient and Modern History, by the Rev. H. Linton. (Portfolio 1.)
Lubbock, Sir John, Bart. F.R.S. M.R.I. (*the Author*)—On Bronze Weapons. 8vo. 1866.
Madras Literary Society—Madras Journal. Third Series. Part 2. Oct. 1866. 8vo.
Mailly, M. E. (*the Author*)—Essai sur les Institutions Scientifiques de la Grande-Bretagne. Partie 6. 16to. 1867.
Mechanical Engineers' Institution, Birmingham—Proceedings, August, 1866. Part 2. 8vo.
Photographic Society—Journal, No. 178. 8vo. 1867.
Raumer, Friedrich von, Hon. M.R.I. (*the Author*)—Friedrich von Raumer an Rudolf Köpke. (L 14) 8vo. 1866.
Sonst und Jetzt. (L 14) 8vo. 1867.
Rodwell, G. F. Esq. (*the Author*)—On some Effects produced by a Fluid in Motion. (Phil. Mag. Feb. 1867.)
Royal Society of London—Proceedings, No. 89. 8vo. 1867.
Saxon Society of Sciences, Royal—Abhandlungen. 1866. 2 parts. 8vo.
Berichte. 1865-66. 3 parts. 8vo.
Symons, G. J. Esq. (*the Author*)—British Rainfall. 1866. 8vo. 1867.
Monthly Meteorological Magazine, Jan. and Feb. 1867. 8vo.
Tuke, Thomas H. M.D. F.R.I.—Notice of John Conolly, M.D., by Sir James Clark. (K 94) 8vo. 1866.
Tyndall, Professor, F.R.S.—L. Agassiz, Glacial Phenomena in Maine. (L 14) 8vo. 1867.
Walford, Weston S. Esq. F.S.A. M.R.I.—Alphabetical Dictionary of Coats-of-Arms belonging to Families in Great Britain and Ireland. By John W. Papworth. Parts 1-14. 8vo. 1858–66.
Yearsley, James, Esq. M.R.I. (*the Author*)—On the Exuberant Growth of the Tonsils. (K 94) 8vo. 1866.

Telegraph Construction and Maintenance Company—Specimens of the Atlantic Telegraph Cable, 1866.

WEEKLY EVENING MEETING,

Friday, March 8, 1867.

William Spottiswoode, Esq. M.A. F.R.S. Treasurer and Vice-President, in the Chair.

Rev. W. Greenwell, M.A.

CANON OF DURHAM.

On the Yorkshire Wold Tumuli.

The object of the discourse was to illustrate the early sepulchral remains of the Wolds, a district in the East Riding of Yorkshire singularly compact and self contained. The tract of country is of moderate elevation, with a surface varied by deep valleys and swelling and rounded hills, the geological formation being for the most part the chalk. The valleys are nearly all waterless, and in this and other respects, as well as in the numerous signs it presents of early occupation, the district possesses a marked similarity to the Wiltshire Downs. It is surrounded on all sides by flat alluvial land. Wood was scanty, the prevailing tree being the thorn, here attaining a large size, whilst considerable tracts were covered by furze and heather. This dearth of wood was not favourable to the presence of large quantities of wild animals, which require its shelter for protection, but that was afforded by the coverts which abounded in the neighbouring low-lying and then swampy ground. In the comparative absence of cultivated crops a population must, in a great measure, depend upon the chace for its support, and the Wolds might therefore seem unsuitable for early occupation. They were, however, largely peopled in pre-historic times, as several facts distinctly show. Occupation is evidenced by remains of houses, which cannot, however, lay claim to much architectural skill either as regards convenience or stability. They were only rude wigwams, formed in two ways, by raising a conical roof of wood and turf upon a low circular wall, and by covering in a circular pit, with a similar shaped, or perhaps a flat, roof. Besides these dwelling-places, there remain some gullets still undestroyed by the plough, as well as extensive lines of mounds and ditches, which seem too large to have been intended for defensive purposes, though they might serve the end of preventing cattle being driven off by sudden raids.

The abundance of the people who were settled in the district is shown, perhaps, most clearly by the number of flint implements which are found scattered over the surface of the ground. They are there in thousands—arrow-points, knives, scrapers, and articles of uncertain use. A common material may in some way explain their frequent occurrence. Still whatever cause may have made them so abundant, their number points to a numerous population. Besides objects of flint and other stone, considerable hoards of bronze swords, spear-heads, gouges, and celts have been discovered.

The question who were the people who occupied the Wolds is one of great interest, and a careful and systematic examination of their burial mounds will tend to settle it. So far as present researches show, two distinct races appear to have occupied the district in pre-historic times, and for some part of this period they lived side by side and intermingled, but at an earlier time it seems almost certain that only one of these races was living on the Wolds. They may be called a long-headed (dolicho-cephalic) and a round-headed (brachy-cephalic) people, the long heads being the earlier.

Upon the Wolds and in other parts of Yorkshire, but principally in the south-western counties of England, a peculiar shaped barrow exists, which is very long in proportion to its breadth, as much as three to one. They are, for the most part, placed approximately east and west, and the burials are at the east end, which is broader and higher than the west. With the primary interments in these barrows no implements of metal have ever been found, those which occur being made of flint. There is a remarkable feature in connection with the interments in the long barrows, and which is quite peculiar to them. A large number of bodies is sometimes found placed together, and the condition of the bones of some of them is very peculiar. Whilst one or more of the bodies have the bones unbroken, and laid in their natural order and juxtaposition, the greater part have the various bones scattered about, separated from each other, and broken, and showing by unmistakable signs that this breakage had taken place at the time of death. This seems to show that the flesh must have been removed before the bones were buried; for if that had not been the case, the broken pieces of the same bone would have been lying close together, and not at some distance from each other. These unusual features appear to point to rites which, if abhorrent to our ideas, have nevertheless been prevalent in many countries, nor has cannibalism been so unfrequent that it need be wondered at to find evidence of its having been used in this country. There are historic accounts of the practice in times much later than when these barrows were erected.

In the long barrows in Wiltshire and Gloucestershire the bodies have been buried unburnt; on the Wolds the intention was to burn them, though that had not perfectly succeeded in every case. The way in which the burning had taken place was peculiar. Upon the bodies, whole and broken up, chalk rubble and flint had been placed, over this was piled wood, which had then been fired, and so the bodies were consumed by the ignited lime.

The skulls of the bodies found in the long barrows are dolichocephalic, and they differ in other respects than in length from the brachy-cephalic skull, which is that ordinarily found in the round barrows of the bronze age. They appear to have been neither a tall nor strong race, and were the earliest inhabitants of the Wolds whose sepulchres have been preserved to us, as is shown by their ignorance of metal, which was known to the people of the round barrows. They present some similarity with long-headed tribes in Spain and the North of Africa. But the great rarity of these long barrows on the Wolds point to a very scanty population.

The round barrows, which generally occur on the high ground, are however numerous. They average 50 or 60 ft. in diameter and 4 or 5 in height; yet when we consider the length of time the Wolds were occupied by the brachy-cephalic race, we can hardly suppose that any but the chiefs and their families, and perhaps captives slain at the funeral, were buried in them, even when we take into account that the same mound often serves for several interments. Occasionally we find a mound containing only the body of a child, probably the favourite child of the chief. Many persons of distinction in the tribe were buried separately, without mounds, but covered by large slabs of stone. These graves are not noticed so frequently on the Wolds, on account of the absence of large stones. The ordinary dead were buried together in large numbers, as has been found in making railway cuttings. In these cases no objects appear to have been interred with the bodies, as in the mounds. On the Wiltshire Downs barrows are often surrounded by a ring of earth and stones to protect the interment. This feature has, on the Wolds, been destroyed by cultivation. These circles also surround burials with no mound, and have then been commonly held to be of Druidical origin, but needlessly. It is probable that even Avebury and Stonehenge, which only differ in size from these circles, are connected with sepulchral purposes.

The two modes of burial, cremation and inhumation, prevailed on the Wolds as elsewhere. The latter, owing perhaps to the scarcity of wood, is the more usual practice; but local and family peculiarities, and not differences of rank or sex, seemed to have determined which mode should be preferred, as burnt and unburnt bodies were constantly buried together at the same time.

When the body was buried unburnt, it seems to have been dressed in its ordinary clothing, and always in a contracted position, the knees being drawn up towards the chin; sometimes it was probably swathed like a Peruvian mummy. It was laid on the side. I have never seen it placed in a sitting position. When stone could be procured it was surrounded with a cist; if not, it was simply placed in the material of the barrow or in a grave sunk below the surface, and very rarely in a split and hollowed tree. In the case of cremation, the body was placed on a pile of wood and consumed with various degrees of completeness. The burnt bones are generally removed from the place of burning and enclosed in an urn or small cist, or simply laid on the ground. Secondary interments occur above the primary and round the sides of the barrow, though seldom on the north and west. Various implements are often found close to the bodies. In one barrow, about 50 ft. in diameter, there were found unburnt six grown-up bodies and six children. It may be noticed that the teeth, though much worn by hard food, show little marks of decay.

The pieces of animal bones which frequently occur in barrows are remains of the funeral feast, and were broken to get at the marrow. The occurrence of sherds of pottery and flint chippings, which seem to have been thrown in indiscriminately, is difficult to account for. From a curious passage in 'Hamlet' it appears that "shards, flints, and pebbles" were thrown on the corpse of a suicide. It is possible that this sacred Pagan rite was remembered in Christian times, but associated with what was irreligious and profane, as pious rites in one religion are often accounted accursed in a new one.

Occasionally, the first interment is disturbed by a subsequent one. Probably this only took place when the family or tribe to which the first belonged had become extinct, and their burying-place had not been respected by their successors. Even the bodies contained in the first grave have been sometimes broken in digging a place for the second interment.

Burnt and unburnt bodies are accompanied by the same implements; but this is not the case with the urns, which are of various shapes, and placed in various positions with respect to the body they accompany. They are not sun-baked, but have been exposed to fire. They are formed by hand, not by the wheel, yet they show great symmetry, and the material, though varying with the different localities, is often well tempered, but not glazed.

The numerous variations in the shape of those found with unburnt bodies resolve themselves into two types, to which the names of "food-vessel" and "drinking-cup" have been given. The first class is like a flower-pot, but more globular, often with a series of projecting knobs round the neck, which in the earlier specimens probably was useful for hanging them up. They vary in height from 4 inches to 7. The "drinking-cups" are larger, and of finer clay and more elegant shape, being taller and more swelling in outline. The ornamentation on both kinds of vessel is profuse and varied, consisting mostly of combinations of short lines, made by a twisted thong or by a pointed instrument.

It is probable that, as has been generally believed, these vessels were for the purpose of holding food for the dead. The custom of the American Indians affords an analogy.

Enclosing or accompanying the remains of burnt bodies we find urns of a much larger size—sometimes exceeding 2 feet in height—and coarser material and workmanship. They have an overhanging rim, and the ornamentation is continued but little below it. They are as often found reversed over the bones as upright. With them often occurs a small vessel, like a saltcellar, and not unfrequently pierced, known as an "incense cup." Other types of vessels are also occasionally found, but none contain the burnt bones except the cinerary urn; the others may possibly have contained food. But all the vessels described were, I believe, especially made for sepulchral use, as they are too imperfectly baked to be used as domestic vessels, from which, too, they greatly differ in appearance.

No rule can be laid down as to the occurrence of weapons and implements with bodies, burnt or unburnt. Often none at all are found; at other times several. With the bodies of men occur axes, spear and arrow heads, knives, and scrapers, all of flint, bone pins and jet buttons, bronze daggers, pins, and, very rarely, simple axes; with those of women, stone rubbers or corncrushers, flint knives and scrapers, jet or amber necklaces, buttons, pins, and bronze awls. When cremation has been used, implements were sometimes burnt with the bones; and sometimes placed with the ashes afterwards.

Neither bronze swords nor socketed celts are ever found with interments; indeed any bronze articles are very rare. This does not arise from that metal being unknown, as the round barrows, I believe, are later than its introduction; but it was for long only used for the more important articles; the commoner ones, and those which, like arrow-heads, were thrown away, being still made of flint. The poor had probably no implements of bronze. The scarcity of bronze with interments, then, is explained by its liability to decay, by its being, like gold, too valuable to place with the dead, and by the fact that it was not the material of which such objects were made as were habitually placed in the grave.

It has been generally held that the depositing with the dead the implements, &c., which they used while alive, implied a belief in another state of existence: without disputing this view, it may be observed that the affectionate and pious feeling of the relatives might be enough to account for the custom; and also that the articles found with the bodies were mostly attached to the dress, or else were such weapons, ornaments, &c., as could hardly be used by others without disrespect to the memory of the deceased; while if they were intended for use in a future state, it seems strange that the majority of the dead should be interred without anything of the kind.

From the broken bones found in the barrows we infer that the Bos longifrons, the roe and red deer, the wild swine and the goat were among the animals used for food, but neither the sheep nor the dog has been discovered on the Wolds.

In the round barrows we find both the long-headed race of the long barrows, and the later perfectly distinct round-headed type, which is far the more frequent, besides one which may have sprung from the union of the two. The earlier race, though perhaps conquered, would not be destroyed, and in time must have become one with the intruders, who were a larger and stronger people, and acquainted with the use of bronze. They were harsh and rugged in the face, with all the features prominent, and the mouth and eyebrows projecting. The head was broad, especially behind, with a remarkably flat occiput, a part which in the long-headed race was remarkably prominent.

The long-headed race appears to have affinities with the south, and the round-headed with the north; the latter approach in type of skull to that of the people of the stone age, who buried in the barrows of Denmark, and may have been allied to the Laps. But the question is too wide for this discourse.

The Wold people have been spoken of as living before the Roman conquest; but this view has been opposed by some, especially by Mr. Thomas Wright, who place them at a time when the Romans had left Britain. But from Cæsar and other sources we learn that the offensive arms of the Britons at the time of the Roman invasion were made of iron. Yet no trace of this metal has been found in the Wold barrows. And neither in these, nor in those of the same class in other parts of England, has any article showing the least trace of Roman influence been found. All their weapons, implements, &c., are as different as possible from corresponding ones of Roman manufacture, both in shape, material, and workmanship. Is it possible that several centuries of the imperial rule could have had no influence in this respect? For the Wolds were in the vicinity of flourishing Roman towns.

We can, however, come to only an approximate date for these barrows. But taking all the circumstances into consideration, we cannot place them later than a hundred years before Cæsar's landing, and probably the greater part of them belong to a time very much earlier, when bronze, though known, was scarce. More extended researches will enable us to come to a more certain conclusion. In the meantime it is safer not to lay down any specific date, but to say, what we can with confidence do, that they belong to a time which ends a century or two before the occupation of Britain by the Romans.

[W. G.]

WEEKLY EVENING MEETING,

Friday, March 15, 1867.

Sir Henry Holland, Bart. M.D. D.C.L. F.R.S., President, in the Chair.

Edward Burnet Tylor, Esq.

On Traces of the Early Mental Condition of Man.

If an antiquary is asked his opinion as to the early condition of mankind, he will probably take up the question with reference to an excellent test of man's civilization, the quality of the tools and weapons he uses. He will show how, within our own knowledge, the use of metal instruments has succeeded the use of sharpened stones, or shells, or bones; how the stone axes and arrow-heads found buried in the ground prove that in every great district of the world a Stone Age has prevailed at some more or less remote period; and lastly, how recent geological researches have displayed to us the traces of a Stone Age extraordinarily low and rude in character, and belonging to a time as extraordinarily remote in antiquity. The history of man, as thus told by a study of the implements he has used, is the history of an upward development, not indeed a gradual steady progress of each family or tribe, but a general succession of higher processes to lower ones.

Now there also exists evidence, by means of which it is possible still to trace, in the history of man's mental condition, an upward progress, a succession of higher intellectual processes and opinions to lower ones. This movement has accompanied his progress in the material arts during a long but undefined period of his life upon the earth; and of this evidence, and of the lines of argument that may be drawn through it, the object of the present discourse is to give a few illustrative examples.

I. In the first place, the *art of counting* may be examined from this point of view. We ourselves learnt to count when we were children, by the aid of a series of words, *one*, *two*, *three*, *four*, and so on, which we were taught to associate with certain numbers, 1, 2, 3, 4, and can thus reckon up to the largest imaginable number, and down to the smallest imaginable fraction. But if we look round among other tribes of men we find a very different state of things. As we go lower in the scale of civilization, it becomes easier and easier to puzzle a man with the counting of 20 objects, or even of 10, and to drive him to the use of nature's counting-machine, his fingers. When we reach the low level of the savages of the Brazilian forests or of Australia, we find people to whom 3 or 4 are large numbers. One tribe, described by Mr. Oldfield, reckoned *one*, *two*, and then *bool-tha*, "many;" but when their poor word-language fails them they fall back on gesture-reckoning. Mr. Oldfield tells us, for instance, how he got from a native the number of men killed in a certain fight. The man began to think over the names, taking a finger for each, and thus after many unsuccessful trials, he at last brought out the result by holding up his hand 3 times, to show that the number was 15.

Now our words one, two, three, four, &c., have no etymology to us, but among a large proportion of the lower races numerals have a meaning; as among many tribes of North and South America and West Africa are found such expressions as, for 5, "a whole hand," and for 6, "one to the other hand," 10, "both hands," and 11, "one to the foot;" 20, "one Indian," and 21, "one to the hands of the other Indian;" or for 11, "foot 1," for 12, "foot 2;" for 20, "a person is finished;" whilst among the miserable natives of Van Diemen's Land, the reckoning of a single hand, *viz.* 5, is called *puganna*, "a man."

For displaying to us the picture of the savage counting on his fingers, and being struck with the idea that if he describes in words his gestures of reckoning, these words will become a numeral, perhaps no language approaches the Zulu. Counting on his fingers, he begins always with the little finger of his left hand, and thus reaching 5, he calls it "a whole hand;" for 6, he translates the appropriate gesture, calling it *tatisitupa*, "take the thumb;" while 7, being shown in gesture by the forefinger, and this finger being used to point with, the verb *komba*, "to point," comes to serve as a numeral expression, denoting 7.

Now, though many numerals, especially fives, tens, and twenties were named from the fingers, hands, and feet, this is far from being the only source of numerals. Many centuries ago, the Hindu scholars, besides their regular series, made a new set of words to serve as a sort of *memoria technica* for remembering dates, &c. Thus, for 1 they said "*earth*" or "*moon;*" for 2, "*eye,*" or "*arm,*" or "*wing;*" for 3, "*Rama,*" or "*fire,*" or "*quality:*"—there being considered to be 3 Ramas, 3 kinds of fire, 3 gunas or qualities; for 4, "*age*" or "*veda,*" because there are 4 ages and 4 vedas. One line of an astronomical formula will show the working of the system:—

vahni tri rtwishu gunendu kritâgnibhûta:

that is to say,

"fire, three, season, arrow, quality, moon, four of dice, fire, element:"

that is: 3 3 6 5 3 1 4 3 5.

When Wilhelm von Humboldt, more than thirty years ago, looked into this artificial system of numeration, it struck him that he had before him a key to the general formation of numerals. When a Malay, he said, calls 5, *lima*, that is, "hand," he is doing the same thing that the Hindu pandits did when they took "wing" as a numeral for 2; and then, he suggested, the numeral words having thus been once made, the sooner their original meaning was got rid of and they were reduced to the appearance of mere unmeaning symbols, the better it would be for their practical use in language. Now a number of actual facts may be brought forward in support of Humboldt's far-sighted suggestion. The Abipones of South America counted to 3, and for 4, said "ostrich-toes," from the division of their ostrich's feet; then for 5, "one-hand;" for 10, "two-hands;" and so on. In Polynesia there is a regular set of decimal numerals, but sometimes, for superstitious reasons, they turn words out of their language for a time, and have to use fresh ones. Thus, in Tahiti, they ejected *rua*, 2, and *rima*, 5; and in a missionary translation of the Bible we find *piti* and *pae* instead; now *piti*, the new word for 2, means "together," and *pae*, the new word for 5, means "side."

In other South Sea islands, the habit of counting fish or fruit one in each hand has led to *tauna*, "a pair," becoming a numeral equivalent for 2; the habit of tying bread-fruit in knots of 4 has made a new numeral *pono*, "a knot," while other terms for 10 and 100 have had their origin from words meaning "bunch" and "bundle." And so, even in European languages, numeral words break out from time to time, ready to become proper numbers, should a vacancy be made for them in the now meaningless series *one, two, three, four.* Thus in English we have *pair* or *couple* for 2, and *score*, that is "notch," for 20. The Letts count crabs and little fish by throwing them 3 at a time, and thus the word *mettens*, "a throw," has come to mean 3, and so in many other cases in other languages.

Now when tribes count by saying *hand* for 5, *take the thumb* for 6, *half a man* for 10, and so on, it is evident that the basis of their numeration is finger-counting. But there is also evidence in the systems of numeration of most civilized languages that they, too, are the successors of a rude unspoken system of gesture counting. The rule of the whole world is to count by fives, tens, and twenties; the exceptions are so late or so incidental that we may neglect them and say that the original counting of mankind is the quinary, the decimal, or the vigesimal system, or a combination of these. We need not go abroad for examples. In the Roman numerals, which count to V, and then begin again VI, VII, we have the quinary system. The decimal system is our familiar one. And when we speak of "threescore and ten," "fourscore and thirteen," we are counting by the vigesimal system, each "score" or notch, thus ideally made, standing for 20, for "one man," as a Mexican or Carib would put it. It is a very curious thing that both we and the French, having two good decimal systems of our own, should have run off into vigesimalism. Why should we have ever said "fourscore and thirteen" for the 93, which we have good Saxon tens to express? and why should they say in France, "quatre-vingt-treize," instead of holding to the Latin original of their language, and saying "nonante-trois?" The reason seems to be that counting by scores is a strongly marked Keltic characteristic, found in Welsh, Irish, Gaelic, and Breton, and has been taken up into the alien numeral systems of France and England. At any rate, the rule of the world is to count by fives, tens, and twenties; and the connection of this rule with the practice of counting on the fingers and toes will hardly be disputed. Indeed the remark has often been made that the fact of our having 10 fingers and 10 toes has led us into a system which is actually not the best; while if we had had 6 fingers on each hand, and 6 toes on each foot, we should probably have taken to using, like the carpenter, the more convenient system of duodecimals.

These are examples of the facts which tend to show that man's early way of counting was upon his fingers; as Massieu, the Abbé Sicard's celebrated deaf and dumb pupil, records in describing his recollections of his yet uneducated childhood: "I knew the numbers before my instruction; my fingers had taught me them. I did not know the ciphers. I counted on my fingers." Among the lower races, the use of word-language has only to a small extent encroached upon gesture-language in counting; among races above these, numeral words are more largely used, but preserve evident traces of a growth out of gesture-counting; while among the higher peoples, though language gives little trace of the original signification of numerals, there still prevails the system of counting by fives, tens, and twenties, of which we can hardly doubt that the norm is given by the arrangement of the fingers and toes. Thus it appears that in the mental history of mankind we may see back to a condition so much lower than our own, that the numerals, which we look upon as so settled a part of speech that we use them as one of the first tests of the common derivation of languages, were still unspoken, and their purpose was served by the ruder, visible signs which belong to the department of gesture.

II. The next argument to be brought forward belongs to a very different province of thought, and touches on the early opinions of mankind as to the *nature and habits of spiritual beings.* It is well known that the lower races of mankind account for the facts and events of the outer world by ascribing a sort of human life and personality to animals, and even to plants, rocks, streams, winds, the sun and stars, and so on through the phenomena of nature. It is also known that a low stratum of the religion of the world consists in belief in, and adoration of, spiritual beings who inhabit the winds and trees and streams, who preside over the ripening of fruits and the falling of rain, give success in war, or inflict disease or misfortune on the savage hunter. Thus the Mintira, a low tribe on the Malayan peninsula, ascribe every ailment that happens to them to a spirit or *hantu.* One causes smallpox, another brings swelling and inflammation in hands and feet, another causes the blood to flow from wounds; indeed, to enumerate all these *hantus* would be to give a list of all their known ailments. The worship of such spirits, found among the lower races over almost the whole world, is commonly known as "fetichism." It is clear that this childlike theory of the animation of all nature lies at the root of what we call Mythology; if the sun and moon are described as semi-human beings, called by the Greeks, Hēlios and Sēlene, by the Esquimaux, Anninga and Malina, this personification is founded on an original opinion still found in lively existence in the world, that the sun and moon are living Anthropomorphic creatures. It would probably add to the clearness of our conception of the state of mind which thus sees in all nature the action of animated life and the presence of innumerable spiritual beings, if we gave it the name of Animism instead of Fetichism. Now by examining a single phase of this Animism, it seems possible to give some idea how generally man in his lowest known state of culture is a wonderfully ignorant, consistent, and natural spiritualist; and also how the effects of his early spiritualism may be traced through the development of more cultured races in proceedings which have often changed their meaning, and lost their original consistency by the encroachment of more real knowledge.

We all know how deep and sincere is the terror of ghosts among savages. It is often no exaggeration to say that they are in more deadly fear of a man after he is dead than while he is alive. The savage's notion of a ghost corresponds very nearly with that of the English peasant in our own day—it is a thin phantom going from place to place, like the person it belonged to, when it does appear, but often invisible, though capable of knocking and uttering sounds. The notion of the ghost runs almost inextricably into that of the spirit or soul, of the breath and the blood, and of those unsubstantial somethings which follow the man and are like him, his shadow and his reflection in the water. Now it is consistent with this opinion of ghosts to hold that by killing a man you can release his ghost and send it where you will. This is what the king of Dahome does when he sends men day after day to take messages to his father in the land of shadows. The Getæ, according to Herodotus, sent a man every five years to their god Zamolxis, giving him their messages, and then throwing him up and catching him on their spears. Thus in British India, some eighty years ago, it is on record that two Brahmans, believing that a man had taken forty rupees out of their house, took their own mother and cut her head off, that her ghost might torment and pursue to death the offender and his family—the old woman being herself a consenting party to the transaction. This is not an isolated case, but one belonging to a recognized Hindu practice.

In perfect accordance with this opinion we find in almost every country in the world, at some time or other, the practice of slaying men and women at the graves of the dead. In one of the South Sea Islands a cord is put round the wife's neck at her marriage, and when her husband dies it will be tightened, to release her soul, that it may accompany his to the land of shadows, and continue to catch fish and cook yams for him there. The Dyaks of Borneo have a passion for waylaying their enemies and bringing home their heads; as they told Mr. St. John, "the white men read books, we hunt for heads instead." They do this to secure the services of a slave in the next world. These practices are the consistent working out of a spiritualistic theory, which, if crude and false, is at any rate intelligible. To some extent the same may be said, when not only the dead man's wives and slaves but his dogs and horses are killed, and buried or burnt at his grave. The man's ghost is to ride the horse's ghost in the land of shadows, and the dog's ghost will run on before after ghostly game; or, as in Mexico, the dog was to carry the man across the river which lies between the world of the living and the world of the dead; while in Greenland, a dog's head was placed by the grave of a little child, that the soul of the dog, who ever knows his way home, might guide the helpless infant to the land of spirits.

But when not only men and animals but inanimate objects are buried or burnt for the dead, what does this mean? When the hunting tribes of North America provide the dead man with his favourite horse, and at the same time with his bow and arrows; while the fishing tribes bury the dead man in his canoe, with the paddle and the fish-spear ready to his hand, what difference can we discern between the purpose of the animate and of the inanimate offerings, which alike are to serve the spirit of their owner? When the dead chief's wives and his slaves, his horses, his weapons, his clothes and ornaments, are indiscriminately buried with him; when food is put in the grave with the dead man and fresh supplies brought every month; when the little child is provided with its rattle and playthings, and the dead warrior has the ceremonial pipe put in his hand, that he may hold it out as a symbol of peace when he comes to the other world, while a store of paint is buried with him that he may appear decently among his brother warriors; in these and hundreds of other instances, the spirit of the dead man is to use the spirits alike of men and animals, and of weapons, clothes, and food. Then we should expect savages to be found recognizing the existence of something of the nature of a spirit or ghost belonging to inanimate objects; and this in fact they do.* The existence of the Fijian opinion is well authenticated, that lifeless objects have spirits, and that the souls of canoes, houses, plants, broken pots, and weapons, may be seen floating down the river of death into the land of souls: and crossing into North America, we find the same idea, not only that souls are like shadows, and that everything is animate in the universe, but that the souls of hatchets, kettles, and such like things, as well as of men and animals, have to pass across the water which lies between their home in this life and the Great Village where the sun sets in the far West. We must not expect the spirits of spears and kettles to have the same distinctness and vitality in savage philosophy as the spirits of men and horses. Inanimate objects want those signs of life that are given to men and animals by the breath, the blood, the independence of voluntary action; but at any rate they have shadows, as in the New Zealand tale of Te Kanawa, who offered the fairies his neck ornament and ear-rings; they took the shadows of them, but the substance they left behind. They have also that property, which in the mind of the savage has so much to do with defining the nature of ghosts—their impalpable phantoms can and do appear far away from where their real substance is, in the dreams and hallucinations which savages look on as real events. When we meet with notions of apparitions among more civilized people, it seems that they hold a theory inherited from the full Animism of the lower races, but much damaged in its consistency by the interference of a better knowledge of facts. When the ghost of Hamlet's father appeared, he "wore his beaver up." What beaver? To a European believer in ghosts, it would seem foolish to talk of the ghost of a helmet; but to a North American Indian it is quite reasonable that a helmet should have a ghost as well as the warrior who puts it on his ghostly head. The opinion of the European ghost-seer is no doubt the more scientific, the more affected by knowledge of the facts of nature; but the broader spiritualism of the savage is more full, more thoroughly consistent, because, as there is much reason to think, it is nearer to its source.

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* The speaker mentioned that he had just found in the works of an American writer, Mr. Alger, independent confirmation of the view he had taken of the savage theory of spirits, as including spectres of inanimate as well as of animate objects.

A slight acquaintance with the spiritualism of the savage has sometimes led to its being considered as the result of a degeneration from the opinions of more cultured races; but more complete knowledge of the facts tends to show that such an opinion inverts the real history of events. The way in which the fullest and most consistent theory of ghosts is at home among savage tribes is well shown by the belief that the spirit arrives in the next world, whole or mutilated, according to the condition of the body at death. For instance, there is an Australian tribe who believe that if a man be left unburied, his soul becomes a wandering ghost. If one of their warriors kills his enemy, he is sometimes embarrassed with the difficulty that by so doing he is setting free a hostile ghost to vex his own people, and therefore he resorts to the device of cutting off the dead man's right thumb, so that the ghost can no longer throw his spear, and may be safely left to wander as an evil spirit, malignant but harmless. The history of the very funeral offerings just spoken of shows in the most interesting way the progress of a ceremony from its source in a crude and savage philosophy to its gradual breaking-down into mere formality and symbolism. To the Aryan of the Vedas it was quite reasonable to burn the priestly sacrificial implements with the dead man's body, for his use in the next world; but the modern Hindu lays one thread of woollen yarn on the funeral cake of his father, saying, "May this apparel, made of woollen yarn, be acceptable to thee!" We may learn from Ovid how the offerings of food to the dead, in ruder times a thorough practical savage proceeding, had in his time dwindled to a mere affectionate, sentimental ceremony. Garlands, he says, and some scattered corn and grains of salt, and bread steeped in wine, and violets laid about: with these the shade may be appeased. "Little the manes ask, the pious thought stands instead of the rich gift, for Styx holds no greedy gods."

"Parva petunt manes—pietas pro divite grata est
Munere. Non avidos Styx habet ima deos."

We may see how the early Christians kept up the heathen custom of burying ornaments with the dead, of putting playthings in a child's grave, doing just what a red Indian squaw will do, but doing it with how changed a purpose. The Chinese keeps up the time-honoured custom of providing the dead with clothes and money; but the money that he will palm off on his dead father is a pasteboard coin stamped like a Spanish dollar and covered with silver-leaf; this he will burn, and his father will have the spirit of it to spend in the next world. The same Chinese will yearly spread a feast for the souls of his dead ancestors; he and his friends will wait a decent while for the ghosts to eat the spirits of the food, and then they will fall to themselves. To see the same thing done nearer home, you have only to travel into Brittany, where on the night of the Fête des Morts you will find the fire made up and the hearth swept, and the supper left on the table for the souls of the dead to come and take their part. And when we see a wreath of everlastings laid upon a tomb, or a nosegay of fresh flowers thrown into an open grave, a full knowledge of the history of funeral offerings seems to justify us in believing what we should hardly have guessed without it, that even here we see a relic of the thoughts of the rudest savages who claim a common humanity with us, a funeral offering vastly changed in signification, but nowhere broken in historic sequence.

Lastly. Another subject may be found to throw light upon an early condition of men's minds. We are all agreed that there is a certain mental process called the *association of ideas.* That we are in the habit of connecting in our minds different things which have, in actual fact, no material connection, we all admit as a matter belonging to this association of thoughts or of ideas. Now we have been taught to keep an eye on the action of the association of thoughts, to recognize it as a fallacious process apt to lead us into all manner of unreasonable opinions. But if we descend to a lower range of civilization, we shall find that the mental association which we tolerate as a sort of amiable weakness, and against which we are at any rate forewarned and forearmed, is the very philosophy of the savage. There is one particularly excellent way of studying the effects of the association of thought. It began to produce, in a time associated with a very low human condition, a set of opinions and practices known as the occult sciences, witchcraft, divination, astrology, and the like. The germs of these imaginary sciences are to be found still lively among the lower races. Their development into elaborate pseudo-scientific systems belongs to a period now beginning to pass away; and we can still study them in their last stage of existence, that in which their remnants have lingered on into a period of higher mental culture, and have become survivals, or, as we call them, "superstitions." In producing the occult sciences, the association of thought works in ways most distinctly recognizable. When the Polynesian weather-maker practises on his sacred stone, wets it when he wants to produce rain, and puts it to the fire to dry when he wants dry weather; and when in Europe water is poured on a stone, or a little girl led about and pails of water poured on her that rain may in like manner be poured down from the sky, we have practices resting on the most evident and direct association of thoughts.

Thus we may see a Zulu busy chewing a bit of wood, and thereby performing an ideal operation, softening the heart of another Zulu with whom he is going to trade cows, that he may get a better bargain out of him. So it is when we find lingering in England a practice belonging thoroughly to the savage sorcerer, that of making an image representing an enemy or part of him, and melting it, drying it up, or wounding it, that the like may happen to the person with whom it is associated. From time to time there is still found hidden about some country farm such a thing as a heart stuck full of pins, the record of some secret story of attempted magic vengeance.

In the ancient and still existing art of astrology, we see the same early delusive association of ideas producing results so perfectly intelligible to us, that it is really difficult for educated people to have patience to study its details. An astrologer will tell us how the planet Jupiter is connected with persons of a bold, hearty, jovial temperament; and how the planet Venus has to do with love and marriage; while to us the whole basis of this theory lies in the accident of the names of certain gods having been given to certain stars, which are therefore supposed to have the attributes of these gods. The wonder is not that much of the magician's sham science is inexplicable to us, but that the origin of so many of its details is still evident.

[An extract from Zadkiel's almanac was here read, with the object of showing the principle on which the astrologer's deductions are still made, the movements of the heavenly bodies being simply taken to symbolize human action, virtue and good fortune being connected with the aspects of the Sun and Jupiter (sunny and jovial influences), &c., the working of the early childlike principle of the association of ideas being thus traceable through the occult sciences from their rise among savages to their decay among educated men.]

By the study of facts like those of which a scanty selection has here been brought forward, it seems possible to look back to an early condition of our race much more nearly corresponding with that of existing savages than with that of the civilized nations even of very ancient times. We seem to have before us the traces of a state of language so low that words for counting had not yet arisen in it, but mere gesture-language served their purpose. It is not meant to imply that we have evidence of a state of pure gesture-language anterior to any spoken language: we do not seem to have such evidence, and even among the lower animals we find, in a rudimentary form, expression by action and by voice going on together. In the working of the minds of these early tribes, we trace a childlike condition of thought in which there is a wonderful absence of definition between past and future, between fact and imagination, between last night's dream and to-day's waking. Out of this state of mind we find arising all over the world a consistent, intense, and all-pervading spiritualism to form a basis upon which higher intellectual stages have been reared. In this low and early mental state there reigns supreme the faculty of association of thoughts. Out of this, when unchecked by experience, arise those delusions of sorcery which pervade and embitter the whole life of the savage, and carry a stream of folly far on into the culture of the higher races. But through age after age there has gone on a slow process of natural selection, ever tending to thrust aside what is worthless, and to favour what is strong and sound. Wilhelm von Humboldt, already once quoted, may serve us again by laying down in few words one of the great generalizations of our intellectual history. "Man," he says, "ever seeks the connection, even of external phenomena, first in the realm of thought; . . . . his first endeavour is to rule nature from the idea outward."

Now if the result of inquiries like the present were to bring out mere abstract truth, barren of all practical importance—this would perhaps be the last place where it would be needful to apologize for the want. But it is to be noticed that they do happen to have this practical importance. There are certain studies which have entered upon a thoroughly scientific stage, and ask no aid from ethnographic research; they care nothing for the crude theories of earlier times, but go directly to their own observed facts by which they must stand or fall. But there are other studies, of not less importance to us than Astronomy or Chemistry, which are in a very different state. In such especially as relate to man, the operations of his mind, his relations to the rest of the universe, the past and future condition of his race, his ethical and political rights and duties—in all these complex and difficult problems we find established side by side sources of opinion of very different value. Some opinions come to us authorized by the best of evidence, and when put to the test of reason and experience the trial proves their soundness. Others again, though founded on some crude theory of less educated times, have been so altered in their scope and meaning by the lessons of experience, as to be on the whole the best known representatives of facts, and by this not unsatisfactory title they hold their ground. Others, lastly, may arise out of opinions belonging to a low stage of culture, and maintain their place, not because they are proved to be true or useful, but simply because they have been inherited from long past generations. Now it is one duty of ethnographic research to follow up these lines of thought, to mark out, among existing opinions, which are old notions kept up in a modified condition to answer a more modern purpose; in what cases a growing knowledge goes about with the remains of the old philosophy which once clothed it, now hanging in strips and tatters about its back; in what case opinions belonging to a low and early mental state survive into the midst of a higher culture, pretending to be knowledge, and being really superstition. Thus the study of the lower races has a work to do in facilitating the intellectual progress of the higher, by clearing the ground, and leaving the way open for the induction of general laws and their correction by the systematic observation of facts, to the results of which method alone we may fitly give the name of Science.

[E. B. T.]

WEEKLY EVENING MEETING,

Friday, March 22, 1867.

Sir Henry Holland, M.D. D.C.L. F.R.S. President,
in the Chair.

Dr. James Bell Pettigrew, M.D. Edin.
ASSISTANT CURATOR OF THE ROYAL COLLEGE OF SURGEONS OF ENGLAND MUSEUM.

On the Various Modes of Flight in relation to Aeronautics.

The subject of flight, natural and artificial, is one which has occupied the attention of mankind from a very early period.

It involves a more or less intimate acquaintance with Anatomy, Physiology, Mechanics, and the higher branches of Mathematics.

If regarded as a natural movement, it forms one of the three kinds of locomotion by which animals progress—the remaining two being walking and swimming; if regarded as an artificial one, it represents the unsolved problem of that grand trio which has for its integral parts the locomotive, steamboat, and flying-machine. Had time permitted, it was my intention to have gone into the subject of locomotion at length. I find, however, I must curtail my remarks under this head, which I do with reluctance, from a feeling that the chain of animal movements, like the great chain of existence, winds in and out and doubles upon itself so completely, as to render a partial examination of it in many respects unsatisfactory.

The movements of animals are adapted either to the earth, the water, or the air. There are others, however, of a mixed character, where they are suited equally to the land and water, or even to the land, water, and air.

The instruments by which locomotion is attained are therefore specially modified.

This is necessary because of the different densities and the different degrees of resistance furnished by the land, water, and air respectively.

As the earth affords a greater amount of support than the water, and the water than the air, it requires a greater degree of muscular exertion to swim than to walk, and a still greater one to fly.

For this reason flight is the most laborious, and in some respects the most complicated and difficult, of all the animal movements.

The peculiarities of the different media, as far as locomotion is concerned, may be briefly stated.

On the land we have the maximum of resistance and the minimum of displacement.

In the air, the minimum of resistance and the maximum of displacement.

The water is intermediate in these respects.

As a consequence, the feet of land animals are small—their bodies large. The horse and deer furnish examples.

In those land animals which take to the water occasionally, or the reverse, the feet are enlarged and usually provided with a membraneous expansion between the toes. Of such, the otter, ornithorhynchus, seal, frog, turtle, and crocodile may be cited.

In addition to the land animals which run and swim, there are some which precipitate themselves, parachute fashion, from immense heights, and others which even fly. In these the membraneous expansions are greatly increased – the ribs affording the necessary degree of support in the dragon or flying lizard, the anterior and posterior extremities in the flying lemur, flying cat, and bat.

Although no lizard is at present known to fly, there can be little doubt that the extinct pterodactyles, which are intermediate between the lizards and crocodiles, were possessed of this power.

The bat is interesting as being the only mammal at present enjoying the privilege of flight; it is likewise instructive, as showing that flight may be attained without the aid of hollow bones and air-sacs, by purely muscular efforts and by the mere contraction and dilatation of a continuous membrane.

If we now direct our attention to the water we find that the amount of surface engaged in locomotion greatly exceeds that in the amphibia. The fish furnishes the best example.

In it the lower half of the body and the broadly-expanded tail are applied to the water very much as an oar is in sculling. The sea-mammals, as the whale, dugong, manatee, and porpoise, swim in precisely the same manner as the fish, with this difference that the tail strikes from above downwards, or vertically instead of horizontally, or from side to side. The seal is exceptional in this respect.

The animals which furnish the connecting link between the water and the air are the flying fishes on the one hand, and the diving birds on the other: the former sustaining themselves for considerable intervals in the air by means of their enormous pectoral fins, the latter using their wings for flying above and beneath the water, as occasion demands.

I have carefully examined the relations, structure, and action of the fins in the flying-fish, and am of opinion that they act as true pinions; their inadequate dimensions only preventing them from sustaining the fish for an indefinite period in the air, at all events so long as they remain moist. They operate upon the air from beneath, after the manner of a kite or spiralifer, and in so doing, lever the animal upwards and forwards.

If they did not act as true pinions within certain limits it is difficult and indeed impossible to understand how such small creatures could obtain the momentum necessary to project them a distance of 200 or more yards, and that sometimes at an elevation of 20 feet above the water.

In birds which fly indiscriminately above and beneath the water the wing is generally provided with stiffer feathers than usual, and reduced to a minimum as regards size. In subaqueous flight the wings may act by themselves, as in the guillemots, or in conjunction with the feet, as in the grebes; but in either case it is the back or convex surface of the wing which gives the effective stroke, the wing in such birds as the great auk, which are incapable of flight, being for this purpose twisted completely round, in order that its concave surface which takes a better hold of the water may be directed backwards.

The wing therefore operates very differently in and out of the water.

In the water it acts as an auxiliary of the foot, and both strike backwards and downwards.

In the air, on the contrary, it strikes *downwards and forwards*, and this is a point deserving of attention, as showing that the oblique surfaces presented by animals to the water and air are made to act in opposite directions. This is owing to the greater density of the water as compared with the air; the former supporting or nearly supporting the animal acting upon it; the latter permitting the animal to fall through it in a downward direction.

But to come to the subject more particularly in hand, *viz.*:—

Flight in its relation to Aeronautics.—The atmosphere, because of its great tenuity, mobility, and comparative imponderability, presents little resistance to bodies passing through it at low velocity. If, however, the speed be greatly increased, the action of even an ordinary cane is sufficient to elicit a recoil.

This comes of the action and reaction of matter, the resistance experienced varying according to the density of the atmosphere and the shape, extent, and velocity of the body acting upon it. While, therefore, almost no impediment is offered to the progress of an animal in motion, it is often exceedingly difficult to compress the air with sufficient rapidity and energy to convert it into a suitable fulcrum for securing the onward impetus. This arises from the fact that bodies moving in this medium experience the minimum of resistance and occasion the maximum of displacement. Another and very obvious difficulty is traceable to the great disparity in the weight of air as compared with any known solid (this in the case of water being nearly as 1000 to 1), and the consequent want of buoying or sustaining power which that disparity necessitates. To meet these peculiarities the insect and bird are furnished with extensive surfaces in the shape of pinions or wings, which they can apply with singular velocity and power at various angles, or by alternate slow and sudden movements, to obtain the necessary degree of resistance and non-resistance. Their bodies, moreover, are constructed on strictly mechanical principles: lightness, strength, and durability of frame; and power, rapidity and precision of action, being indispensable. The cylindrical method of construction is consequently carried to an extreme; the bodies and legs of insects displaying numerous unoccupied spaces, while the muscles and solid parts are tunnelled in every direction by innumerable air-tubes which communicate with the surrounding medium by a series of apertures termed spiracles.

A somewhat similar disposition of parts is met with in birds, these being in many cases furnished not only with hollow bones, but also (especially the aquatic ones) with a liberal supply of air-sacs. They are also provided with a dense covering of feathers or down, which adds greatly to their bulk without materially increasing their weight. The air-sacs are well seen in the swan, goose, and duck; and I have in several instances carefully examined them with a view to determining their extent and function. They appear to me to be connected with the function of respiration, a view advocated by Hunter in 1774, and within the last year or so by Drosier, of Cambridge. That they have nothing whatever to do with flight is proved by the fact that some excellent flyers, take the bats *e.g.*, are destitute of them, while the wingless running birds, such as the ostrich and apteryx, which are incapable of flight, are provided with them. The same may be said of the hollow bones: some really admirable flyers, as the swallows, martins, and snipes, having their bones filled with medullary substance, while the bones of the running wingless birds alluded to are filled with air. Furthermore, and finally, a living bird weighing 10lbs. weighs the same when dead minus a very few grains; and all know what effect a few grains of heated air would have in raising a weight of 10lbs. from the ground.

When we have said that cylinders and hollow chambers increase the area of the insect and bird, and that an insect and bird so constructed is stronger, weight for weight, than one composed of solid matter, we may dismiss the subject, flight being, as I shall endeavour to show by-and-by, not so much one of weight as of power properly directed, *i.e.* power directed on strictly mechanical principles. Those who subscribe to the heated-air theory are of opinion that the air contained in the cavities of insects and birds is so much lighter than the surrounding atmosphere, that it must of necessity contribute materially to flight; but the quantity of air imprisoned is, to begin with, so infinitesimally small and the difference in weight which it experiences by increase of temperature so inappreciable, that it ought not to be taken into account by any one endeavouring to solve the difficult and important problem of flight. The Montgolfier or fire balloons were constructed on the heated-air principle; but as these have no analogue in nature and are apparently incapable of improvement, they need not detain us at this stage of the inquiry. The area of the insect and bird when the wings are fully expanded is, with the single exception of the bats, greater than that of any other class of animals, their weight being proportionably less. It ought, however, never to be forgotten that even the lightest insect or bird is immeasurably heavier than the air, and that there is no fixed relation between the weight of body and the expanse of wing in either class. We have thus light-bodied and large-winged insects and birds, as the butterfly, heron, and albatross; and others, whose bodies are comparatively heavy, while their wings are insignificantly small, as in the sphinx-moth and stag-beetle among insects, and the grebe, quail, and partridge among birds. Those apparent inconsistencies are readily explained by the greater muscular development of the heavy-bodied short-winged insects and birds, and the increased power and rapidity with which the wing is made to oscillate. This is of the utmost importance in the science of aerostation, as showing that flight may be attained by a heavy, powerful animal with comparatively small wings, as well as by a lighter one with enormously enlarged wings. While, therefore, there is apparently no correspondence between the area of the wing and the animal to be raised, there is an unvarying relation as to the weight and number of oscillations, so that the problem of flight seems to resolve itself into one of weight, power, velocity, and small surfaces; *versus* buoyancy, debility, diminished speed, and extensive surfaces: weight in either case being a *sine qua non.*

In order to utilize the air as a means of transit, the body in motion, whether it moves in virtue of the life it possesses, or because of a force superadded, must be heavier than it. If it were otherwise, if it were rescued from the operation of gravity on the one hand, and bereft of independent movement on the other, it must float about uncontrolled and uncontrollable, as happens in the ordinary gas balloon. The difference between an insect or bird and a balloon here insisted upon was, I have learned since writing the above, likewise pointed out by His Grace the Duke of Argyll, in his very able and eloquent article in 'Good Words,' entitled "The Reign of Law"* —an article whose merits cannot be too widely acknowledged or too universally known. The wings of insects and birds are, as a rule, more or less triangular in shape, the base of the triangle being directed towards the body, the sides anteriorly and posteriorly. They are also conical on sections from within outwards and from before backwards, this shape converting the pinion into a delicately-graduated instrument, balanced with the utmost nicety to satisfy the requirements of the muscular system on the one hand, and the resistance and resiliency of the air on the other. While all wings are graduated as explained, innumerable varieties occur as to their general contour, some being falcated or scythe-like, others oblong, others rounded or circular, some lanciolate, and some linear.

Wing of Insect.—The wings of insects may consist either of one or two pairs; the anterior or upper pair, when two are present, being in some instances greatly modified and presenting a corneous condition. When so modified they cover the under-wings when the insect is reposing, and have from this circumstance been named elytra from the Greek ἔλυτρον, a sheath. The elytra or wing-cases as they are sometimes called, are dense, rigid, and opaque in the beetles; solid in one part and membraneous in another in the cockroaches; more or less membraneous throughout in the grasshoppers; and completely membraneous in the dragon-flies. The superior or upper wings are indirectly connected with flight in the beetles, cock-roaches, and grasshoppers, and actively engaged in this function in the dragon-flies and butterflies. The true wings, and by this I mean the membraneous ones, present different degrees of opacity; those of the moths and butterflies being non-transparent; those of the dragon-flies, bees, and common flies presenting a delicate, filmy, gossamer-like appearance. They have, however, this feature in common, and it is fundamental; both pairs are composed of a duplicature of integument, or investing membrane, and are strengthened in various directions by a system of hollow, horny tubes, known to entomologists as the neuræ or nervures. These nervures taper towards the extremity of the wing, and are strongest towards its root and anterior margin, where they supply the place of the arm in bats and birds.

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* 'Good Words' for February, 1865. This article I am glad to find has been reprinted in a separate form with numerous illustrations, and should be read by all interested in the subject of aeronautics.—J. B. P.

The neuræ are arranged at the axis of the wing after the manner of a fan or spiral stair; the anterior one occupying a higher position than that farther back, and so of the others. As this arrangement extends also to the margins, the wings are more or less twisted upon themselves, and present a certain degree of convexity on their superior or upper surface, and a corresponding concavity on their inferior or under surface; their free edges supplying these fine curves which act with such efficacy upon the air in obtaining the maximum of resistance and the minimum of displacement. As illustrative examples of the form of wing alluded to, that of the beetle, bee, and fly may be cited: the pinion in those insects acting as helicēs, or twisted levers and elevating weights, much greater than the area of the wing would seem to warrant. The insects adverted to fly, as a rule, with great accuracy and speed, and frequently in a straight line.

From the foregoing account it is evident that the wings of insects vary as regards their number, size, and shape. They also differ as regards their surfaces, margins, venation, degree of consistence and position, so that it might naturally be asked, Do the several orders of wings act according to a common principle, or does each wing act according to a principle of its own? There can, I think, be but one answer to this question. All wings obtain their leverage by presenting oblique surfaces to the air, the degree of obliquity gradually increasing in a direction from behind, forwards and downwards, during extension when the sudden or effective stroke is being given, and gradually decreasing in an opposite direction during flexion, or when the wing is being more slowly recovered preparatory to making a second stroke. The effective stroke in insects, and this holds true also of birds, is therefore delivered *downwards and forwards*, and not, as the majority of writers believe, vertically, or even slightly backwards. This arises from the curious circumstance, that insects and birds when flying actually fall through the medium which elevates them, their course being indicated by the resultant of two forces, *viz.*: that of gravity, pulling vertically downwards, and that of the wing, acting at a given angle in an upward direction. The wing of the bird acts after the manner of a boy's kite, the only difference being that the kite is PULLED FORWARDS upon the wind by the string and the hand, whereas in the bird the wing is PUSHED FORWARDS on the wind by the weight of the body and the life residing in the pinion itself. The angle at which the wing acts most efficaciously as an elevator, as proved by an examination of the pinion of the living insect, bat, and bird, when fully extended and ready to give the effective stroke, is an angle of 45° with the horizon. As, however, this angle could not be uniformly maintained without a rotatory motion which would wrench the wings from their fixings, a compromise is adopted, the wing being made to rotate on its axis to the extent of a quarter of a turnin one direction during extension, and the same amount in an opposite direction during flexion. That the wing rotates upon its axis as explained may be readily ascertained by watching the movement in the larger domestic fly. If the insect be contemplated either from above or beneath, the blur presented by the rapidly oscillating wing will be found to be concave, the depressed portion representing the wing when its plane of least resistance is parallel with the plane of progression. Of this I have had the most convincing proof, particularly in semi-torpid insects where the wing was plied with less vigour than usual. To confer on the wing the multiplicity of movement which it requires, it is supplied with a double hinge or compound joint which enables it to move not only in an upward, downward, forward, and backward direction, but also at various intermediate degrees of obliquity. An insect furnished with wings thus hinged may, as far as steadiness of body is concerned, be not inaptly compared to a compass set upon gimbals, where the universality of motion in one direction ensures comparative fixedness in another.

Many instances might be quoted of the marvellous powers of flight residing in insects as a class. The male of the silkworm moth (Attacus Paphia) is stated to travel more than 100 miles a day;* and an anonymous writer in Nicholson's Journal calculates that the common house-fly (Musca domesticus) in ordinary flight makes 600 strokes per second, and advances 25 feet; but that the rate of speed, if the insect be alarmed, may be increased six or seven fold, so that under certain circumstances it can outstrip the fleetest racehorse. Leeuwenhoek relates a most exciting chase which he once beheld in a menagerie about 100 feet long, between a swallow and a dragon-fly (mordella). The insect flew with such incredible speed and wheeled with such address, that the swallow, notwithstanding its utmost efforts, completely failed to overtake it. *

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* Linn. Trans. vii. 40.

Wing of Bird.—There are few things in nature more admirably constructed and where design can be more readily traced than in the wing of the bird. Its great strength and extreme lightness, the manner in which it closes up or folds during flexion, and opens out or expands during extension, as well as the method according to which the feathers are strung together, and slate each other in divers directions to produce at one time a solid resisting surface, and at another an interrupted and comparatively non-resisting one, present a degree of fitness to which the mind must necessarily revert with pleasure. The wing of the bird, like that of the insect, is concavo-convex, and more or less twisted upon itself when extended, so that the upper or thick margin of the pinion presents a different degree of curvature to that of the nether or thin margin: the curves of the two margins in some instances even intersecting each other. This twisting is in a great measure owing to the manner in which the bones of the wing are twisted upon themselves, and the spiral nature of their articular surfaces, the long axes of the joints always intersecting each other at right angles. As a result of this disposition of the articular surfaces the wing may be shot out or extended, and retracted or flexed in nearly the same plane, the bones composing the wing rotating on their axes during either movement. This secondary action, or the revolving of the component bones upon their own axes, is of the greatest importance in the movements of the wings, as it communicates to the hand and forearm, and consequently to the primary and secondary feathers which they bear the precise angles necessary for flight. It in fact ensures that the wing, and the curtain or fringe of the wing which the primary and secondary feathers form, shall be screwed into and down upon the wind in extension, and unscrewed or withdrawn from the wind during flexion. The wing of the bird may therefore be compared to a huge gimlet or auger, the axis of the gimlet representing the bones of the wing, the flanges or spiral thread of the gimlet the primary and secondary feathers. As the degree of rotation made by the bones of the forearm and hand during extension amounts as nearly as may be to a quarter of a turn of a spiral, it follows that in flexion the wing presents a knifelike edge to the wind; whereas in extension the curtain of the wing is rotated in a downward direction until its anterior or concave surface makes an angle of 45° with the horizon. From this description it will be evident that by the mere rotation of the bones of the forearm and hand the maximum and minimum of resistance is secured much in the same way that this object is attained by the alternate dipping and feathering of an oar.

In the majority of quick-flying birds—at all events in such as do not glide or skim—considerable advantage is gained by the primary and secondary feathers being thrown out of position during flexion, this arrangement preventing retardation, by diminishing the amount of air displaced. This slating or overlapping and unslating action of the feathers during extension and flexion is, however, one of the peculiarities or refinements, and not necessarily an essential in flight, as this function can be efficiently performed by the insect and bat where no feathers are present, and where consequently no opening or closing of them can possibly occur. The wing of the bird may be said to act in three different ways:—1st, During extension, when it gradually makes an angle of 45° with the horizon; 2nd, During the downward stroke, when it maintains the angle of 45° with the horizon, and makes a variable angle with the body; and 3rd, During flexion, when it acts at a gradually decreasing angle in virtue of its being carried against the wind by the body of the bird which is in motion; it being a matter of indifference whether the wing acts on the air or the air on the wing, so long as the body bearing the latter is under weigh; and this is perhaps the chief reason why the albatross, which is a very heavy bird,* can sail about for such incredible periods without apparently moving the wings at all. Captain Hutton thus graphically describes the sailing of this magnificent bird:—"The flight of the albatross is truly majestic, as with outstretched motionless wings he sails over the surface of the sea, now rising high in air; now, with a bold sweep and wings *inclined at an angle* with the horizon, descending until the tip of the lower one all but touches the crest of the waves as he skims over them." †

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* The hobby falcon which abounds in Bulgaria is equal to this task—the dragon-fly forming a principal constituent of its food.

"Tranquil its spirit seemed, and floated slow,
Even in its very motion there was rest."

As an antithesis to the apparently lifeless wings of the albatross, the ceaseless activity of those of the humming bird might be adduced. "In those delicate and exquisitely beautiful birds, the wings, according to Mr. Gould, move so rapidly when the bird is poised before an object that it is impossible for the eye to follow each stroke, and a hazy circle of indistinctness on each side of the bird is all that is perceptible."

The various movements involved in ascending, descending, wheeling, gliding, and progressing horizontally are all the result of muscular power, properly directed and acting upon appropriate surfaces—that apparent buoyancy in birds, which we so highly esteem, arising not from superior lightness but from their possessing that degree of weight which enables them to subjugate the air; weight and independent motion being the two things indispensable in successful aerial progression. The weight in insects and birds is in great measure owing to their greatly-developed muscular system—this being in that delicate state of tonacity which enables them to act through its instrumentality with marvellous dexterity and power, and to expend or reserve their energies, which they can do with the utmost exactitude in their lengthened and laborious flights. The elastic structures which receive or draw back the wing in the insect and bird during flexion are of the utmost consequence in the movements of the wings; these by their mere contraction enabling the muscles of the wing to rest nearly half the time they are in action. In this we have a probable explanation of the extraordinary power of endurance displayed by insects and birds on the wing.

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* The average weight of the albatross, as given by Gould, is 17lbs. 'Ibis,' 2nd series, vol. i. 1865, p. 295.

The Pelicanus onocrotalius weighs 25lbs. Roget's 'Bird's Jour.' vol. i. p. 490.

† On some of the birds inhabiting the Southern Ocean, by Captain W. F. Hutton. 'Ibis,' 2nd series, vol. i. 1865, p. 282.

The foregoing remarks on the wings of insects and birds lead me to speak of the inclined plane as applied to the air, but before doing so, it will be advisable to allude briefly to the balloon.

Balloon.—This, as my audience is aware, is constructed on the obvious principle that a machine lighter than the air must necessarily rise through it. The Montgolfier Brothers invented such a machine in 1782. Their balloon consisted of a paper-globe or cylinder, the motor power being superheated air supplied by the burning of vine twigs under it. The Montgolfier or fire balloons, as they were called, were superseded by the hydrogen-gas balloon of MM. Charles and Robert, this being, in turn, supplanted by the ordinary gas balloon of Mr. Green. Since the introduction of coal gas in the place of hydrogen gas no radical improvement has been effected; all attempts at guiding balloons having signally failed. This arises from the vast extent of surface which they necessarily present, rendering them a fair conquest to every breeze that blows; and because the power which animates them is a mere lifting power which, in the absence of wind, must act in a vertical line, all other motion being extraneous and foreign to it. It consequently rises through the air in opposition to the law of gravity, very much as a dead bird would fall in a downward direction in accordance with it. Having no hold upon the air, this cannot be employed as a fulcrum for regulating its movements, and hence the cardinal difficulty in ballooning as an art.

Any one attempting to control the movements of a balloon is very much in the position of a boatman who endeavours to steer his craft, which is drifting with the current, by pushing against the stern.

If ever the balloon is to be utilized as a means of transit, this will probably be achieved by converting part of its lifting power into a horizontal propelling power, which possibly could be done by affixing a horizontal screw, like a small windmill, to the car; this apparatus receiving its motion by being forced against the air from beneath during its ascent (the air playing upon it from above), and communicating its movements to a similar and smaller screw placed vertically or at right angles, which could be made to revolve with great celerity as a driving screw. To prevent rotation in the balloon itself, it might to be armed with plates of some light material placed at right angles to the plane of rotation. The great expense, however, involved in the construction and filling of the balloon will always operate against its being used otherwise than as a luxury; while the enormous expanse and delicacy of the material employed, as well as the change in volume of the contained gas arising from increase or decrease of temperature, cannot fail to prove troublesome, not to say dangerous.

Finding that no marked improvement has been made in the balloon since its introduction in 1782, we naturally turn our attention to some other method of traversing the air; and here I would add my independent testimony in favour of the helice or screw, not only as a lifting power, but also as a propelling power. When I commenced my inquiries into the structure and uses of wings, I was early struck with the curious manner in which they are twisted upon themselves, and how they are rotated on and off the wind during flexion and extension, after the manner of screws; and without knowing (for the subject of artificial flight is not much in my way) that the helice had been proposed as a means for raising inanimate bodies, I had actually constructed a double screw, with a view to testing its efficacy in this respect.* I have therefore unwittingly laid anatomy and physiology under contribution in support of what I find is not a new doctrine.† I was impelled in this direction by detecting the principle in nature, and from knowing that a body to rise and progress in the air need not necessarily be lighter than it; in fact, that the balloon is constructed on a principle diametrically opposed to that on which the bat, insect, and bird are constructed, and is from this circumstance open to serious, and in some respects, insuperable objections.

The efficacy of the screw in water is well known, and the action of the child's toy, usually called the spiralifer, will illustrate its utility as applied to the air. This toy, for toy it has hitherto been, consists of two inclined planes, produced by simply twisting the enveloping wires in opposite directions. It therefore represents the most primitive form of screw. This apparatus, simple as it may appear, curiously enough furnishes the mechanical appliance by which a body may be elevated, or elevated and carried in a horizontal direction at one and the same time. By applying the necessary power the spiralifer can be made to act vertically or horizontally, or at any intermediate angle, so that we have in it an easily regulated and perfect driving power. The position taken up by the advocates of the screw is the reverse of that occupied by the advocates for the balloon; so that the aeronaut promises at no distant day to be fairly impaled on the horns of a dilemma, by having on the one hand, a motor power which (because of the space occupied by it) no human ingenuity can direct; and on the other a thoroughly manageable and docile elevating and driving apparatus, minus an adequate motor power. The problem of flight will probably be solved by one employing a certain proportion of gas to assist him in overcoming the inertia of his machine while he uses the screw as a propeller and partial elevator. Of the two systems propounded, if they'be judged separately, I incline to that which proposes to employ the screw both in elevating and propelling, and this for two reasons: 1st, Because the screw or a modification of it is the instrument by which, as I have shown, the insect, bat, and bird rises and progresses; and 2nd, Because a certain degree of weight is necessary to overcome the air and make it useful for the purposes of aerostation.

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* This screw had four fans or blades, two of which revolved from left to right; the remaining two from right to left. This I found to be necessary to prevent rotation in the driving apparatus, which consisted of a steel spring and clockwork.

† Paucton the engineer, predicted the future importance of the screw in aerial navigation, as early as 1867.

That the principle of the helice as applied to the air is correct, is proved by the very remarkable experiments of MM. Pontin d'Amécourt and De la Landelle, both of whom have constructed within the last three years helicopteric models, which not only rise by themselves into the air, but also carry graduated weights.* The difficulties therefore attending aerial locomotion by means of the screw are already partially surmounted.

The advantages which will accrue from the employment of the screw in aerostation may be briefly stated.

It occupies little space, is strong without being heavy, and is prodigiously powerful.

It rigidly economizes the motor power by keeping the inclined planes of which it is composed closely applied to the air throughout its entire revolution.

The speed of the screw can be increased at pleasure—increased velocity, as I have shown in the insect and bird, conferring enormously increased propelling and lifting power.

By a judicious combination of horizontal, vertical, and oblique screws, almost any degree of speed may be attained, and any course, whether upwards, downwards, or forwards, pursued.

A machine elevated and propelled by screws will be necessarily a compact machine—a machine which will navigate the air as a master; its weight and the small surface occupied by it rendering it superior even to moderately high winds.

The nearer such machine is kept to the earth and the greater the density of the atmosphere, the greater will be its facility and power — the inconveniences arising from temperature and excessively rarefied air being thus avoided.

The aerial screw machine should be constructed whenever practicable of hollow cylinders fixed into a floor, composed of one or more flattened cylindroid chambers filled with hydrogen or other gas to diminish weight. The flattened cylinders, if laid horizontally or inclined in a slightly upward direction, would act mechanically as sustainers and gliders, as do the wings in sailing and gliding birds. It is just possible that the motor power required for the helicopteric flying-machine may be derived from compressed atmosphere, the air being compressed by the aid of an engine on *terra firma*, and stowed away in the cylinders comprising the floor or other portions of the machine before starting.

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* Extract from a paper, by Mons. Nadir, 1863, quoted in 'Astra Castra:' By Hatton Turnor, London, 1865, p. 340.

When and where such a machine will be successfully launched no one can of course predict. The subject of artificial flight, however, has been so frequently discussed of late years, and has excited so much interest in America, France, and other portions of the Old and New World, that it must obviously receive a settlement in one direction or other at no distant date. Even Britain, involved as she is in business and politics, and caring little about science which is not directly remunerative, has made a move in this direction, and we have now the "Aeronautical Society of Great Britain," presided over by His Grace the Duke of Argyll, himself a Goliath in aeronautical matters. It were much to be desired that those who can afford the time or the means requisite for conducting experiments on a scale commensurate with the importance of the subject would lend their aid to this great public movement.

Homo Volans.—Whether the genus homo will ever be able, by his unaided exertions, to leave the scene of his joys and sorrows for the fields etherean, time only can determine. Borelli, a great anatomical authority,* made elaborate calculations to prove the absurdity of such an attempt. His calculations, however, will not deter the more sanguine and speculative portions of mankind from renewing their exertions in this direction as opportunity permits; and I may state, for their guidance in the matter, that if man ever flies it will not be by employing his arms simply, but by concentrating the energies of his entire muscular system—by transferring in fact the movements of his arms and legs to a central axis or shaft, surmounted by one or more horizontal and vertical screws of appropriate size and shape; these being made to revolve with a velocity to be determined by experiment. The value of this hypothesis could be readily tested, and at a trifling expense, by a machine constructed after the manner of a velocipede, which need not be of a very complicated character.

In order to construct a successful flying-machine, it is not necessary to imitate the filmy wing of the insect, the silken pinion of the bat, or the complicated and highly differentialed wing of the bird, where every feather may be said to have a peculiar function assigned to it; neither is it necessary to reproduce the intricacy of that machinery by which the pinion in the bat, insect, and bird is moved: all that is required is to distinguish the form and extent of the surfaces and the manner of their application, and this has, in a great measure, been already done. When Vivian and Trevithick constructed the locomotive, and Symington and Bell the steamboat, they did not seek to reproduce a quadruped or a fish ; they simply aimed at producing motion adapted to the land and water in accordance with natural laws, and in the presence of living models. Their success is to be measured by an involved labyrinth of railroad which extends to every part of the civilized world, and by navies whose vessels are despatched without the slightest trepidation to navigate the most boisterous seas at the most inclement seasons. The aeronaut has the same task before him in a different direction, and in attempting to produce a flying-machine he is not necessarily attempting an impossible thing. The countless swarms of flying things testify as to the practicability of the scheme, and nature at once supplies him with models and materials. If artificial flight were not attainable, the insects and birds would afford the only examples of animals whose movements could not be reproduced. The outgoings and incomings of the quadruped and fish are, however, already successfully imitated, and the fowls of the air, though clamorous and shy, are not necessarily beyond our reach. Much has been said and done in clearing the forest and fertilizing the prairie, can nothing be done in reclaiming the boundless regions of the air?

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* 'De Motu Animal.'

[J. B. P.]

Royal Institution of Great Britain.

WEEKLY EVENING MEETING,

Friday, March 29, 1867.

Sir Henry Holland, Bart. M.D. D.C.L. F.R.S. President, in the Chair.

Edward Frankland, Esq. F.R.S.

PROFESSOR OF CHEMISTRY, R.I.

On the Water Supply of the Metropolis.

Another attack of the most terrible epidemic to which modern London is subject, has once more called earnest attention to the serious defects of the Metropolitan Water Supply. The origin and spread of cholera is confessedly still involved in much obscurity, but the experience which we have derived from four visitations leads irresistibly to the conclusion, that, whilst no successful barriers have been devised against the introduction of the disease into this or any other country, cholera can never establish itself as an epidemic unless the water supply of a community be tainted with sewage impurity. Thus Manchester suffered fearfully from cholera in 1832 and in 1849, whilst supplied with impure water, but after the introduction of pure water from the Derbyshire hills, the return of the disease in 1854, and again last year, manifested itself in Manchester by a few sporadic cases only; although in other respects Manchester is one of the most unhealthy towns in the United Kingdom.

The violence of the epidemic, as Dr. Farr has shown, exhibits also a close relation to the *degree* of sewage contamination of the water supply. Thus he has demonstrated that in the visitation of 1849, that portion of the metropolitan population which was supplied by water taken from the Thames at Kew suffered a mortality from cholera of 8 in 10,000. Of every 10,000 people supplied with water taken from the river at Hammersmith, 17 died. Of the inhabitants of Belgravia, St. George's Hanover Square, Chelsea, and Westminster, supplied with water taken below Chelsea Hospital, 47 in 10,000 died. Whilst the populations drawing their supply still lower down, *viz.* at Battersea and between Hungerford and Waterloo Bridges, where the river was still more foul, suffered to the extent of 163 in 10,000. In the year 1854, one-half of this latter district was supplied by water taken above Teddington Lock, and the deaths fell to 87—little more than one-half; whilst last year, when the whole supply was drawn above Teddington Lock, the loss of life from cholera was only 8 in 10,000.

The water withdrawn from the Thames is now all taken above Teddington Lock, and its filtration before distribution is rendered compulsory by the Metropolitan Water Act of 1852. The wisdom of thus withdrawing the water at a higher point, and of enforcing its filtration, is evidenced by the comparatively slight mortality from cholera last year in those districts supplied with Thames water. Far different, according to the Registrar-General, was the fate of that portion of the metropolis which had the misfortune to be supplied from reservoirs at Old Ford, belonging to the East London Water Company. The suddenness and virulence of the outbreak of cholera in the east of London last summer at once aroused the suspicions of the Registrar-General, who requested me to make an immediate investigation into the East London Water Company's supply. I found the chief reservoir at Old Ford to be situated close to the river Lea, which is there little better than an open sewer. This reservoir is also sunk 16 feet beneath the low ground, which is there only just above the level of spring-tides; consequently the water in the reservoir is always below Trinity high-water mark. I pointed out that soakage from the adjacent foul river and from the surrounding soil, saturated with sewage, must take place into such an excavation, with its floor of two-and-a-half acres in extent, an opinion which has been confirmed by recent investigation when this reservoir was emptied as far as possible by pumping, for the soakage was so great that it was found impossible to empty it. The mortality in that portion of London supplied from these reservoirs was frightful, for whilst the deaths in the districts drawing water from other sources varied from 2 to 12 in 10,000, they ranged from 63 to 111 in 10,000 in those districts supplied from Old Ford.

Present Metropolitan Water Supply.—London is at present supplied with water by nine companies, who deliver about 108,000,000 gallons daily. Some idea may be formed of the vastness of this supply by a comparison of its volume with some well-known magnitude. If it were contained in a reservoir having a floor area equal to that of Westminster Hall, the walls would require to be carried to the height of 1070 feet, or more than three times the height of the Victoria tower, to enable it to contain the water which is daily distributed in the metropolis. Five of the water companies abstract about one-half of the total supply from the Thames; two withdraw about 42,000,000 gallons from the river Lea, and the remainder is pumped by two other companies (the Kent and South Essex Companies) from artesian wells sunk into the chalk of the Thames basin. Such is the present volume of water daily supplied to London and its suburbs; what will be the amount required twenty years hence it is difficult to estimate, but if the annual rate of increase since 1850 be continued, it can scarcely be less than 150,000,000 of gallons, for in 1850 the gross daily quantity delivered was only 44½ millions of gallons, in 1856 it had reached 81 millions of gallons, whilst in 1865 it was 108 millions of gallons.

Proposed Water Supply of London.—Notwithstanding the best efforts of the water companies, the present supply of water to the metropolis is far from satisfactory, owing to causes which are mostly beyond the control of those to whom that supply is entrusted; it is therefore contemplated either to change entirely the source of supply, and thus obtain water of greater purity than any available in the neighbourhood of London, or so to alter the conditions at present affecting Thames water, as to materially improve its quality. For this purpose no less than five schemes have been recently brought forward, *viz.*:—

1. Sources of the Severn, proposed by Mr. Bateman.
2. The Cumberland Lakes—Messrs. Hemans and Hassard
3. Thames water filtered through Bagshot Sands—Mr. Telford Macneill.
4. Storage reservoirs near the sources of the Thames—Mr. Bailey Denton.
5. Derbyshire and Staffordshire hills—Mr. Remington.

The last three of these schemes have scarcely yet assumed a shape for discussion from a chemical point of view; we shall therefore confine our attention to the first two.

Mr. Bateman's Scheme.—Mr. Bateman proposes to obtain the metropolitan water supply from the mountain ranges of Cader Idris and Plynlimmon, in North Wales, which constitute the chief sources of the Severn. These mountains rear their heads into the moist air brought from the Atlantic by the prevailing south-westerly winds, and receive the precipitation of from 70 to 150 inches of rain per annum. We should thus avail ourselves of a great natural and very active distillatory apparatus, furnishing water of great purity. These Welsh hills consist of the Upper and Lower Silurian formations, "which yield water as pure in quality as that of Loch Katrine, and which afford sites for magnificent reservoirs, which may be constructed with perfect safety and facility, and of sufficient capacity to economize the full annual rainfall I have assumed, and to last out droughts of from 140 to 150 days' duration, both for town supply and river compensation. One of these districts, of 66,000 acres in area, is situated a little to the east of the range of mountains of which Cader Idris and Aran Mowddy are the highest summits, forming the drainage ground of the rivers Banw and Vyrnwy. The other district, of about equal area, is situated immediately to the east of Plynlimmon, 2500 feet in height. The discharge pipes of the lowest reservoir in each of these districts will be placed at an elevation of about 450 feet above the level of Trinity high-water mark. The water will be conducted by separate aqueducts, of 19 miles and 21½ miles in length respectively, to a point of junction near Martin Mere, from whence the joint volume of the water will be conveyed by a common aqueduct to the high land near Stanmore, where extensive service reservoirs must be constructed, which will be at an elevation of at least 250 feet above Trinity high-water mark. From these reservoirs the water will be delivered to the city at high pressure, and under the constant supply system. The total distance from the reservoirs on the Severn to London will be 183 miles. One of the reservoirs on the river Vyrnwy will, by an embankment of 76 feet in height, form a lake of five miles in length, and will contain 1,089,000,000 cubic feet. Another, on the river Banw, by an embankment of 80 feet in height, will form a lake of four miles in length, and contain 940,000,000 cubic feet; and a third, in the same district, by an embankment of similar height, will contain 732,000,000 cubic feet. Amongst the reservoirs on the Severn will be one which, by an embankment of 75 feet in height, will contain 2,230,000,000 cubic feet; this single reservoir being 50 per cent. greater than the available water in Loch Katrine." Mr. Bateman estimates the cost for 220,000,000 gallons per day at 10,850,000*l.*, the interest upon which, together with cost of maintenance, &c., would be met by a domestic rate of 10*d.* in the pound, and a public rate of 2*d.* in the pound. The present rate paid to the London water companies is much heavier, being about 1*s.* 5*d.* in the pound. The total gathering ground in Mr. Bateman's scheme is estimated at 204 square miles.

Messrs. Hemans and Hassard's Scheme.—This scheme lays under contribution the magnificent condensing surface of the Cumberland and Westmoreland mountains, where, as every tourist knows, rainless days are rare exceptions. The extent of gathering ground would be 177 square miles, whilst the average annual rainfall in the district is 100·56 inches. The districts from which water is proposed to be taken, lie on the northern slopes of the range of hills towering over Grassmere, Windermere, and Kendal, and draining into the rivers Lowther and Greta, and into the lakes of Haweswater, Ullswater, and Thirlmere. This scheme has the advantage of naturally-formed reservoirs, which would, however, require to be further enlarged by embankments; and these natural advantages are also to some extent counterbalanced by a greater length of conduit (280 miles), and by the necessity for several tunnels, two of which would be respectively 7¼ and 8 miles long. The daily delivery of water in London would be 250,000,000 gallons, and the cost of the works, &c., 13,500,000*l.* The interest upon this capital, cost of maintenance, &c., and compensation to present water companies, would be met by a domestic rate of 1*s.* 1*d.* per pound.

From a chemical point of view, it is at present quite impossible to give the preference to one or the other of these colossal schemes, both of which are truly worthy of the latter half of this century of engineering triumphs, and of the great city on behalf of which they are projected. Before we can appreciate, however, the advantages of such magnificent undertakings, it is necessary that we should first consider the chemical quality of our present supply, and compare it with that of the water which we should obtain from these new sources.

Quality of the present Metropolitan Water Supply.—Absolutely pure water is never found in nature. In addition to mechanically suspended impurities which can be mostly removed by filtration, potable waters also contain various solid substances in a state of solution. These substances are left behind as a solid residue when such waters are evaporated to dryness; they have been commonly classified by chemists into the three following divisions:—

1. Matters which are expelled when the solid residue is heated to redness in contact with air.
2. Matters which are not expelled at a red heat, and which decompose soap.
3. Matters which are not expelled at a red heat, and which do not decompose soap.

The substances of the first division consist of:—

a. Organic matter.
b. The products of the decomposition of certain mineral salts, chiefly nitrites and nitrates.

Formerly ammoniacal salts, a certain amount of moisture, and even hydrochloric acid were amongst the products expelled on ignition, but since the adoption of the suggestion of Hofmann and Blyth in the year 1856 to add a known weight of carbonate of soda to the water before evaporation, these substances have been excluded from category No. 1, and it is important to bear this in mind when comparing the analyses of waters made prior to 1856 with those which have been made since that year. Notwithstanding the more definite character, however, thus given to the matters expelled on ignition, it is still difficult to interpret the meaning of this loss. It may all arise from organic matter, —nay, there may even be more organic matter in the solid residue of a water than is indicated by the total loss on ignition, as I have recently had occasion to observe; or it may be all due to the dissipation of mineral ingredients, the result, however, of the decomposition of previously existing organic matter. When it is large it throws suspicion upon the character of the water, it indicates either the presence of organic matter, animal or vegetable, or it denotes previous contamination with sewage or decaying animal matters. This analytical determination is thus surrounded with much uncertainty; and it has always been considered, as indeed it deserved to be, highly unsatisfactory. Hence the attempts which have been made to indicate, directly or indirectly, by means of permanganate of potash, the amount of real organic matter involved in this loss by ignition. Permanganate of potash when dissolved in water readily yields oxygen to many substances capable of combining with this element. Thus if it be added to water acidulated with sulphuric acid, and containing oxalic acid in solution, the latter is completely and rapidly converted into carbonic acid and water at the expense of oxygen derived from the permanganate; and it is found that one part by weight of oxalic acid in being thus oxidized abstracts almost exactly eight parts by weight of oxygen from the permanganate, the latter being converted into sulphate of manganese. In undergoing this chemical change the rich violet colour of the solution of permanganate of potash vanishes; and it is thus easy to ascertain, by the non-disappearance of the characteristic tint of the permanganate, when the oxidation of the oxalic acid is complete. Now a similar disappearance of colour occurs when the solution of permanganate of potash is added to an acidulated sample of potable water containing organic matter; and it has been assumed that, as in the case of the oxalic acid, the organic matter contained in the water is completely oxidized by the permanganate, which was thus thought to indicate the amount of oxygen required to oxidize completely the organic matter contained in the water. Dr. Letheby has even employed this reaction for the estimation of the *actual weight* of organic matter contained in a known volume of water, on the assumption that every grain of organic matter contained in a sample of water robs the permanganate solution of eight grains of oxygen. Such a method of ascertaining the actual amount of organic matter in a water, or even the amount of oxygen required to convert this organic matter into its final mineral products of oxidation, would be invaluable on account of the extreme facility with which it can be applied; and it was therefore not without a certain amount of regret that, after employing this process for many months, I noticed unmistakable symptoms of its untrustworthiness, symptoms which were amply confirmed on submitting it to rigorous experimental tests. By the addition of known weights of different organic substances to equal volumes of pure distilled water, the latter was artificially contaminated with a known proportion of each kind of organic matter. Each sample of water so artificially contaminated was made to contain three parts of organic matter in 100,000. I then proceeded to ascertain—first, the amount of oxygen which this organic matter abstracted from the permanganate of potash; and secondly, the actual amount of organic matter present in the water, on the assumption that each part by weight of organic matter consumed eight parts by weight of oxygen from the permanganate of potash. The same test was also applied to another sample of distilled water from which all organic matter was carefully excluded, but to each 100,000 parts of which three parts of nitrite of soda were added. The importance of this last experiment will be evident when it is stated that nitrite of soda is rarely absent from the different waters supplied to London. The amount of oxygen consumed by the organic matter was determined for two different periods of time, *viz.*:—first, for a period at the end of which the acidulated and contaminated water remained tinted with permanganate for ten minutes after the addition of the latter; and secondly, for a period of six hours, during the whole of which time the permanganate was present in excess.

The results are contained in the following table, where they are compared with the known amount of organic matter present and the known amount of oxygen which that organic matter would require for its complete oxidation.

1 Name of Substance, 3 parts of which were contained in 100,000 parts of water.	2 Oxygen absorbed in 10 minutes. (Experiment.)	3 Oxygen absorbed in 6 hours. (Experiment.)	4 Oxygen required to oxidize organic matter. (Calculated.)	5 Amount of organic matter present. (Calculated from Column No. 2).	6 Amount of organic matter present. (Calculated from Column No. 3.)	7 Amount of organic matter actually present.
Gum Arabic . . .	·0102	·0350	3·55	·082	·280	3·0
Cane Sugar . . .	·0064	·0152	3·37	·051	·111	3·0
Starch	·0143	·0302	3·55	·114	·241	3·0
Gelatin	·0792	·1836	6·76	·634	1·469	3·0
Creatin	·0080	·0172	6·59	·064	·138	3·0
Alcohol	·0093	·0164	6·26	·074	·131	3·0
Urea	·0092	·0119	6·40	·074	·095	3·0
Hippuric Acid . . .	·0328	·0600	5·90	·262	·480	3·0
Oxalic Acid (crystallized)	·3747	·3750	·38	2·998	3·000	3·0
Nitrite of Soda . .	·6910	·6913	0·00	5·528	5·530	0·0

From this table it is seen that of the nine kinds of organic matter operated upon, only one was completely oxidized by permanganate of potash, even after the lapse of six hours, whilst it will be remarked that urea, hippuric acid, and creatin—three organic substances likely to be present in water recently contaminated with sewage—suffer an oxidation which, even in the most favourable case, only reaches $\frac{1}{30}$th of complete oxidation; whilst if we attempt to calculate the amount of these substances present in the water, from the quantity of oxygen so absorbed, instead of finding three parts of each in 100,000 of water, we obtain only ·138 part of creatin, ·095 part of urea, and ·480 part of hippuric acid. On the other hand, the mineral salt, nitrite of soda, weight for weight, surpasses every form of organic matter experimented upon in the avidity with which it absorbs oxygen; and three parts of this inorganic substance in 100,000 of water would actually, by the mode of calculation above described, indicate no less than 5½ parts of organic matter. Thus it is evident, that for the estimation of the amount of organic matter in water or the quantity of oxygen necessary to oxidize that organic matter, permanganate of potash is utterly untrustworthy and fallacious. Whilst, however, this reagent is quite worthless for the quantitative estimation of organic matter in water, it may still be used in certain cases as a qualitative test where there is no opportunity for accurate analytical examination. Thus, if a clear and colourless water decolorizes much of the permanganate solution, the water ought to be rejected for domestic use as being of *doubtful* quality; for although such a water may be absolutely free from organic impurity, yet its decolorizing action upon the permanganate would indicate with considerable certainty that the water had been in contact with decaying animal matters. Should the water, however, instead of being colourless, be tinged of a yellow or brownish-yellow colour when viewed through a considerable stratum, as in a quart decanter for instance, its capability of decolorizing a considerable amount of permanganate solution ought not to be regarded with the same suspicion as in the case of a colourless water, because the yellow tint of such waters is generally owing to the presence of peaty matter, which, though innocuous, has the power of decolorizing permanganate of potash.

Having thus convinced myself of the fallacy of the permanganate process of analysis, and there being no other method by which the estimation of organic matter in waters can be even approximately effected, I have, for some months past, in conjunction with my pupil, Mr. Armstrong, been endeavouring to remedy this grave defect in water analysis; and we have at length succeeded in replacing the unsatisfactory item of "organic and other volatile matter," by certain precise and definite determinations, which throw great light upon the present condition and previous history of the sample of water submitted to analysis.

The two most important things to be ascertained about a water used for domestic purposes are, first—the amount and character of the organic matter present in the water at the time of analysis; and secondly, the amount of hardening or soap-destroying materials which the water contains. Unfortunately, the first of these data cannot be ascertained; but we have devised processes by which the amount of the two most important elements of organic matter, carbon and nitrogen, can be determined with considerable precision. For this purpose the following processes are necessary:—

1. Determination of the carbon contained in the organic matter. To distinguish this carbon from that which is contained in the mineral carbonates present in most waters, I will term it *organic carbon.*

2. Determination of the total combined nitrogen. This nitrogen may exist in the water in one or more of the three following forms:—*a.* As a constituent of organic matter—*organic nitrogen.* *b.* As a constituent of mineral nitrites and nitrates. *c.* As a constituent of ammonia.

3. Determination of the nitrogen present as nitrites or nitrates.

4. Determination of ammonia.

5. Calculation of amount of organic nitrogen. This is obviously a very simple operation, for if from the amount of total combined nitrogen (determination No. 2), there be deducted the amount of nitrogen present as nitrites and nitrates (determination No. 3), plus the amount of nitrogen present in the ammonia (determination No. 4), the remainder will be the amount of organic nitrogen.

The processes by which these determinations are made will be fully described elsewhere.

The organic matters containing nitrogen which occur dissolved in water, are chiefly, if not entirely, of animal origin, being derived either from sewage or manured land; be their origin, however, animal or vegetable, no distinction founded upon their source can be drawn between their respective noxious qualities. After admixture with spring or river water, these noxious organic matters undergo slow oxidation, by which they are finally resolved into comparatively innocuous mineral compounds; their carbon is converted into carbonic acid, and their hydrogen into water; and these products can no longer be identified in the aërated waters of the river or spring: but the nitrogen is converted into nitrous and nitric acids, which, combining with the bases contained in most waters, remain dissolved, and constitute a record of the sewage or other analogous contamination to which the water has been subject. With certain corrections, presently to be mentioned, the analytical determination of the nitrogen contained in these salts and in the form of ammonia writes, as it were, the history of the water, as regards its contact with decomposing animal matter. Such *previous organic contamination* may be conveniently expressed in parts of average filtered London sewage, which, if thus completely oxidized in a river, would yield a like amount of nitrogen, in the form of nitrites, nitrates, and ammonia. For this purpose, average filtered London sewage may be taken as containing 10 parts of combined nitrogen in 100,000 parts, as deduced from the numerous analyses of Way, Hofmann, and Witt. The number so obtained as the *previous sewage contamination* of a water requires, however, a correction, since rain-water itself contains combined nitrogen as ammonia, nitrite of ammonia, and nitrate of ammonia. The amount of these substances present in rain which falls at Rothampstead has been most carefully determined by a laborious series of monthly analyses, made independently on the one hand by Messrs. Lawes and Gilbert, and on the other by Professor Way, and extending over two years. The results of these chemists accord well, and they give as the average amount of nitrogen in the forms of ammonia, nitrite of ammonia, and nitrate of ammonia, ·0985 part in 100,000 parts of rain-water. This must be deducted therefore from the calculated amount of previous sewage contamination of any sample of water. It corresponds to 985 parts of previous sewage contamination in 100,000 parts of the water. There is no doubt that this reduction is too large, and therefore favourable to the character of the water, since in most cases but a very small proportion of the water of a river or spring falls as rain directly into the stream; and Professor Way has proved that almost every trace of the ammonia contained in rain-water is absorbed when the water percolates through cultivated soils. Now, as three-fourths of the combined nitrogen in rain-water is in the form of ammonia, it is evident that rain-water must be deprived of much of its original nitrogenous contamination before it reaches such a river as the Thames. The very small amount of combined nitrogen found in natural waters of undoubted purity, such as that of Loch Katrine for instance, also testifies to the liberality of the above allowance. The water of Loch Katrine contains only one-third as much combined nitrogen as that present in rain falling at Rothampstead, so that, starting from the base line of purity above proposed, the water of Loch Katrine exhibits a *negative* previous sewage contamination equal to 575 parts in 100,000; or, in other words, it would require 575 parts of average London sewage to be added to, and allowed to oxidize in each 100,000 parts of Loch Katrine water before its purity would be reduced to the standard with which I propose to compare the metropolitan waters. It is necessary here to remark, however, that owing to the more copious rains of the Highlands of Scotland, the rain-water of that district probably contains less combined nitrogen than that which falls at Rothampstead.

The nitrogenous organic matter which has escaped the process of oxidation above described, and which therefore still exists in the water at the time the analysis is made, constitutes what may be appropriately termed the *present sewage contamination* of the water. The existence of this contamination is shown by the presence of organic nitrogen in the water, and its amount may be expressed by the number of parts of average filtered London sewage (of the strength above described); which if contained in 100,000 parts of pure water, would contaminate the latter with the same amount of combined nitrogen. By operating upon one litre of water for the determination of total combined nitrogen, one per cent. of sewage can be detected with certainty; but smaller percentages ought, in operations upon such a small quantity of water, to be considered as falling within the possible errors of experiment. Thus in the table of analytical results given below, the indications of organic nitrogen, and consequently of present sewage contamination, amounting in the maximum to one-half per cent., ought to be disregarded; because as the total combined nitrogen was determined in one litre of each water, the amount of present sewage contamination indicated by the analysis falls within the limit of possible experimental error. The subjoined tabulated results obtained in the analysis of samples of the metropolitan waters collected in February last and during the present month, show therefore that none of these waters contained as much as one per cent. of present sewage contamination. This search for unoxidized sewage, or its equivalent in a water, may be rendered more minute by operating upon a larger volume of water, by which the possible error of experiment is reduced in proportional amount: thus, if 10 litres of water be used for the determination of total combined nitrogen, one-tenth of a per cent. of present sewage contamination can be ascertained with certainty. This operation has been performed upon 10 litres of the Thames water delivered by the Grand Junction Company, and collected during the present month; and it is satisfactory to find that this minute examination failed to detect any actual sewage contamination, consequently the sample of the Grand Junction Company's water operated upon did not contain $\frac{1}{1000}$th of its volume of unoxidized sewage. It must be consolatory to the drinker of Thames water to know, that although, according to Mr. Bateman, the population within the basin of the Thames above the points at which the water is withdrawn for the supply of London exceeds 1,000,000 persons, the drainage of some

600,000 of whom is poured into the river, the whole of this fæcal matter is so completely oxidized before it reaches the water-cisterns of London, as to defy the detection of any trace in its noxious or unoxidized condition. If the average flow of Thames water just above the point at which the London companies withdraw their supply be taken at 800,000,000 of gallons daily, the drainage of 600,000 people ought to produce a sewage contamination of 2250 parts in 100,000. It could scarcely be expected that this calculated number should approximate very closely to that obtained by the actual analysis of Thames water, since the calculated number depends upon many contingencies, as for instance, upon the volume of water actually flowing past the points of withdrawal at the time the companies abstracted the water analyzed; and secondly, upon the greater or less retention of the fæcal matters in the sewers of the towns draining into the river: it is interesting, however, to find that the sewage contamination of Thames water as determined by analysis does not differ much from that calculated from the above data. The analytical table given below shows that the average previous sewage contamination of the water delivered by the five companies drawing their supply from the Thames during the months of February and March, 1867, was 2466 parts in 100,000 of water, the amount calculated from the number of persons draining into the river being as just mentioned 2250 parts in 100,000 of water. As summer advances and aquatic vegetation becomes vigorous in the bed of the Thames and its tributaries, this coincidence of calculated and analytical results will doubtless be disturbed; as the water plants can scarcely fail to withdraw an appreciable amount of nitrates and nitrites from the water, thus diminishing the amount of combined nitrogen, and consequently of previous sewage contamination, as determined by analysis.

The second important class of impurities contained in water used for domestic purposes consists, as above mentioned, of certain mineral salts which possess the power of decomposing soap. These substances are the hardening or soap-destroying constituents of waters; from a purely sanitary point of view they are of less direct importance, than the organic impurities, still by rendering efficient ablution and thorough cleanliness difficult of attainment, they doubtless indirectly affect the health of communities supplied with waters in which they are present in considerable quantities. The chief hardening ingredients in potable waters are the salts of lime and magnesia. These salts decompose soap, forming curdy and insoluble compounds containing the fatty acids of the soap, and the lime and magnesia of the salts. So long as this decomposition goes on the soap fails to produce a frothiness in the water, and is useless as a detergent; but when all the lime and magnesia salts have been decomposed by the action of the soap, the slightest further addition of the latter produces a lather when the water is agitated, but this lather is again destroyed by the addition of a further quantity of the hard water. Thus the addition of hard water to a solution of soap, or the reverse of this operation, causes the production of the insoluble curdy matter above mentioned. Bearing this in mind, it is easy to understand the process of washing the skin with soap and hard water, which may be thus described:—1st, the skin is wetted with the water, then soap is applied; the latter soon decomposes all the hardening salts contained in the small quantity of water with which the skin is covered, and there is then formed a strong solution of soap which penetrates into the pores of the skin. This is the process which goes on whilst a lather is being produced in washing, but now the lather requires to be removed from the skin. How can this be done? Obviously, only in one of two ways, *viz.* by wiping it off with a towel, or by rinsing it away with water. In the former case the pores of the skin are left filled with soap solution, in the latter they become plugged up with the greasy curdy matter which results from the action of the hard water upon the soap solution occupying the pores of the cuticle. As the latter process of removing the lather is the one universally adopted, the operation of washing with soap and hard water is perfectly analogous to that used by the dyer or calico-printer when he wishes to fix a pigment in the pores of any tissue. He first introduces into the tubes of the fibre of calico, for instance, a liquid containing one of the ingredients necessary for the formation of the insoluble pigment, this is followed by another liquid containing the remaining necessary ingredients; the insoluble pigment is then produced within the very tubes of the cotton fibre, and is thus imprisoned in such a manner as to defy removal by subsequent washing. The process of ablution, therefore, in hard water is essentially one of dyeing the skin with the white, insoluble, greasy and curdy salts of the fatty acids contained in soap. The pores of the skin are thus blocked up, and it is only because the insoluble pigment produced is white that such a repulsive operation is tolerated. To those, however, who have been accustomed to wash in soft water, the abnormal condition of the skin thus induced is for a long time extremely unpleasant.

Nevertheless, opinion is not quite unanimous as to the advantages of soft water over hard: some persons consider hard water to be necessary for the supply of the calcareous matter of the bones; others believe soft water to be peculiarly liable to attack and dissolve the lead of the pipes through which it is conveyed, or of the cisterns in which it is stored.

An examination of the grounds upon which these opinions are based would completely refute them; but the limits of this discourse do not permit of such a digression, and I must therefore content myself with a mere allusion to one or two facts in connection with them. First, as to the necessity of hard water for the supply of the calcareous matter of bones. If it be assumed that a man drinks daily half-a-gallon of Thames water, he obtains from it 3¼ grains of lime, chiefly in the form of chalk. This amounts to not quite three ounces per annum, which does not seem to be a very large contribution to bony matter. Now suppose the use of this water to be discontinued, and that no part of it is replaced by bitter beer, which always contains far more lime in a given volume than Thames water; but we will assume that the individual consumes one-third of a pint of milk per day, he then receives in this quantity of milk more lime than his system can acquire from two quarts of Thames water. Then as to soft water attacking and dissolving lead and thus becoming poisonous; it is by no means true, as a general proposition, that soft water does attack and dissolve this metal. The very soft water of Loch Ness, as supplied to Inverness, does not attack lead, as evidenced by the condition of the lead pipes which I now produce, and through some of which that water flowed for six years; neither does the soft water of Ennerdale Lake, supplied to Whitehaven, attack lead. Even those soft waters which do attack the metal, such as those now supplied to Glasgow and Manchester, only do so when the surface of the lead is clean and bright. The action soon ceases; in fact as soon as the metal becomes tarnished the pipes are protected; and no complaints of any symptoms of lead poisoning have, for the past ten years, been heard from these large cities. Lastly, a sample of very soft water, taken from one of the principal streams from which it is proposed to supply London, has no action even upon clean and bright lead. Notwithstanding the numerous researches made in connection with this subject, the causes of the attack of lead by water have not yet been completely elucidated; it has, however, been established that the presence of oxygen and the comparative absence of carbonic acid in the dissolved gases are essential conditions to this action. Messrs. Graham, Miller, and Hofmann, in their report on the Metropolitan Waters in 1851, first showed that carbonic acid, when dissolved in water, was a complete protection against lead contamination, and from a series of experiments recently made, I find that two volumes of carbonic acid dissolved in 100 volumes of water, completely protect even distilled water from such contamination. Rain water as it descends to the earth dissolves atmospheric gases, and this solution is afterwards continued in brooks and rivers. Of the chief atmospheric gases carbonic acid is by far the most soluble; 100 volumes of pure water can dissolve 100 volumes of this gas; oxygen, on the other hand, only dissolves to the extent of 3 volumes in 100 volumes of water. Nevertheless, owing to the much larger proportion of oxygen than of carbonic acid in atmospheric air (500:1), water takes up oxygen more rapidly than carbonic acid, and hence freshly fallen rain-water acts upon lead; but when the water flows a great distance through an open conduit, the carbonic acid absorbed finally reaches the protecting proportion, and the action upon lead ceases, although the water retains its original softness. Hence there is no necessary connection between soft water and lead corrosion. Even distilled water, left in contact with the air, for some time, loses its property of acting upon lead.

The third class of impurities present in potable waters, *viz.* matters which are not expelled at a red heat, and which do not decompose soap, require no detailed notice; they consist chiefly of salts of the alkali metals, such as the sulphates and chlorides of potassium and sodium. Unless present in excessive quantity they are innocuous, both as regards the internal and external use of the water.

We are now in a position to understand the following table, which contains the results of the analytical examination of the waters supplied to the metropolis during the past two months:—

Quality of the Waters supplied to the Metropolis during the Months of February and March, 1867.

1 Names of Companies.	2 Total solid impurity in 100,000 parts of Water.		3 Organic Carbon.		4 Nitrogen as Nitrates and Nitrites.		5 Ammonia.	
	Feb.	March.	Feb.	March.	Feb.	March.	Feb.	March.
Thames.								
Chelsea	28·58	30·96	·433	·185	·337	·352	·004	·004
West Middlesex . .	28·68	30·26	·340	·245	·356	·313	·006	·008
Southwark and Vauxhall	29·08	31·22	·293	·256	·357	·344	·005	·005
Grand Junction . .	29·44	31·54	·417	·311	·322	·345	·004	·004
Lambeth	29·36	32·10	·423	·289	·341	·341	·005	·008
Other Sources.								
New River . . .	29·72	27·70	·272	·284	·350	·332	·003	·004
East London . . .	33·56	30·36	·293	·270	·357	·320	·004	·004
Kent	39·84	39·90	·088	·114	·421	·417	·008	·004
South Essex . . .	38·32	37·68	·143	·185	·844	·851	·007	·005
Water from Loch Katrine as supplied in Glasgow . . .	3·28	—	·256	—	·031	—	·002	

Names of Companies.	6 Total combined Nitrogen.		7 Previous Sewage contamination.		8 Hardness.		9 Soap destroyed.	
	Feb.	March.	Feb.	March.	Feb.	March.	Feb.	March.
Thames.								
Chelsea	·371	·355	2420	2565	16·2	18·3	194·4	219·6
West Middlesex . .	·412	·319	2630	2205	16·2	18·9	194·4	226·8
Southwark and Vauxhall	·361	·348	2630	2495	16·8	19·1	201·6	229·2
Grand Junction . .	·325	·348	2270	2495	17·1	19·4	205·2	232·8
Lambeth	·356	·347	2470	2485	16·0	18·5	192·0	222·0
Other Sources.								
New River . . .	·396	·335	2540	2365	18·5	16·8	222·0	201·6
East London . . .	·392	·323	2620	2245	18·8	18·3	225·6	219·6
Kent	·428	·420	3300	3215	23·1	23·0	277·2	276·0
South Essex . . .	·850	·855	7520	7565	21·1	21·4	253·2	256·8
Water from Loch Katrine as supplied in Glasgow . . .	·041	—	0	—	·3	—	3·6	

The numbers in columns 2, 3, 4, 5, 6, 7, 8, and 9, all relate to 100,000 parts of the waters. Column No. 2 shows the total solid impurity contained in each water as delivered from the Company's mains. No. 3 gives the amount of carbon contained in the organic matter present in this solid impurity. No. 4, the amount of nitrogen in the form of salts of nitric and nitrous acids. Column No. 5 shows the amount of ammonia present in each sample, and column No. 6 records the total amount of nitrogen in the several forms of nitrogenous organic matter, salts of nitric and nitrous acids and ammonia, whilst column No. 7 exhibits the previous sewage contamination estimated as above described. Column No. 8 shows the hardness of each water as estimated by the soap test; that is, the number of parts of carbonate of lime, or its equivalent of other hardening salts contained in 100,000 parts of the water. Finally, column No. 9 gives the amount of soap which it is necessary to add to 100,000 parts of each water before a lather can be produced, this amount of soap being thus wasted or destroyed in decomposing the hardening constituents of the water. It is usual to call each part of carbonate of lime or its equivalent of other hardening material in 100,000 parts of water, a *degree of hardness.** Each degree of hardness indicates the destruction of 12 parts of the best hard soap by 100,000 parts of water.

As an example of the mode of reading the above table, we may take the Chelsea Company's water, 100,000 lbs. of which contained, in the month of February last, 28·58 lbs. of solid impurity; the organic matter constituting a portion of this impurity contained 0·433 lb. of carbon. This solid impurity also contained 0·337 lb. of nitrogen in the shape of nitrates and nitrites, besides 0·004 lb. of ammonia; whilst the total amount of combined nitrogen in every form was found to be 0·371 lb. The above quantity of water as supplied by the Chelsea Company had been, after its descent to the earth as rain, contaminated with sewage or manure matter equivalent to 2420 lbs. of average London sewage. By gradual oxidation, partly in the pores of the soil, partly in the Thames and its tributaries, and partly in the reservoirs, filters, and conduits of the Chelsea Water Company, this sewage contamination had been entirely converted into comparatively innocuous inorganic compounds before its delivery to consumers. Finally, 100,000 lbs. of the said water contained 16·2 lbs. of carbonate of lime, or their equivalent of other hardening ingredients; whilst, if the water were used for washing, 100,000 lbs. of it would occasion the waste of 194·4 lbs. of the best hard soap.

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* The degree of hardness more usually employed by chemists is that first proposed by Dr. T. Clark, *viz.* one grain of carbonate of lime or its equivalent, in one imperial gallon of water, or one part in 70,000 of water. The degrees in the above table harmonize better with the decimal arrangements of the rest of the analytical results: they are readily converted into Clark's degrees by multiplying by 7, and then moving the decimal point one place to the left.

For the purpose of comparison I have also appended to the above table the results yielded by Loch Katrine water, as delivered in Glasgow, when submitted to the same analytical processes. A glance at the table will show how vastly superior is the quality of this water as compared with the best at present supplied to London. 100,000 lbs. of this water contain but 3·28 lbs. of solid impurity; it has no sewage contamination, previous or present, and it has only 0·3 degree of hardness, occasioning the destruction of only 3·6 lbs. of soap by 100,000 lbs. of the water.

Such is the chemical history of the water at present supplied to the metropolis; and it must be borne in mind that, grave as are its defects, the mode of the delivery of this water to consumers is still more defective. That in a densely-populated city, water should be delivered only once and for a few minutes in twenty-four hours, and not at all on Sundays, is a condition of things utterly incompatible with the supply of wholesome and palatable water. Even if the water of Loch Katrine itself were delivered in London according to the system at present adopted by the metropolitan water companies, it would infallibly be rendered unfit for human consumption after twenty-four hours' exposure to the vile atmosphere and sewer gases in which the water cisterns of London are systematically placed.

The fundamental defects of our present water supply may be thus summed up:—

1. Great previous sewage contamination.
2. Liability to present sewage contamination.
3. Great hardness.
4. Intermittent supply.

Quality of the proposed Metropolitan Water Supply.—The waters from the sources of the Severn and from the Cumberland Lakes have not yet been submitted to the above analytical processes, and it is therefore impossible to compare them in all respects with the present metropolitan supply. The water of the Bala Lake, in North Wales, which may be regarded as similar to that which would be supplied by Mr. Bateman's scheme, was examined by the late Dr. R. D. Thomson, and the waters of the Cumberland Lakes have been more elaborately investigated by Professor Way. From the analyses of these chemists the following numbers are calculated:—

	Total solid impurity in 100,000 parts.	Hardness.	Soap destroyed.
Bala Lake	2·97	1°·1	13·2
Hawes Water	5·70	2°·9	34·8
Ullswater	5·94	3°·0	36·0
Thirlmere	5·16	2°·1	25·2

A comparison of these numbers with those given in the previous table exhibits the great superiority of the proposed waters over those at present supplied to London as regards total solid impurity and soap-destroying ingredients, whilst it can scarcely be doubted that waters obtained from such sources will be as free from deleterious organic contamination as that of Loch Katrine.

Amelioration of present Water Supply.—In the event of a new source of water supply being at once determined upon, at least seven years must elapse before it can be rendered available to the metropolis; it therefore becomes important to inquire how far it is possible in the interim to ameliorate our present supply. The first and most obvious improvement would be the substitution of the *constant* for the *intermittent* system of delivery. With certain restrictions, all the metropolitan companies express their willingness to make this change, and with the unanimity of opinion regarding its advisability it is difficult to account for the delay in effecting it, unless it arise from the paltry cost involved in the alteration of the present fittings, which would fall upon the landlords of small tenements. Most towns of importance in Great Britain have been long supplied with water on the constant system; why, then, is this boon denied to London, where it is much more urgently required? Until this alteration is effected it is, for the bulk of the population, almost useless to improve the quality of the water. Where a supply for one or even two days has to be stored in a filthy butt, exposed to the foul atmosphere of a crowded court or alley, good and wholesome water can never reach the lips of the consumers.

The most formidable danger arising from the use of the present water supply is undoubtedly the liability to actual sewage contamination, such as that which there is every reason to believe destroyed so many lives in the east of London last summer. How can we best protect ourselves against this noxious contamination? The answer is, there is no absolutely reliable protection. Filtration through animal charcoal is perhaps the best safeguard; but I have shown that this process fails to remove from water the matter which is believed to constitute cholera poison. Permanganate of potash is also an excellent purifier of water, but there is not the slightest evidence that this agent can destroy cholera poison. Boiling the water for a short time is no guarantee that its noxious qualities are destroyed; for, even on the very probable supposition that cholera and other similar poisons are organic germs, we know that many such germs, especially those which are of a low type, retain their vitality after being boiled in water or even after exposure to a temperature of 248° F. for a considerable time. The late Dr. Lindley mentions the fact of raspberry seeds germinating after being boiled for jam; and as syrup boils at a higher temperature than water, these seeds must have been exposed to a heat considerably higher than that of boiling water. Nearly twenty years ago a curious red fungus or mould (*Oïdium aurantiacum*) attacked the bread of Paris. M. Payen exposed pieces of bread, upon which spores of the fungus had been sown, for half an hour to 248° F. in tubes; the red fungus afterwards germinated, although its vitality was destroyed when the temperature was raised to 284° F. I have undoubted evidence of the production of violent cramps and diarrhœa by the drinking of tea made from water which, previous to boiling, had become contaminated with sewage.

Nevertheless, whilst none of these methods can be *relied upon* for the destruction of noxious organic matter in water, I am far from wishing to discourage their use as measures of precaution. More especially would I recommend filtration through animal charcoal as a most undoubted and valuable means of greatly reducing the amount of organic matter in water. I find that water will readily pass through a stratum of animal charcoal three feet thick, at the rate of 41,472 gallons per day per square foot, the oxidizable organic matter contained in the water being reduced to one-half. Five hundred tons of animal charcoal would be an ample quantity through which thus to pass the whole of the present metropolitan water supply. This at 13*l.* per ton would cost 6500*l.* This charcoal would require to be heated to redness in retorts or ovens, for a couple of hours every six months. It would last for two years, and would then be worth nearly half its original cost as manure.

With regard to the excessive hardness of the London waters, it does not appear that any practicable scheme of amelioration can be contrived. Some twenty years ago a beautiful and very simple process of softening hard waters by the addition of lime was devised by Dr. Clark, of Aberdeen; but although this process has repeatedly been tried by water companies, it has invariably been again abandoned, since, notwithstanding the cheapness of the material employed, the amount of carbonate of lime deposited by the London waters when submitted to this treatment was, in the case of such vast volumes of water, so enormous, as to cause the process to be pronounced impracticable. It is to be feared, therefore, that we must for the present be content to block up the pores of our skins with the greasy curd of hard water; but it is very desirable that the other ameliorations of which I have spoken should be carried out at once, although they ought not to delay the introduction of a water-supply free from sewage contamination. Such a supply is a priceless boon to a community; and relying upon our experience in other cities, it is not too much to hope that its introduction into London would be the means of banishing for ever *epidemic* cholera from the capital of this country.

[E. F.]

GENERAL MONTHLY MEETING,

Monday, April 1, 1867.

William Spottiswoode, Esq. M.A. F.R.S. Treasurer and Vice-President, in the Chair.

Charles John Leaf, Esq.
John Neal, Esq.
Evan Wynne Roberts, Esq.

were *elected* Members of the Royal Institution.

John Brunskill, Esq. and
T. A. Rochussen, Esq. C.E.

were *admitted* Members of the Royal Institution.

The decease of Sir George Everest, C.B. F.R.S. and of Lewis Powell, M.D. F.S.A. Managers of the Royal Institution, was announced from the Chair.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

Trustees of the British Museum—Catalogue of Hebrew Books. 8vo. 1867.
List of Birds. Part 5. Gallinæ. 16to. 1867.
Alcock, Lieut-Colonel F. St. Leger, M.R.I. (the Author)—The Militia, the Nucleus of our Defensive Force. (K 94) 8vo. 1867.
Asiatic Society of Bengal—Journal, No. 136. 8vo. 1866.
Astronomical Society, Royal—Monthly Notices. Vol. XXVII. No. 4. 8vo. 1867.
Chemical Society—Journal for March, 1867. 8vo.
Editors—Artizan for March, 1867. 4to.
Athenæum for March, 1867. 4to.
British Journal of Photography for March, 1867. 4to.
Chemical News for March, 1867. 4to.
Engineer for March, 1867. fol.
Horological Journal for March, 1867. 8vo.
Journal of Gas-Lighting for March, 1867. 4to.
Mechanics' Magazine for March, 1867. 8vo.
Pharmaceutical Journal for March, 1867.
Franklin Institute—Journal, No. 493. 8vo. 1867.
Geological Institute, Royal, Vienna—Jahrbuch. Band XVI. No. 4. 4to. 1866
Held, Dr. Joseph (the Author)—Staat und Gesellschaft. Band III. 8vo. Leipzig. 1865.
Horticultural Society, Royal—Proceedings. Vol. I. No. 7. 8vo. 1867.
Irish Academy, Royal—Transactions. Vol. XXIV. Science. Parts 7, 8. 4to. 1866–7.
Proceedings. Vol. VII. Parts 9, 15. 8vo. 1860–2. Vol. IX. Parts 2, 3, 4. 8vo. 1865–7.

Loseby, E. T. Esq. (*the Author*)—On the Phenomena of Meteors. (O 14) 1867.
Medico-Chirurgical Society, Royal—Proceedings. Vol. V. No. 6. 8vo. 1867.
Meteorological Society—Proceedings, No. 28. 8vo. 1867.
Newton, Alfred Vincent, Esq.—A Display of Heraldry. By Wm. Newton. 8vo. 1846.
Photographic Society—Journal, No. 179. 8vo. 1867.
Royal Society of London—Proceedings, No. 90. 8vo. 1867.
Royal Society of Literature—Transactions, Vol. VIII. Part 3. 8vo. 1866.
Society of Antiquaries—Archæologia. Vol. XL. Part 1. 4to. 1866.
Proceedings. Vol. III. Nos. 1, 2. 8vo. 1865.
Stepney, Colonel Cowell—Papers relating to William, first Earl of Gowrie, and Patrick, his last surviving Son. 8vo. 1867.
Symons, G. J. Esq. (*the Author*)—Symons' Monthly Meteorological Magazine. March, 1867. 8vo.
United Service Institution, Royal—Journal, No. 40. 8vo. 1866.

WEEKLY EVENING MEETING,

Friday, April 5, 1867.

COLONEL PHILIP JAMES YORKE, F.R.S. Vice-President, in the Chair.

WILLIAM PENGELLY, Esq. F.R.S.

On the Insulation of St. Michael's Mount, Cornwall.

THE opinion which at present prevails respecting the antiquity of man is largely ascribable, either directly or indirectly, to the results obtained, in 1858, from the exploration of the celebrated cavern on Windmill Hill, at Brixham, in South Devon. The entrances of this cavern are in the slopes of the hill, 78 feet above the *existing* bottom of the valley immediately beneath, and 100 feet above the level of mean tide. Within the memory of persons still living this valley was fully fifteen feet deeper than it is at present, it having been to that extent filled up by the artificial lodgment of rubbish in order to the formation of the principal thoroughfare to Brixham harbour. Prior to this the tide occasionally flowed up the valley above the point immediately below the cavern entrances.

The natural bottom of the valley consisted of vegetable remains, lying on and rooted in blue clay of unknown depth; being, in fact, a portion of the submerged forest which covers a large part of the bottom of Torbay, and which has been traced seaward to the five-fathoms line. Mixed with the vegetable remains, which are those of such species of plants and trees as still exist in the neighbourhood, have been found the bones of *Elephas primigenius*, *Bos longifrons*, red deer, horse, and wild hog. Similar and coeval forests are well known to exist on the opposite shores of the English, Bristol, and St. George's Channels, as well as in other localities. They everywhere present the same phenomena, among which may be specially mentioned large vertical stumps of trees, with roots and rootlets ramifying to considerable distances through the clay.

From the character and arrangement of the materials found in the cavern, it was obvious that they had been introduced by the action of a small stream of water flowing persistently through it at a time when the bottom of the valley was on the level of the cavern entrances.

From the foregoing statements, it follows that since the cave-earth, containing the bones of extinct mammals and human implements, was carried into the cavern, the following changes have been wrought in the district :—

1st and earliest. The depth of the valley was increased by at least one hundred feet.

2nd. The excavated valley was partially refilled by the lodgment in it of a thick mass of blue clay.

3rd. In this clay grew a forest which afforded food and shelter to numerous animals, some of which belonged to species now extinct.

4th. The entire country underwent a slow, tranquil, and uniform subsidence, to the extent of at least forty feet.

Though the time required for and represented by these changes must be great, it fails to fill the interval between the present day and the earliest traces of man in Devonshire; for since the last adjustment of the relative level of sea and land the waves have cut back the cliffs until they have formed a foreshore which, in some cases even where the rocks are hard and crystalline, is more than half-a-mile in width. This strand constitutes a *fifth* change since the advent of man in south-western England. It is the object of this discourse to show that the commencement of this, the most recent of the changes, must have been long before the Christian era.

It is well known that St. Michael's Mount in Cornwall is an island at every high water, and, with rare exceptions, a peninsula at every low water. Its distance from Marazion Cliff, the nearest point of the mainland, to spring-tide high-water mark on its own strand, is about 1680 feet. The tidal isthmus consists of the outcrop of highly inclined Devonian slate and associated rocks, and in most cases is covered with a thin layer of gravel or sand. At spring tides, in still weather, it is at high water about twelve feet below, and at low water six feet above, the sea level. In fine weather it is dry from four to five hours every tide; but occasionally, during very stormy weather and neap tides, it is impossible to cross from the mainland for two or three days together.

The Mount is an outlier of granite, measuring at its base about five furlongs in circumference, and rising to the height of 195 feet above mean tide. At high water it plunges abruptly into the sea, except on the north or landward side, where the granite comes into contact with the slate. Here there is a small plain occupied by a village; adjacent to which is the harbour, which was built in 1726-7, and is capable of receiving ships of 500 tons burden.

The country immediately behind or north of the town of Marazion, consists of Devonian strata traversed by traps and elvans, and attains a considerable elevation.. The town stands on a small plain, which terminates in a cliff from twelve to twenty feet high. Judging from the cliff, the plain is a subaerial accumulation of fragments of rock derived from the adjacent hill, and embedded, without any approach to regularity of arrangement, in a yellowish clay, which probably forms no more than from 30 to 40 per cent. of the entire mass.

It is obvious that, all other things being the same, the Mount would be a permanent peninsula if the district were raised twelve feet, and always an island if it were six feet lower. It must have been the former during the growth of the well-known adjacent submerged forest; and its insulation was necessarily the result either of the subsidence by which the forest area was carried below the sea level, or of a *subsequent* retreat of the coast line in consequence of the wasting action of the waves.

There can be no doubt that the Marazion plain is somewhat ill-adapted, *if much exposed*, to resist the encroaching tendency of the sea; the vertical cliff in which it terminates suggests the idea that the waves have shorn it of some part of its area, and this suggestion is apparently strengthened by the fact that *in some places* the cliff is bounded by a sea-wall. A careful study of the plain, however, shows that though the space between its margin and some of the Marazion houses is scarcely a yard broad, the wall is so very slender as to indicate that it could never have been intended, and was not expected, to be called on to resist powerful attempts at encroachment. Moreover, several parts of the cliff have never had any artificial protection, and these have not retreated, even to the extent of a single inch, more than those which are defended by the wall.

Again, the only quarter from which destructive waves can be sent to this part of the coast, is that included within the quadrant of the horizon between south-west and south-east, and on this side they are so effectually intercepted by the Mount as to render it probable that the cliffs have wasted scarcely more rapidly than has the natural granitic breakwater which defends them. From information furnished by intelligent natives, familiar with the district during the last seventy-five years, it appears that there has been no loss of area in that time; but east of it, where the Mount affords no shelter, there has been a "great loss of land," which, on being measured under their directions, was found to have been at the rate of about thirty-three feet in a century.

If, from the foregoing data, the retrocession of the sheltered cliffs be taken at ten feet in a century—probably a high estimate—the Mount cannot have become an island within the last 16,800 years; and it must be borne in mind that on the hypothesis of insulation by encroachment, at present under review, the submergence of the forests was still earlier.

Geologists have called attention to the fact that the "Greens," or sand-banks which form the coasts immediately east and west of

Penzance—the former extending almost to the Mount—have wasted at a rate greatly exceeding any of the figures just given. According to Dr. Boase, the western "Green" has, during the last 150 years, lost about a foot in breadth annually. To apply this rate to the Marazion plain, however, would be utterly fallacious, for the "Greens" consist of loose sand exposed to the unchecked fury of the waves. Moreover, there is a difference of opinion as to the cause of this waste. Mr. Edmonds, a native writer of considerable experience, states that "in the course of the year the sea always deposits more than it withdraws. The great cause of the lessening of the banks appears to be the constant abstraction of the adjacent sand and pebbles, between low and high water, for manure, ballast, road-making, building, and other purposes."

Though the hypothesis of insulation by encroachment carries back the era of submergence fully 17,000 years from the present time, the rival supposition, that the severance of the Mount from the mainland was simply the result of the subsidence of the country, leaves the chronology of the event an open question. It may have happened in more recent or in more modern times, but it must not be forgotten that the forests, which the subsidence carried down, go back to the mammoth era.

History, however, affords some evidence on the question. St. Keyna is said to have made a pilgrimage to the Mount, and there to have met St. Cadoc, another pilgrim, about the year 490. An apparition of St. Michael was seen on the Mount in 495, or, as some assert, in 710. It is of no avail to object that, at least, the latter event is improbable. The well-established fact that its occurrence was taught and believed, warrants the opinion that the monkish chroniclers carefully recorded every great event connected with a spot so sacred, and that they would have certainly mentioned so important an occurrence as its severance from the mainland. Nor was the belief in this sanctity of brief duration. In 1044, Edward the Confessor granted a charter to a body of monks already established there, and in 1079, Pope Gregory VII. granted a remission of a third of their penance to all persons who should visit the Church of St. Michael at the Mount, with oblations and alms.

From detailed descriptions still in existence, it appears that the dimensions of the Mount, and its distance from the mainland, were in the 16th and 15th centuries, much the same as at present. Leland (1533 to '40) says, "the cumpace of the roote of the Mont of S. Michael is not dim" (half) "myle about." William of Worcester (1461 to '82) *estimates* the distance from Marazion to the foot of the Mount to be "700 steppys," and as he states that the length of St. Michael's Church was "30 steppys," and of the new chapel was "40 feet or 20 steppys," it is obvious that according to his measurement the church was 60 feet long. Now the church is still intact, and measures 65 feet 3 inches in length, so that, making the corresponding correction, the space between the mainland and the

Mount, instead of 1400 feet, as he *estimated*, would be 1522 feet. It is idle, however, to insist on even a near approach to accuracy in his figures, the probability being that at most he only "stepped" the interspace, and there being no evidence respecting the terminal points of the distance thus roughly measured. Nevertheless, the statement is sufficient to show that the condition of the Mount is now essentially the same as it was four centuries ago, and that the rate of waste has been almost inappreciably slow.

Bishop Lacy, on August 10, 1425, "considering the great losses of vessels and of lives during the storms in Mount's Bay, encouraged the faithful to complete the stone causeway between Marazion and St. Michael's Mount;" whence it appears that the Mount harbour was then the only one in the bay, that a considerable trade was carried on there, that the condition of the Mount was as fully exposed as it is at present, and that the "causeway," apparently begun, was not a mere footpath to be used at low water, but was intended as a permanent protection for ships.

The earliest passage, however, believed to be descriptive of the Mount, is the famous one in Diodorus Siculus (9 B.C.). Having given a description of Britain, that author says, "Now we shall speak something concerning the tin that is dug and gotten there. They that inhabit the British promontory of Belerium" (Land's End), "by reason of their converse with merchants, are more civilized and courteous to strangers than the rest are. These are the people that make the tin, which, with a great deal of care and labour, they dig out of the ground, and that being rocky, the metal is mixed with some veins of earth, out of which they melt the metal, and then refine it; then they cast it into square pieces like a die, and carry it to a British isle near at hand called Iktis; for at low tide, all being dry between them and the island, they convey over in carts an abundance of tin in the meantime. (There is one thing peculiar to those islands which lie between Britain and Europe, for at full sea they appear to be islands, but at low water for a long way they look like so many peninsulas.) Hence the merchants transport the tin they buy of the inhabitants to Gaul; and for thirty days' journey they carry it in packs on horses' backs through Gaul to the mouth of the river Rhone."

From this passage it may be inferred that the account was copied from a description by some one who had visited Britain; that the Iktis was near the Land's End; that no place in the district afforded superior accommodation and shelter for maritime trade; that it was adjacent to the tin country; and that it was the only commercial station in Britain, or that all others were comparatively recent. To these conclusions it may be added that the Mount answers admirably in every respect to the description of the Iktis; that besides it there is no island which can be supposed to have been the spot described by the historian; and that the geographical changes which have taken place in the Land's End district within the last 2000 years have been scarcely appreciable or enormously great according as the Mount is or is not the Iktis.

Notwithstanding the close agreement between them, writers are much divided respecting their identification. It is, perhaps, noteworthy, that those who are conversant with the geology of Cornwall admit the claims of the Mount, whilst most archæologists deny them. The sceptics, moreover, are much divided amongst themselves, some advocating the pretensions of the Isle of Wight; others, those of St. Nicholas Island in Plymouth Sound; the Black Rock at the entrance of Falmouth Harbour, which is submarine from about half-flood to half-ebb, and has never less than three fathoms of water around it; or the Wolf Rock, about 8 miles S.W. of the nearest land in Cornwall, and which is covered at every high water.

Setting aside all other considerations, it seems fatal to the pretensions of the Isle of Wight, St. Nicholas Island, and the Black Rock, that they are immediately adjacent to excellent harbours, of which the traders would probably have availed themselves rather than of semi-insulated stations near them, to which tin could have been taken in carts at certain states of the tide only.

The following are amongst the objections which have been made to the claims of the Mount:—

1. That the tidal strand is too limited to be called a "long way."
2. That the Mount is not large enough for the trade of which the Iktis was the seat.
3. That it is a solitary rock of the kind, whilst Diodorus speaks not of an *island*, but of *islands*.
4. That in the Confessor's Charter it is described as *near* the sea, not *in* it.
5. That in Domesday Book it is stated to have been much larger than it is now.
6. That, according to its ancient British name, it was situated within a wood since the British language was first spoken in Cornwall.
7. That several early authors speak of a great loss of land in the district.
8. That this loss is confirmed by the character of the sea bottom between the Land's End and Scilly, and by articles which have been recovered thence.
9. That it is also confirmed by certain family traditions.

These objections will be considered in the order in which they stand.

1. "Long" and "short" are comparative terms. To a geographer accustomed to the feeble tides of the Mediterranean, a breadth of 1680 feet left dry at low water would undoubtedly appear to be a "long way."

2. It was not the ore, but the smelted tin which was taken to the Iktis. Unless the early trade in this metal greatly exceeded that in modern times, that the Mount was much larger than was required for all the traffic may be safely inferred from the following statement,

made in 1838 by the late Mr. Davis Gilbert, a native and resident of Cornwall, and sometime President of the Royal Society:—"At the foot of the Mount a small pier existed from a time probably anterior to the monastery itself; but, in the early part of the last century a lease on lives was granted to Mr. George Blewett. . . . This gentleman rebuilt the pier on a very enlarged scale, and concentrated here almost the whole commerce of Penwith hundred, which has since his time gone to Penzance and Hayle."

3. The Mount is by no means a solitary rock of its kind. Within seventy miles east of it, there are certainly four that actually are, or probably were within the last 1900 years. precisely similar though slightly larger islands—Looe Island, St. Nicholas Island, the Mewstone, and Borough Island.

4. In the Confessor's Charter the Mount is stated to be "*juxta mare.*" This is usually translated *near the sea*, but it would, perhaps, be more correctly rendered *next*, or, as Dr. Barham observes, *by* the sea, when, in either case, it would be a correct description of the present position of the spot.

5. In Domesday Book (1086), "The Land of St. Michael" in "Cornvalge" is stated at "two hides"—supposed to be not less than 240 acres. At present the Mount measures about seven acres only, and it could have been but very little, if at all larger, in William of Worcester's time. There are, however, at least four St. Michaels in Cornwall, and it is *assumed* rather than *proved* that the St. Michael of the Survey is the Mount. But waiving this point, it is not the acreage of the immediate vicinage, but the property of the Church, wherever situated, which is described.

6. It is frequently asserted that Florence of Worcester, who died in 1188, mentioned the Mount under an old British name which signifies that the spot itself was formerly in a wood. This is incorrect, as Florence does not once allude to the Mount. The error, no doubt, arose from confounding Florence with William of Worcester, who lived fully 350 years later. The alleged British name, which appears to have been first mentioned by Carew in 1602, assumes so many forms, and there is so much uncertainty about its exact import, as to render it improbable that it is of any value as evidence. But accepting the prevalent translation—"the hoar rock in the wood"—three different explanations have been suggested. First, that the name was given by a people who spoke British, and who were contemporaries of the wood which surrounded the Mount. There is no doubt that man existed in south-western England when the forests, now submerged, were sub-aerial, and within one of which the Mount must have stood;—his tools have been found in these forests, and also in the more ancient cavern deposits. But to suppose the name to be older than the subsidence, is to suppose the British language coeval with the mammoth, whose remains have been found in the forests, but not in the lake-dwellings of Switzerland or the kitchen-middens of Denmark, and which, so far as is at present known, did not outlive the Age of

Bronze,—an antiquity so great as to render it eminently improbable that any philologer could now give a trustworthy translation of a language then spoken in this country.

The second suggestion is that the name was not contemporary with the submerged woods, but was given whilst the British language was spoken, in consequence of trees which grew on the Mount itself in its present condition; or because the Marazion plain and adjacent lowlands were formerly well wooded, when the Mount, seen from the sea, or from the opposite side of the bay, would *appear* to be in a wood. It has also been suggested that the "hoar rock" was originally not the Mount itself, but a wood-surrounded rocky cairn on it, and that the name first given to a *part* was ultimately applied to the *whole*. Amongst the objections to each of these guesses there is the fatal one, that, so far as is known, the idea of loss of area is older than the name. As has been stated, the latter is first mentioned by Carew in 1602, whilst William of Worcester, more than a century before, speaking of the Mount, terms it "Monte Tumba antea vocata *le Hore rok in the wold*;" and that he understood it to imply a loss of area is evident from his subsequent statement that the Mount was "originally inclosed with a very thick wood, distant from the ocean six miles, affording the finest shelter for wild beasts." It is obvious that this description can apply only to times anterior not only to the fifteenth, but to the eleventh century; for, whatever may be the exact import of the phrase, the Mount was "*juxta mare*" in 1044. The era of the topography which William described, but of which he records no evidence, was separated from his day by an interval of time wider than that which divides him from us. Leland (1533 to '40) says, "Ther hath been much Land devourid betwixt *Pensandes* and *Mousehole*. Ther is an old Legend . . . . a Toumlet in this Part (now defaced and) lying under the Water." He subsequently states that, "In the Bay betwyxt the Mount and *Pensants* be found nere the lowe Water marke Rootes of Trees yn dyvers Places, as a token of the Grounde wasted," and thus furnishes the earliest known mention of the submerged forest, as well as of evidence of loss of area. Carew (1602) having stated that the Mount is termed by "the Cornishmen Cara Couz in Clowze, that is the hoar Rock in the Wood," adds, in a note, "Tradition tells us that in former ages the Mount was part of the insular continent in Britain, and disjoined from it by an inundation or encroachment of the sea, some earthquake or terrestrial concussion. To prove this opinion, the country people tell us that oak trees have been found under the sand."

The third explanation of the alleged British name is suggested by a consideration of the foregoing quotations.

There is first a tradition of a loss of area which took place at least five centuries previously. Seventy years later the tradition or, as the recorder calls it, the "old legend" is repeated, and with it, and as a proof of its truth, the earliest mention of a submerged forest. Seventy years later still, the same tradition and proof, causes assigned in such terms as to prove *the* cause unknown and the fact unrecorded, and, from materials derived from a language hastening to extinction, there is somewhat clumsily manufactured a name supposed to express the early topography. At low tide the remains of a forest were seen on the strand, in a condition which proved that the trees were *in situ*, and that there had been a subsidence. To the mind's eye the area was re-elevated, the Mount became surrounded with trees = a "hoar rock in a wood" = "*Carreg Luz en Kuz.*"

7. To his other statements, William of Worcester adds, but gives no evidence, that there were "140 parish churches submerged between the Mount and Scilly." Were this assertion accepted it would follow that after Cornwall was christianized (not earlier than the fifth century) and divided into parishes, but prior to the Confessor's Charter, there had been lost 140 parishes, having, according to the existing average in the adjacent hundred of Penwith, an aggregate area of 830 square miles, or twice that required to fill the space between the Cornish coast and a line joining the Lizard with Scilly. A loss so enormous that it is impossible to believe that the monkish chroniclers, laboriously minute as they were, especially in all things affecting the Church, would have omitted to record it, happening, as it must, within or near their own times.

8. Carew, in proof of this subsidence between the Land's End and Scilly, states that "this space carrieth continually an equal depth of forty or sixty fathoms (a thing not usual in the sea's proper dominions)," and that "fishermen also casting their hooks thereabouts, have drawn up pieces of doors and windows." It is not easy to see the force of the first statement; moreover, it exceeds the truth, the depth being from thirty to forty fathoms. It would be awkward, however, to accept this proof, as it would prove also that prior to the submergence there could have been no English Channel. The second statement could only be entertained by one who had never reflected on the power of the Atlantic waves.

9. The legends and crests of the families of Trevillian and Vyvyan are also cited to prove the subsidence. The Herald's Office is a somewhat novel court for the settlement of a question in physical science.

Of those who believe in the comparatively recent insulation of the Mount, a few ascribe it to encroachment of the sea without change of level; but the great majority contend for a general subsidence. As may be expected, the latter differ as to the date of the event. The late Dr. Borlase (1756) favoured the end of March, A.D. 830, when, as the Irish annals say, an inundation happened on the west coast of Cork, but which is not recorded to have visited any other locality. Others prefer the very destructive "great sea flood" which, according to the Saxon Chronicler, Florence of Worcester, and William of Malmesbury, occurred in 1014, on Michaelmas eve, or near the equinox. The prevalent opinion, however, strongly inclines to "a very high tide" mentioned by the same three historians as happening on St. Martin's day, 1099. Malmesbury speaks of it as very destructive on the banks of the

Thames; the other authors speak of the great injury it wrought, but there is no evidence of its extending beyond the Thames. The Chronicler says, "The same day was the first of the new moon;" hence it has been remarked that it was not the highest spring tide, and that, being in November, it was not near the equinox. One of the highest tides, however, which, during the present century, have visited the south-west of England, occurred on the 26th October, 1859, the day of new moon.

It does not fall within the scope of the present discourse to enter on a consideration of such questions as "Did the Phœnicians ever carry on trade with Cornwall by way of the Straits of Gibraltar?" "Where were the Cassiterides?" "Was tin ever wrought in Scilly?" or "Was the tin taken from Cornwall to the continent directly, or coastwise to the Isle of Wight, or some other near point?" With reference to the last, however, it may be remarked, in passing, that a block of tin which, answering well to the description by Diodorus, was dredged up near Falmouth harbour upwards of forty years ago, appears to suggest that the route was, at least, occasionally coastwise.

In conclusion, and by way of recapitulation: St. Michael's Mount has undergone no important change during the last four centuries; there is no sufficient evidence that it has done so since the Christian era; those whose habit and interest it was to record such an occurrence are silent on the question; it affords the requisite shelter, and is abundantly large enough for the storage and shipment of the early Cornish tin, and for the traffic consequent thereon; it possesses all the characters, and occupies the position of the Iktis of Diodorus, and no other existing island has any claim to this distinction; nineteen centuries ago it possessed a safe harbour, so that its insulation must have been effected long before; it was at one time unquestionably a "hoar rock in a wood," but in all probability it had ceased to be so long before any language now known to scholars was spoken in the district. Prior to its insulation, was the era of the growth of the forests now submerged along our entire sea-bord. Before this, again, was the period of the deposition of the blue clay—the forest soil: earlier still, was the epoch of the excavation or re-excavation of the valleys, in whose bounding hills are the caverns of South Devon; and when, still farther into the past, the bottoms of the valleys were at least 100 feet above their present level, the red loam was carried into the caverns, and there were also introduced evidences of contemporary men. Great as is the age of these cavern deposits, it does not exceed the *antiquity of man in Devonshire*, and hence, in all probability, it falls far short of the *antiquity of man*.

[W. P.]

WEEKLY EVENING MEETING,

Friday, April 12, 1867.

Sir Henry Holland, Bart. M.D. D.C.L. F.R.S. President, in the Chair.

Balfour Stewart, Esq. LL.D. F.R.S.
DIRECTOR OF THE KEW OBSERVATORY.

On the Sun as a Variable Star.

The man of science who would extend his inquiries into the remoter regions of the universe is beset from the first, with the following difficulty.

How is he to know that the laws with which he is here familiar hold in those distant regions?

Now, without attempting to discuss the origin of our beliefs, it may be inferred that the same principle which induces us to think that the laws of light which were proved to be true to-day will hold true to-morrow, induces us likewise to believe that what is proved to be true here will hold elsewhere.

But while we are all, without exception, led by an innate belief in unity of design to attribute the same fundamental laws to different regions of the universe, it is only lately that we have been informed of a similarity or rather identity which we had no previous reason to expect.

Spectrum analysis, that very powerful and searching method of investigation, while it informs us of great varieties in the molecular constitution of different regions of the universe, seems to proclaim the fact that the elemental forms of matter are greatly the same throughout, and that familiar substances, such as sodium, magnesium, and iron, with which we are here so well acquainted, form also the staples of other worlds.

A study of binary stars has likewise shown that the law of gravitation is not the peculiar attribute of our solar system, and we may entertain the hope that as our knowledge extends the appearance of resemblance between ourselves and distant regions will extend along with it.

In fine, there can be little doubt that we are to-day in a position to argue with more confidence regarding these regions than we were fifty years ago, and that we may assume that the laws of heat and light are the same throughout the universe. If this principle be allowed, the discussion of the varying brightness of certain stars, and it may be of our own sun, is one which we can approach with great hopefulness.

Indeed, we are now by means of this principle enabled to limit the number of the immediate causes of this change of light, and to say the change must be due to one or other of these causes. Now, when these various causes are examined one by one there is a tendency in the mind to reject certain of these and to prefer others, apart altogether from the results of observation; and this is one of those tendencies which, if it is indulged in with distrust, ought not to be wholly discarded.

But, while attempting to exhibit this evening this method of selection, and to hit upon the most probable cause of variable luminosity, it becomes us all, as disciples of the school of Bacon, to see what observation has to say to our selection; does this ultimate court of appeal confirm our conclusions or does it not?

[The phenomena presented by variable stars were then described; and it was next argued, from the ten-yearly period of sun-spots that our own luminary is a variable star. The photographs of sun-spots were also exhibited on a screen by aid of the electric light.]

Let us now proceed to the subject of the diminution of light which characterizes variable stars, and according to the principles already stated, see what such a diminution most probably implies. Two cases may be discussed under this head, according as the temperature may be supposed to remain constant or to change. First,—If the temperature remains constant, then a change of light implies a change of the surface or of the state of the heated body. This was illustrated by two simple experiments. In the first of these a ball of iron marked with chalk, and a piece of porcelain of a black and white pattern, both heated to redness, were viewed in the dark, and the white of both was found to be less luminous than the black, thereby proving that change of the reflecting power of the surface produces change of luminosity, even although the temperature be the same. In the second experiment it was shown, by means of the oxyhydrogen flame viewed by itself and then directed against a piece of lime, that a solid substance gives out much more heat and light than a gas at the same temperature, and thus that a change of state may be expected to produce a change of luminosity.

If, therefore, a solid or liquid substance change suddenly from a black to a white or reflecting surface, or if it become gaseous, it will decrease in luminosity.

It was then argued that we cannot readily suppose the variable luminosity of certain stars to be caused by a periodical change of surface from white to black and *vice versâ*.

On the other hand, if we assume (as is most probable) that the photosphere of a star consists of incandescent, detached particles similar to a cloud, we may well imagine this cloud-surface to contract or expand in the atmosphere of the star, so as to present a variable area to a distant observer. Mr. De la Rue and some others, including the speaker, entertain a suspicion that something of this kind may possibly take place with regard to our sun, although to a very small extent; but this is not proved. In the meantime, suffice it to say, such an explanation will not account for a variability analogous to sun-spots; and to account for these and similar phenomena, we must look for some other explanation. We may therefore take it for granted that a change of surface or of state, without a change of temperature, will not account for the phenomena of variability, in as far as these are similar to sun-spots.

A change of luminosity may, however, take place in another way, even while the temperature of the hot body remains constant. It may be caused by the interposition of a cold absorbing screen between the source of light and the observer.

Under this head may be ranked one hypothesis regarding stellar variability, which supposed the decrease of luminosity to be caused by a dark body of great size coming between the star and our earth. The probability of such a body being in an exact line between the star and the earth is, however, very small; and as our system is supposed to move in space, we should soon escape its interference, unless its size was supposed to be enormously large. This is not likely; and if stellar variability is caused by such a screen, the screen must be supposed to lie close to the star—in fact, to be connected with the star itself, and to form part of the atmosphere of the star.

The second case of varying luminosity is that in which the temperature may be supposed to change; and here evidently a decrease of luminosity implies a decrease of temperature.

If we imagine that stellar variability is caused by a decrease of temperature, we are led to contemplate two possible causes of this decrease.

In the first place, there may be supposed to be some chemical or molecular change periodically recurring, which produces a marked decrease of temperature. Evaporation on an extensive scale might account for it; but the same objection applies here as before, when we considered change of surface.

We have 'not the shadow of a proof that such processes are periodically going on in the sun or any star; and we do not get rid of our burden by this means, but merely, as it were, shift it from one shoulder to another.

Now, if we do not readily admit that the supposed fall of temperature is produced by some such process, we can only account for it by the redistribution of some previously existing comparatively cold matter; and this comparatively cold matter must be either on one side, under, or above (by under is meant nearer the centre).

It has been supposed by some that this cold matter might be to one side; that, in fact, a star might have one hemisphere cold and the other hot: so that, if it revolved round an axis, the effect produced on a distant spectator would be that of a varying brightness. The difficulty in this idea is two-fold. In the first place, we cannot easily conceive such an extremely artificial distribution of heat; and even if this could be conceived, we could not imagine that it would be a permanent one. This, therefore, is an unlikely hypothesis.

Next, the idea has been entertained that the comparatively cold matter whose re-distribution we are at present supposing, exists below, or nearer the centre of a sun or star than its luminous envelope; and the fact that sun-spots appear to be depressions, has given countenance to this idea; but if we imagine that the sun or star has been for a very long period of time surrounded by this luminous envelope, we shall have great difficulty in imagining the interior to be colder than it. As far as we can judge from terrestrial experiments, a body surrounded with a heated envelope, such as that of the sun, will ultimately have in all its parts the temperature of this envelope. We cannot, therefore, readily assent to this hypothesis, although it might add the sun and stars to the list of habitable worlds.

Now, if the colder matter, whose re-distribution we are supposing, come neither from one side nor from below, it must come from above; that is to say, above the luminous envelope of the sun and stars we must have colder matter.

Thus we see that if we consider the decrease of luminosity to be due to the presence, between the source of light and the observer, of a comparatively cold, absorbing body, we are driven to an atmosphere; and if we consider the decrease of luminosity to be due to a decrease of temperature, we are still driven to look to an atmosphere as the immediate cause.

We must now bring our results to the test of observation, and ask: What reason have we, in the first place, for assuming the existence of solar and stellar atmospheres? and in the second, What reason have we for supposing the decrease of luminosity to be immediately caused by an increased action of this atmosphere? Now, in the sun we have various proofs of the existence of a comparatively cold, absorbing atmosphere above the luminous envelope.

(1.) The existence of dark lines in the spectra of the sun and certain stars denotes the existence in these bodies of a comparatively cold, absorbing atmosphere above the luminous envelope.

(2.) The existence of such an atmosphere surrounding our own luminary is indicated by the fact that the sun's limb is less bright than his centre; this effect being, no doubt, caused by an absorbing atmosphere, and being greatest round the edge, for the same reason that any similar effect of our own atmosphere is greatest near the horizon.

(3.) Finally, the presence of a solar atmosphere extending as far as 72,000 miles above the bright surface, is indicated by the presence of those red flames which occur during a total eclipse of the sun. These red flames have been proved by Mr. De la Rue to belong to our luminary, while, from the nature of the light which they emit, we may infer (although this is not yet proved) that the heated matter is gaseous.

We now come to consider what observational evidence there is that the changes of luminosity in the sun's disc are due to the effects cooling, absorbing, or both together, of a greater or less stratum of atmosphere.

In the first place (under this head), there is abundant evidence that the luminous surface of the sun does not consist of one uniform mass of incandescent, solid, or liquid matter, it is neither land nor ocean, but it is a cloud. The proof of this assertion is derived from the behaviour of certain bright patches, or faculæ, as they are called, which appear near the sun's border, and generally accompany a spot or dark patch. The brightness, when near the border of these faculæ, denotes that they have escaped, in a great measure, the absorbing influence of the solar atmosphere, which influence is very strong near the border; in other words, they are elevated above a great portion of this atmosphere, and as they remain suspended for some time, they cannot be heavy matter. Indeed, masses of luminous matter have been known to sail across a spot evidently above it, and leaving it afterwards quite undisturbed.

Now, if we imagine the bright surface of the sun to be a cloud surface, or surface of condensation of small particles in contradistinction to a solid or liquid heavy surface, we at once, by such an hypothesis, greatly increase the freedom of action. Such a boundary might easily be depressed by the accumulation of an enormous down-rush of cold atmosphere, or it might be raised above its ordinary level, and, generally speaking, would be more impressible than a continuous solid or liquid surface.

Again, if we view a spot as the centre of a disturbance of some kind, it is, in the first place, worthy of remark that the faculæ or bright portions which accompany a spot for the most part fall behind, as far as rotation is concerned, a fact which has been shown in the solar researches of Messrs. De la Rue, Stewart, and Loewy. Now, this would always take place in the case of a body carried from a lower region to a higher one, or from a region possessing less to one possessing greater absolute velocity of rotation. We are, therefore, induced to suppose that faculæ are masses which have been carried upwards from the area of disturbance, and have thus fallen behind.

So much for the up-rush of matter or the ascending current; and now, it may be asked, have we any evidence that a spot is a descending current?

We have evidence of a precisely similar character to that we have for faculæ, and we are entitled to conclude, from the observations of sun-spots made by Carrington, that spots, instead of falling behind, as far as rotation is concerned, move forward as if they had come from a higher region.

Also Lockyer, one of our sun-observers in this country, noticed a piece of matter in the very act of moving down. It was first of all as bright as the faculæ, then it became like the ordinary surface, then it grew dark like the spot itself, still retaining its identity of form.

It would thus appear that the comparatively cold, absorbing atmosphere is accumulated above the area occupied by a spot, while the faculæ are so high up as to escape its influence; and finally, we are led to conclude that all the variations in brightness that appear on the surface of our luminary are due to the presence, to a greater or less extent, of a comparatively cold, absorbing atmosphere.

We thus perceive that the phenomena of variability, as far as these are analogous to sun-spots, are due, most probably, to a greater or less amount of a comparatively cold, absorbing atmosphere.

A down-rush and a corresponding up-rush would thus appear to be the immediate cause of these spots; yet why, it may be asked, have these phenomena a periodicity? Why is there a ten-yearly period of sun-spots besides other probable periods?

At the same time, the following question arises, Why are sun-spots confined to the equatorial regions of the sun, which are also those regions which border upon the ecliptic or plane of the planets' motion?

Arguing possibly from this fact, the illustrious Galileo seems to have imagined a connection between sun-spots and planetary configurations; but he did not publish his ideas, probably from want of evidence.

In order to obtain as much information as possible on this point, Messrs. De la Rue, Stewart, and Loewy have measured the areas of all the spots in Carrington's original pictures, extending from the beginning of 1854 to the end of 1860, and the result deduced from these measurements is favourable to the idea of a connection between the behaviour of sun-spots and planetary configurations.

These results were obtained by noticing that at one period of time all the spots, as a rule, increase towards the centre of the sun's visible disc as they pass over it by rotation, while at another period they all decrease from their first appearance, or perhaps increase from their first appearance. Considering the earth merely as the point of view from which these various phenomena are observed, we have to ask, What is the cause of this peculiar behaviour of sun-spots?

It evidently must refer to something without the sun, and that something is not so very difficult to find. When these phenomena denoting peculiarities in the behaviour of sun-spots are attentively studied, it is seen that every twenty months the same behaviour occurs again. Couple with this the fact that every twenty months the planet Venus returns to the same position with reference to the earth, and we can scarcely help attributing some predominating influence over the behaviour of sun-spots to this planet. A closer analysis of the phenomena observed shows us that this is the case; and that as any portion of the sun's surface retreats by rotation from the neighbourhood of Venus, the spots on that portion have a tendency to increase, attaining a maximum at the point furthest from Venus. Jupiter has also much influence. Now, is it not a very extraordinary circumstance that two planets which are never so near the sun as they are near the earth, should appear to cause phenomena of the vast magnitude of solar spots?

It naturally occurs to us that the sun must be in a most sensitive molecular state, in consequence of which that wonderful mass experiences great changes from very small outward influences. (Experiments were here made, showing examples of this state.) Professor Tait and the speaker have conjectured that the properties of a body, especially with reference to heat, light, and electricity, may be influenced by the neighbourhood of a large body. An influence of this kind would naturally be most powerful upon a body such as the sun, which is of a very high temperature, just as a poker thrust into a hot furnace will cause a greater disturbance of heat than if thrust into a chamber very little hotter than itself.

We have, moreover, very good grounds for supposing the sun to be in a very sensitive molecular state. We may infer from certain experiments, especially those of Cagniard De la Tour, that at a very high temperature and under a very great pressure, the latent heat of vaporization is very small, so that a comparatively small increase of heat will cause a considerable mass of liquid to assume the gaseous form, and *vice versâ*.

We might thus suppose that an extremely small withdrawal of heat from the sun might cause a copious condensation, and this change of molecular state would, through the alteration of reflection, &c., alter, to a great extent, the distribution over the various particles of the sun's surface of an enormous quantity of heat.

Again, convection is very strong in the sun, since the force of gravity is very strong, so that great mechanical changes might very easily result.

[B. S.]

ANNUAL MEETING,

Wednesday, May 1, 1867.

SIR HENRY HOLLAND, Bart. M.D. D.C.L. F.R.S. President, in the Chair.

The Annual Report of the Committee of Visitors for the year 1866 was read and adopted.

The Books and Pamphlets presented in 1866 amounted to 130 volumes, making, with those purchased by the Managers, a total of 407 volumes added to the Library in the Year.

Thirty-eight new Members were elected in 1866.

Sixty-three Lectures and Twenty Evening Discourses were delivered during the year 1866.

Thanks were voted to the President, Treasurer, and Secretary, to the Committees of Managers and Visitors, and to Professor Faraday, and the other Professors, for their services to the Institution during the past year.

The following Gentlemen were unanimously elected as Officers for the ensuing year:—

PRESIDENT—Sir Henry Holland, Bart. M.D. D.C.L. F.R.S.
TREASURER—William Spottiswoode, Esq. M.A. F.R.S.
SECRETARY—Henry Bence Jones, M.A. M.D. F.R.S.

MANAGERS.

Henry Wollaston Blake, Esq. M.A. F.R.S.
George Busk, Esq. F.R.C.S. F.R.S.
Right Hon. Edward Cardwell, M.P.
Right Hon. the Viscount Cranborne, M.P.
Warren De la Rue, Esq. Ph.D. F.R.S. Pres. Chem. Soc.
Right Hon. Sir William Erle, D.C.L. F.R.S.
John Peter Gassiot, Esq. F.R.S.
John Hall Gladstone, Esq. Ph.D. F.R.S.
Cæsar H. Hawkins, Esq. F.R.S.
John Carrick Moore, Esq. M.A. F.R.S.
Sir Roderick I. Murchison, Bart. K.C.B. D.C.L. F.R.S.
Right Hon. the Earl Percy, LL.D.
William Pole, Esq. M.A. F.R.S.
William Frederick Pollock, Esq. M.A.
Colonel Philip James Yorke, F.R.S.

VISITORS.

Charles Beevor, Esq. F.R.C.S.
John Ashton Bostock, Esq.
John Charles Burgoyne, Esq.
George Frederick Chambers, Esq.
Samuel Gaskell, Esq.
Rev. G. Godwin Pownall Glossop, A.M.
Thomas Williams Helps, Esq. M.A.
William Charles Henry, M.D. F.R.S. F.G.S.
Thomas Hyde Hills, Esq.
William Edward Kilburn, Esq.
Edward Henry Moscrop, Esq.
Arthur Giles Puller, Esq.
Edward Owen Tudor, Esq. F.S.A.
Henry Twining, Esq.
Henry Vaughan, Esq.

WEEKLY EVENING MEETING,

Friday, May 3, 1867.

SIR HENRY HOLLAND, Bart. M.D. D.C.L. F.R.S. President, in the Chair.

JOHN STUART BLACKIE, F.R.S.E.

PROFESSOR OF GREEK IN THE UNIVERSITY OF EDINBURGH.

On the Music of Speech in the Greek and Latin Languages.

THE title of this paper indicates a subject too vast for the compass of a single discourse. What I intend to confine myself to on the present occasion, is to show in a few propositions that the system of pronouncing the Greek and Latin languages to which the southern half of the island of Great Britain has long been trained, is scientifically altogether perverse and historically quite unwarranted, and that it mars the music of the most harmonious of the ancient languages to a degree quite inconceivable by those whose ears have been dulled by a long course of arbitrary and barbarous vocalization. And not only are some of the finest features of the classic tongues thus systematically defaced, but the Englishman is put at an unnatural disadvantage with all foreigners, not only as concerns the ancient languages, but likewise in reference to the spoken tongues of Europe. For the peculiar habits of English vocalization being engrained by the long course of classical tuition in our great public schools and universities, the organs of speech often acquire a rigidity which it is difficult afterwards to correct; and an artificial barrier is raised between Latin and those Romanesque languages which are only variations of that tongue with fundamentally the same tone. In these days of travelling and touring it is often most tantalizing to an Englishman to find that his Latin is of no service to him with a Hungarian nobleman or an Italian priest, and his Oxonion Greek as useless in Athens as it would be in Kamschatka. A Scotsman, on the other hand, whose classical training is generally inferior to that of an English scholar, will follow a Latin oration by a Göttingen or Bonn professor without difficulty. The disadvantages of these peculiarities are so great, that with a practical people like the English, they ought to be allowed the greatest weight, independently altogether of the scientific aspect of the matter. Expressions of opinion on this point have been put forth by some of our most distinguished scholars and men of literary eminence. Tennyson, our poet laureate, whose practice in such a region ought to constitute law, never reads Virgil with any other vocalization than the masculine and sonorous one, which belongs to the recitation of the *Divina Comedia*; and Mr. Gladstone, in vol. i. of his 'Homeric Studies,' p. 92, has the following decided declaration. "I should gladly see the day when, under the authority of scholars, improvement might be effected in our solitary and barbarous method of pronouncing both the Greek and the Latin languages. In this one respect the European world may still with justice describe the English as "*penitus toto divisos orbe Britannos.*" To a reform in this method I can see nothing that is opposed, unless it be that spirit of dogged conservatism, which, while in politics it may be sometimes useful, in the region of scientific research and of scholarly practice, is simply absurd. A convention of English, Scottish, and Irish scholars might easily be held in London or Oxford, which could lay down the main lines of necessary remodelment in this domain, after a few hours' consultation. No man who loves the truth, and has even superficially looked into the sources of philological tradition, can doubt for a moment that the peculiarities which distinguish the English enunciation of Latin and Greek are arbitrary and barbarous, and ought to be abolished. And though it be no doubt true that the different European nations have interpolated some of their own orthoepic peculiarities into their recitation of Latin and Greek, it is to be noted that these peculiarities affect only a few secondary articulations; while the English method strikes a fundamentally false keynote, inverts the poles of vocalization, and substitutes a figment for a fact in some of the most important elements on which the music of the classical languages depends.

In what follows, to avoid prolixity, I shall confine my remarks principally to the Greek language; but the scholar will have no difficulty in perceiving that the principles of my method apply to Latin as broadly, and in some points with less liability of contravention, than to Greek.

The subject of the music of speech divides itself naturally into three great sections:

1. Articulation and vocalization: the musical value and significance of the vowels and consonants of which spoken language is composed, and from the combination of which words are made.
2. The accent or intonation of words in utterance.
3. The rhythm or measured cadence to which sentences are made subject, in poetical composition with curious care, and with a more arbitrary sweep of harmony in prose.

The comparative musical excellence of a language depends of course on the completeness and richness of its whole elements of utterance, and of the cunning displayed in their combination. But for my present purpose, it will be sufficient to notice the quality of the vowel sounds, on which the sonorous value of a language mainly depends. Now with regard to the pronunciation of the Greek vowels we have a distinct and clear utterance of a rhetorician of the highest authority, which, if it had been duly weighed by English scholars, would long ago have furnished a sure basis on which to raise a true scale of vowel variation for the Greek and Latin languages.

In the fourteenth chapter of his well-known treatise περὶ συνθέσεως ὀνομάτων, Dionysius of Halicarnassus writes as follows:—

"There are seven vowels; two long, η and ω, and two short, ε and ο; three both long and short, α, ι, υ. All these are pronounced by the wind-pipe acting on the breath, while the mouth remains in its simple natural state, and the tongue remaining at rest takes no part in the utterance. Now, the long vowels, and those which may be either long or short, when they are used as long are pronounced with the stream of breath extended and continuous; but the short vowels, and those used as short, are uttered by a stroke of the mouth cut off immediately on emission, the windpipe exerting its power only for the shortest time. Of all these, the most agreeable sounds are produced by the long vowels, and those which are used as long, because their sound continues for a considerable time, and they do not suddenly break off the energy of the breath. Of an inferior value are the short vowels, and those used as short, because the volume of sound in them is small and broken. Of the long again, the most sonorous is the α, when it is used as long, for it is pronounced by opening the mouth to the fullest, while the breath strikes the palate. The next is η, because in its formation, while the mouth is moderately open, the sound is driven out from below at the base of the tongue, and keeping in that quarter does not strike upwards. Next comes the ω, for in it the mouth is rounded, and contracts the lips, and the stroke of the mouth is sent against the extreme end of the mouth (ἀκροστόμιον, the lips, I presume). Inferior to this is the υ, for in this vowel an observable contraction takes place in the lips, so that the sonorous breath comes out attenuated and compressed. Last of all comes ι, for here the stroke of the breath takes place about the teeth, while the opening of the mouth is small, and the lips contribute nothing towards giving it dignity as it passes through. Of the short vowels, neither is sonorous; but ο is the least disagreeable, for it parts the mouth more than the other, and receives the stroke nearer the windpipe."

Now not to go into a detailed criticism of this passage, two things are obvious on the surface of it, and incontestable: that the sound of the first letter of the alphabet here indicated is not that of the English *a* in *nation*, but certainly that of the long Italian *a*, or the fine hollow English *a* in *hall;* and secondly, that the sound of the last vowel here mentioned, *i*, is the most slender of all, corresponding to the thin sound of *ea* in *cheap*, not to the broad diphthongal song of *i* in *time*. For there can be no doubt, as Walker remarks, that the peculiar English sound of the long *i*, by which the letter is named, is a diphthong, composed of the *broad a* followed by an *e*. Thus our English orthoepy has achieved the monstrous result, that while we have attenuated the broadest sound in the ancient vowel scale, we have broadened the most attenuated—a perversity equalled only by the strange habit which vulgar English speakers have of inserting an *r* or a *h* where no *r* or *h* ought to be, and sinking the same letters where they ought to be pronounced. What a wretched effect this inversion of the points of polarity in the vowel scale must have in marring the music in some of the finest lines in Greek and Latin poetry need not be mentioned. Let a man only try it in Italian, and he will see how unpardonable such an offence is.

A very long and elaborate method of proof would be necessary, were it my purpose on the present occasion, to corroborate the values of the Greek vowels as here given by Dionysius. Let it be enough to state that Seyffarth, Liscov, and other writers have corroborated the grand outlines of the scheme here given by the various methods of philological induction appropriate to this branch of investigation; and as the result of the whole, it appears that the proper classical values of the long vowels may be stated as in the following scale:—

ᾱ, as in Italian *amāre;*
η, as in English *māne;*
ω, as in English *boat;*
υ, as in German *brüder;*
ι, as in English *cheap;*

the short vowels, of course, being the same sounds pronounced sharply, as in the *staccato* notes of music. The practice followed in some English schools of pronouncing the short ă like *a* in *hat*, and the long ā like *ā* in *mane*, which is a quite different vowel, must be reprobated. The long *a* should always be pronounced like the English *aw* or *au*, as in *cawl*, *maul*, &c.

With regard to the diphthongs, it is a most unfortunate circumstance that Dionysius has not thought it necessary to describe their sounds. The inference which some have drawn from this, that the rhetorician considered their pronunciation was always identical with the elements of which they are composed, so that αι or καὶ should be like the English *i* in *time*, pronounced in the broadest style is altogether unwarranted. With regard to two of the diphthongs, however, philological investigations leave us in no manner of doubt that the received English pronunciation is altogether wrong. It seems quite certain that ου was pronounced like the English *oo* in *boom ;* and this sound is not only more beautiful in itself than the English *ou* in *bound*, but is in accordance with the whole habit of Greek utterance, which inclines to the use of the front part of the mouth, while the English speech comes more from the back part, as the American is apt to lose itself after a most ungraceful fashion, in the nose. It seems certain also, that in the time of the Ptolemies αι was always pronounced like *a* in the English *vain*, as it is still pronounced by the modern Greeks.* On the phonic value of the other diphthongs I am afraid we must be content to remain in ignorance; at least I have been able to gain possession of no evidence which seems to me such as to place this matter on a firmly scientific basis. There is an interesting passage in Moschopulos (Titze, Leipzig, 1822, p. 24), which seems to say, contrary to the doctrine of the modern Greeks, that in the diphthong ει the ι was silent, and the sound of ε alone was heard.

I have said nothing in the above argument on the pretensions of the modern Greeks, who have written not a few learned treatises to prove that their characteristic *itacism*, or the predominance of the slender sound of *i*, ought to be allowed to rule the pronunciation of the Greek language, as indeed it actually did everywhere in Europe before the revolutionary innovations of Erasmus. Now, while it is quite certain, on the one hand, that a certain amount of attenuated itacism was characteristic of Greek as contrasted with Latin, at a very early period,† the supposition that half-a-dozen distinct vowels and diphthongs, η ι υ ει οι, should all have been pronounced originally with the same sound, is preposterous. Even in the days of the Apostles, I cannot be brought to believe that such a sentence as χάρις ὑμῖν καὶ εἰρήνη πληθυνθείη was pronounced with the slenderest sound in the

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* NOTE.—See on these points my work on the Pronunciation of Greek. Edinburgh : Edmonston & Douglas, 1852.

† NOTE.—“*Quamquam iis major est* GRACILITAS *nos tamen sumus* FORTIORES.”—Quinctilian.

Greek language ten times repeated. Nothing short of a miracle, indeed, could have preserved the language of Pericles and Plato from some corrupt tendency, during the long space of more than two thousand years which has elapsed since they thundered in the Pnyx and preached in the Academy. Let the modern Greeks, therefore, confess honestly that the excessive itacism of their actual speech is a declension and a deformation from the perfection of their ancient vocalism. On the other hand, let the arbitrary mouthers of Greek speech according to an English—that is, a barbarous—model, bear in mind that Greek is a living language; that it has changed by degrees only as English has changed and is changing; and that, while the English pronunciation of the principal vowels is a gross perversion and an arbitrary invention, the Alexandrian and Byzantine corruptions are only the excess of what is characteristic. This is the rank outgrowth of a living reality; the other, a scholastic figment. As a matter of fact, therefore, and for a sort of international convenience, I have no objection —as indeed the French Academy two years ago advised—to adopt the modern Greek vocalization in reading Greek prose; but the euphony which belongs to poetry seems imperatively to demand that in the recitation of Homer, at least, the οι should receive something of that full, manly sonorousness which the strong lungs of the Homeric heroes could not fail to send forth. I am not inclined to sacrifice the full roll of πολυφλοίσβοιο and ἱπποδάμοιο to any merely Byzantine orthodoxy, however undisputed.

I now come to the accent or intonation—a point on which the defenders of the present perverse pronunciation are found to make a stiff fight, but on which, when the probabilities and authorities are carefully considered, they have really no case at all. Professor Malden, of London, in a paper read before the Philological Society, May 28, 1847, maintains that the acute accent of the Greeks, which is substantially *the* accent, meant only elevation of tone in the voice, and did not also imply *stress* or *emphasis*. There is not the slightest foundation for this assertion. All the proof lies most distinctly the other way. The words ἐπίτασις and ἄνεσις, by which the Greek grammarians generally express the acute and grave accent, signify properly *stress* or *emphasis*, and *remission* or *relaxation* of the voice. That this emphatic stroke of the breath (κροῦσμα, *Theodos. Grammat. Goettling*, p. 61) was in Greek enunciation accompanied by an elevation of the voice, is attested by the word ὀξύς, and is indeed only what might have been expected from the very nature of the case. To elevate the voice on the unemphatic syllable is possible indeed, and may sometimes be observed, as Professor Malden remarks, in the intonation given to English words in some parts of Scotland; but this practice is certainly not so obvious and natural as the other. At all events, the Greek grammarians give us not the slightest indication that while a superior stress lay upon one syllable, a higher key belonged to another. On the contrary, they distinctly indicate by the whole doctrine of enclitics or unemphatic syllables, that accent means emphasis. An enclitic is a monosyllabic or dissyllabic word, which in actual speech is so closely connected with another more emphatic word, as to have no separate verbal emphasis. In all languages the pronouns are such words, and in Italian there is a separate form of that part of speech to express this unemphatic adjunct of a verb, which is always written so as to make one word with the verb, as *datemi*, *dateci*, &c. But, wherever the emphasis of speech requires that these words should receive a separate rhetorical emphasis, as when *I*, for instance, is contrasted with *you*, *yes* with *no*, and so forth, these helpless little recumbents immediately assume an erect position, and are strongly emphasized, as when for the sake of contrast, in English we emphasize the qualifying elements in such compounds as *prógression* and *rétrogression*, *ínvade* and *évade*. Now the Greek language constantly uses this device; and the use proves that accent signifies emphasis. Further; in the decadence of language there is a well-known tendency of words to be curtailed of their unemphasized syllables; in the haste of careless and ill-regulated utterance, that syllable alone remains on which the dominant stress lies; and so the accent, which Diomede calls the *anima vocis*, is the most persistent element in a word, and asserts itself with decision, when terminations are lost and quantity forgotten. Thus in Italian we have *città* for *civitáte*, *amò* for *amávit*, and many such. Modern Greek in the same way shows *μᾶς* for *ἡμᾶς*, *δὲν* for *οὐδέν*, *πίσω* for *ὀπίσω*, *πωρικά* for *ὀπωρικά*, *ψάρι* for *ὀψάριον*, *παιδί* for *παιδίον*, and others of the same kind. Again, the modern Greeks themselves, who inherit, through the persistency of acoustic habit, the accents of the ancients, give a decided preponderance to the last syllable of Παρνασσό, *σκοπό*, *καλό*, &c., as no one can have been but a few days in Greece without observing. What right, therefore, in the face of all these facts a modern Englishman can have to draw back the accent of these words from the last syllable, and lay a decided stress of the voice on the preceding one, no reason can divine. It is merely an arbitrary perversion. For the glaring absurdity of our existing practice is not that we pronounce Greek without accents, but that we pronounce it with Latin accents. The accentuation of the language of ancient Rome, though not marked in the books, was handed down to us correctly, through the Roman Church and its beautiful hymnology; in this language, therefore, we never err in point of emphasis, though we are often careless enough in respect of quantity. But in the Greek language, though Aristophanes of Byzantium, more than 200 years before Christ, took the wise precaution of teaching the accent to the eye as well as the ear in the text of every written sentence, we take upon ourselves systematically to reject that indication, and read Greek exactly as if we were reading Latin! A more extraordinary instance of a determination to take the wrong road, when the finger of the directing-post is pointing out the right, will not be found in the annals of learned blundering. There are no fools like learned fools. That *accent* means *stress* in Latin we habitually allow, by laying the emphasis on those syllables where according to

Roman tradition the accent lies; but while the Roman grammarians indicate no difference in *nature* between Greek and Roman accents, we take upon ourselves to deny that dominance to the accented syllable of a Greek word which belongs to it by the same right that the accented syllable of a Latin or German word is allowed to predominate. Nay more; we do this in the face of those very Roman writers who tell us in the most distinct terms, that, while no Latin word is accented on the last syllable, the Greek language is particularly rich in this final oxytone, and that this element of variety in the *place* of the accent is one great element of orthoepic beauty in the language of polished Athens, as compared with that of more sturdy but less harmonious Rome! Nor is this all; while in teaching we allow our scholars to infect the ear with a false accent for a period of years, we then commence a course of special indoctrination to inform the understanding where the accent should be placed! Absurdity cannot go beyond this. Such a method of teaching, used in the case of any living language, would expose the pedagogue to the just reproof of his principal, and the deserved contempt of his scholars.

That the ancients actually recited their speeches with the observance of the just accent should require no proof with reasonable persons, and is distinctly stated by Theodosius. "Καὶ δεῖ τὸν νέον ἀρχῆθεν κατορθοῦν τὴν ἀνάγνωσιν κατὰ προσωδίαν καὶ τόνον" (p. 58, *Göttling*). And there is a well-known anecdote about Demosthenes, in Ulpian's 'Commentaries' I believe, to the effect that the great orator was on one occasion publicly hissed for laying a false accent on the word μισθωτης. It is certain, also, that if any scholar were to deliver to a Greek audience a Greek oration pronounced according to our perverse British practice, he could scarcely fail to talk nonsense in almost every other sentence; for the sense of many Greek words, otherwise identical in sound, depends altogether on the accent being laid on a different syllable. It was to prevent this confusion partly that Aristophanes invented the accentual marks: πρὸς διαστολὴν τῆς ἀμφιβόλου λέξεως, as Arcadius expressly tells us (p. 186, Barker). In Jelf's grammar will be found a long list of such words; and yet we pronounce ταῦτα, *these things*, and ταυτά, the *same things*; ἔλεος, *mercy*, and ἐλεός, a *board for hashing mince collops*; βρότος, *gore*, and βροτός, a *mortal*; ὦμος, a *shoulder*, and ὠμός, *cruel*; θόλος, a *dome* or *cupola*, and θολός, *mud*, as if they were the same. Thus our system, starting from an utter disregard of philological science, ends, as all random proceedings in such matters must do, in nonsensical practice. But not only is the sense of the spoken language destroyed to the ear, the musical character of the whole language is marred and perverted. The dominant accent upon the proper syllable is as essential to the melody of Greek prose, as the due prolongation of the vowel at marked intervals is to Greek poetry. Imagine French pronounced with the favourite English antepenultimate accent, and you will realize some analogy to the barbarous effect to a well-tuned Greek ear made by the received English enunciation of the following

sentence from Plato, where the oxytone accent prevails, "*οἱ τε θηρευταὶ πάντες, οἵ τε μιμηταὶ, πολλοὶ μὲν οἱ περὶ τὰ σχήματά τε καὶ χρώματα, πολλοὶ δὲ οἱ περὶ μουσικὴν, ποιηταὶ τε καὶ τούτων ὑπηρέται, ῥαψῳδοὶ, ὑποκριταὶ, χορευταὶ, ἐργολάβοι, σκευῶν τε παντοδαπῶν δημιουργοὶ τῶν τε ἄλλων, καὶ τῶν περὶ τὸν γυναικεῖον κόσμον. Καὶ δὴ καὶ διακόνων πλειόνων δεησόμεθα ἢ οὐ δοκεῖ δεήσειν παιδαγωγῶν, τιτθῶν, τροφῶν; κομμωτριῶν, κουρέων καὶ αὖ ὀψοποιῶν τε καὶ μαγείρων.*" (*Rep.* II., 373 B.)

Innumerable passages of this kind constantly occur, where the fine roll of the voice on the *ῶν* of the genitive plural and other terminational syllables is, by the indiscriminate application of the meagre Latin accentuation, altogether lost. For my part, now that my ear is, by the practice of many years, tuned to the correct recitation of Greek prose, I should as soon consent to blotting out the *οἶο* and the *άων* from the Homeric hexameter, as to the wholesale swamping of the oxytones which the English practice induces in reciting the musical ocean-roll of a Plato, or the energetic thunderclaps of a Demosthenes.

Apologies for the present perverse practice of pronouncing Greek have no doubt been attempted, and will, in all likelihood, be attempted again. In the most desperate case, a clever advocate will find something to say for an accused person, of whose guilt no sane juryman can have a doubt. The vulgar objection, for instance, to the correct pronunciation of the Greek word *ἄνθρωπος*, with the accent on the ante-penult and the penult long, *viz.* that the quantity of the penult is thereby lost; this objection disappears the moment we produce a German word such as *ábhaŭen*, or an English word such as *lándhōlder*, where exactly the same relation of accent and quantity exists. And if in giving full effect to the accent on the first syllable of such words the quantity of the second syllable might in rapid speaking be somewhat curtailed, this is only what happens in all languages, and should not be allowed to disturb the rational and legitimate accentuation of Greek. In Latin, for instance, we know with perfect certainty that the long final *ō*, in verbs exactly observed by Virgil, as in *cano, fremo*, is often shortened by Martial; for the obvious reason, that as the poet of colloquial wit, this writer followed the loose law of Roman talk, which habitually cheated the long final *o* in this and some other cases of its legitimate prolongation. And if there be any scholar who cannot be made to understand how the penult of such a word as Ἀριστοφάνης (and there are hundreds such in Greek) should be accented and short, while the last syllable is unaccented and long, this can only be explained by that gross quality of hearing that is the natural product of long years of perverse practice, and sopited sensibility.

On the third great section of the doctrine of the music of language in Greek and Latin, viz., *Rhythm* and *Metres*, my present limits allow me only to make one remark; a remark necessary to repel the objection felt by many to reading Greek prose according to accent, *viz.* that this practice annihilates altogether the metrical movement of verse.

The remark is this: all ancient poetry was a part of music, not of colloquy; the singing element of language is quantity, not accent—full, broad, and prolonged vocalism as we see in Italian; therefore in the composition of their verses made to be sung, not read, the ancients subordinated the colloquial accent, or sunk it altogether. This is certain from the double fact, that while on the one hand Hephæstion and other ancient metrical writers never allude to accent as an element in poetical composition, no modern reader can bring a metrical movement out of any Greek verse otherwise than by the systematic neglect of it. Another incontrovertible proof of the practice of the ancients in this matter is that the contrary character in modern poetry has produced a contrary law of poetical composition. Our poetry, written to be read, and read a thousand times for once that it is sung, follows the laws of reading and of spoken discourse, *viz.* the metrical accent is kept identical with the colloquial accent, while the quantity is left to shift for itself, at least does not lie under the dictatorship of certain fixed and commutable laws. And if it seems a strange thing to us that the ancients in the composition of poetry should have habitually tolerated a syllabic accentuation contrary to what they used in prose, it would no doubt appear an equally strange thing to them, that in the singing of the most beautiful hymns and songs we habitually prolong *God* into *goad*, and shorten *goad* into *god*, as it may suit our convenience. Practically there is not the slightest difficulty —as I know from experience—in reading Greek prose according to both accent and quantity, and measuring Greek poetry to the ear, by the musical accent and the exact pronunciation of short and long syllables. In fact, those who pronounce σκοπός, as if it were σκόπος, would find themselves as much put out by the occurrence of such a word in the close of an Iambic line, as I should be if the line ended in βρότος. Nay, it is a curious fact, that in some of the most beautiful Greek choral songs (as in the ἀέναοι Νεφέλαι of Aristophanes, '*Clouds*,' 275-290), the musical accent indicating the metre is, in a great majority of cases, coincident with the syllabic accent of the words as spoken in common colloquy. I consider, therefore, the objection that pronunciation, according to accent, annihilates quantity, not only scientifically worthless, but practically unmeaning. Those who make these objections appear to me like persons suffering under a certain nervous disease, which makes them imagine that their bodies are made of glass, and that the moment they rise to walk they will fall to pieces. But it is not so. The supposed incompatibility of accent and quantity vanishes the moment a man works himself into the living practice of the thing. Here, as in more important matters, the only solution of sceptical doubts is to be found in action. As for the modern Greeks, if, after having endured long centuries of decadence and oppression, they have forgotten their ancient musical cunning, and will persist in reading Homer according to the spoken accents of their common colloquy, that is their loss, not ours. A little training would no doubt set them right in this matter, and they would gladly meet us half-way, if we did not habitually insult them by throwing overboard all the living traditions of their orthoepy. Our circumstances have been more favourable than theirs; and the one-sided perversity into which we have fallen deserves less excuse. Historically, I believe it is traceable principally to the metrical hobbihorsicality which distinguished the scholarship of Bentley and Porson. The Roman accentuation was transferred to Greek, as more convenient to mark the quantity of certain penultimate syllables. This was all. To remedy one small inconvenience for the sake of ignorant, lazy, or careless schoolmasters, a huge organism of systematic blunders was created. I hope the time has now arrived when thoughtful scholars will no longer allow themselves to be guided in important points of philological practice by no higher law than that which governs crinolines and chignons. I think this matter should be settled by reason; and the great English universities could perform few more thankworthy services than by promulgating an ordinance on this subject, which would at once bring England into harmony with the great guildry of European scholarship, and furnish our public teachers with a rule of classical orthoepy both scientific in its basis and convenient in its application.

[J. S. B.]

GENERAL MONTHLY MEETING,

Monday, May 6, 1867.

Sir Henry Holland, Bart. M.D. D.C.L. F.R.S. President,
in the Chair.

The following Vice-Presidents were nominated for the ensuing year:—

The Right Hon. Edward Cardwell, M.P.
John Peter Gassiot, Esq.
Colonel Philip J. Yorke, and
William Spottiswoode, Esq. F.R.S. the Treasurer.

Henry Cosmo Bonsor, Esq.
Colonel Charles Douglas, R.A.
Frank Clarke Hills, Esq.
Arthur Thompson, Esq.

were *elected* Members of the Royal Institution.

The following Professors were re-elected:—
John Tyndall, Esq. LL.D. F.R.S. as Professor of Natural Philosophy.
Edward Frankland, Esq. Ph.D. F.R.S. as Professor of Chemistry.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

Secretary of State for India—Report on the Registration of Ozone, in the Bombay Presidency, in 1864–65. fol. 1866.

Governor-General of India—Geological Survey of India:
Annual Report. 1865-6. 8vo.
Catalogues of Cephalopoda and Meteorites. 8vo. 1866.
Memoirs. Vol. V. Art. 3, 4. 8vo. 1866.
— Palæontologica Indica. III. 10–13. fol. 1866.

Lieutenant-Governor of Bengal—Report on the Calcutta Cyclone. By Lieutenant-Colonel J. E. Gastrell and H. F. Blanford. 8vo. 1866.

The Corporation of London—Catalogue of Works of Art. Part I. 8vo. 1867.

United States Naval Observatory—Astronomical and Meteorological Observations for 1864. 4to. 1866.

Actuaries, Institute of—Journal, No. 67. 8vo. 1867.

Architects, Royal Institute of British—Proceedings, 1867. Part II. 4to.

Asiatic Society of Bengal—Journal, Nos. 134, 137. 8vo. 1866.

Astronomical Society, Royal—Monthly Notices, Vol. XXVII. No. 5. 8vo. 1867.

Bavarian Academy of Science, Royal—Sitzungsberichte, 1866. Band II. 2, 3, 4. 8vo.

Chemical Society—Journal for April, 1867. 8vo.

Editors—Artizan for April, 1867. 4to.
Athenæum for April, 1867. 4to.
British Journal of Photography for April, 1867. 4to.
Chemical News for April, 1867. 4to.
Engineer for April, 1867. fol.
Horological Journal for April, 1867. 8vo.
Journal of Gas-Lighting for April, 1867. 4to.
Mechanics' Magazine for April, 1867. 8vo.
Pharmaceutical Journal for April, 1867.

Faraday, Professor, D.C.L. F.R.S.—Atlas der Hautkrankheiten. Lief. VI. fol. 1866.
Akademie der Wissenschaften: Sitzungsberichte. Math. Nat. Classe:—Abth. I. 1866. Nos. 7, 8. 8vo. Abth. II. Nos. 6, 7, 8, 9. 8vo.

Figueroa, Senhor Rafael Pardo de (the Author)—Critica del Regiemẽto de Navigacio, &c. (L 14) 8vo. 1867.

Franklin Institute—Journal, Nos. 494, 495. 8vo. 1867.

Geographical Society, Royal—Proceedings, Vol. XI. No. 2. 8vo. 1867.

Geological Society—Quarterly Journal, No. 90. 8vo. 1867.

Linnean Society—Journal and Proceedings: Botany, No. 39. 8vo. 1867.

Mechanical Engineers' Institution, Birmingham—Proceedings, August, 1866. Part 3. 8vo.

Medico-Chirurgical Society, Royal—Proceedings. Vol. V. No. 7. 8vo. 1867.

Meteorological Society—Proceedings, Nos. 29, 30. 8vo. 1867.

Photographic Society—Journal, No. 180. 8vo. 1867.

Royal Society of London—Proceedings, No. 91. 8vo. 1867.

Statistical Society of London—Journal, Vol. XXX. Part 1. 8vo. 1867.

Symons, G. J. Esq. (the Author)—Symons' Monthly Meteorological Magazine. April, 1867. 8vo.

Vereins zur Beförderung des Gewerbsfleisses in Preussen—Verhandlungen, Sept.–Dec. 1866. 4to.

Victoria Institute, or Philosophical Society of Great Britain—Journal of Transactions. Vol. I. Nos. 1–3. 8vo. 1866–7.
Geological Theories. By the Rev. J. Kirk. 8vo. 1867.

WEEKLY EVENING MEETING,

Friday, May 10, 1867.

Sir Henry Holland, Bart. M.D. D.C.L. F.R.S. President, in the Chair.

Professor Alexander Bain, M.A.
of the University of Aberdeen.

On the Doctrine of the Correlation of Force in its Bearing on Mind.

The speaker began by giving his view of the Correlated Forces. There are five commonly recognized forces: one Mechanical, or *Molar*, movement in mass; and four *Molecular*, or movement in molecule—Heat, Light, Chemical Force, Electricity. Of vital force, it is difficult to speak as a whole, but one member of it—the Nerve Force—allied to Electricity, is in every respect entitled to rank among the Correlated Forces, making the fifth of the molecular group. With the exception of Light, all the forces are exchangeable on an assignable rate of commutation.

The speaker then quoted the received views as to the maintenance of the animal forces; which depend, in the last resort, upon the oxidation of the food. From this oxidation, or animal combustion, is derived, the temperature of the body, the muscular or mechanical energy, and the nervous power, or nerve force. Now the extension of the Correlation of Force to mind, if admissible, must be made through the nerve force, an undoubted member of the group.

The speaker then referred to some of the theories of the connection of mind and body, and indicated the views (1) of Aristotle, and (2) of Aquinas, who may be considered the author of the modern settlement of the relations of the material and the immaterial. Both philosophers represented the highest intellectual processes as conducted by the immaterial substance, a doctrine liable to serious difficulties. The influence of modern physiology had compelled the admission of a material basis for the intellectual functions; and the consequence of this admission has been a modified theory, expressed as the "mutual action of mind and body." The speaker regarded this view as an advance on Aquinas, but as still incorrect. It supposes that we know mind apart from body, and that pure mind can exert influence upon pure body. But we have no experience of pure mind: we know only of the compound mind-body; and when we speak of the mind acting on the body, it is the compound phenomenon that really produces the effect. When a shock of fear deranges the digestion, the cause is not the abstract consciousness called the emotion of fear, but this in combination with a condition of the nervous system; so that the real sequence is mind-body operating upon mind-body.

The commonly conceived difficulty as to the union of mind with body is, properly speaking, no difficulty at all. We accept every alliance that we find in nature as a fact, and merely generalize it to the uttermost; such are the alliances of gravity and inert matter, heat and light. The real difficulty in the case is of another kind. When we speak of two things being united, we can hardly avoid supposing a *local* union, a union in place, or extension. We apply this notion to mind and body, and find ourselves in hopeless perplexity. Body is an extended thing, but mind proper is unextended; it has no dimensions, form, or division of parts; when we are under a feeling of pleasure or pain, we are in a mood where the property of extension is inapplicable; and yet this unextended consciousness has for its essential condition certain modes of extension, namely, the corporeal frame and its parts. Mind is not a place but a *state;* we live by turns in two different states, the one unextended, the other having the property of extension. The union of mind and body is not a local union, because connection in place supposes the two things to be extended or objective things; it is a union of *dependence*, and of *sequence in time.* An extended organism is the condition of our existing in states of the unextended—pleasures, pains, and ideas; and our life is a continuous thread of alternative states, extended and unextended, object and subject. The familiar mode of expressing body and mind by the words 'external' and 'internal' is faulty, for these words suppose relation in place, and apply only to two extended things.

The speaker then proceeded to explain and prove the doctrine of the correlation of the material forces with the mental force. Every mental display, every mode of feeling and thought, demands a certain definite expenditure of nerve force; and the nerve force is definitely related to the source of all vital force, the combustion or oxidation of the food. He gave a series of proofs and illustrations of the position, that the mental manifestations are in exact proportion to their physical supports. He showed that according as the mind is exerted, force is drawn away from the proper corporeal functions, which are to that extent weakened. This is merely the general or scientific statement of the common experience of the incompatibility of great mental exertion with great physical robustness.

Another important consequence of the doctrine of the definite correlation of mind with the physical forces, is the mutual limitation of the mental manifestations among themselves. Taking the three distinct functions or departments of mind—Feeling, Will, and Intellect—it may be maintained that they each involve a certain definite physical expenditure, and that an excessive degree of the one necessarily stints the others. On comparing the cost of the three different functions, the speaker held that the demands of the intellect are the largest, and that great and continued intellectual efforts use up a very large amount of the whole energies of the system.

He applied the doctrine of mutual limitation of mental functions to explain the incompatibility of different modes of mental eminence, as scientific and artistic power in a high degree; great sensibility with great activity of temperament; and intellectual originality with emotional exuberance.

[A. B.]

WEEKLY EVENING MEETING,

Friday, May 17, 1867.

Sir Henry Holland, Bart. M.D. D.C.L. F.R.S. President,
in the Chair.

William Odling, M.B. F.R.S.

On the Occlusion of Gases by Metals.

I.

The remarkable property first observed by M. Deville, in the case of homogeneous platinum and iron, when at a red heat, of being permeable to hydrogen gas, is not by any means confined to these two metals; and has been shown by Mr. Graham to be manifested in a much greater degree by palladium, even at temperatures falling considerably short of redness.

An exhausted tube of wrought palladium, surrounded by atmospheric air, remains perfectly vacuous at a red heat; surrounded by an atmosphere of hydrogen, it remains vacuous at 100°, but allows of some transmission at 240°; while at 265°, and up to a temperature just short of redness, there is a steady and considerable passage of hydrogen to its interior, maintained vacuous by the Sprengel pump. Surrounded, under the same conditions, by coal gas, the free hydrogen of the coal gas alone finds its way into the interior of the tube, the remaining constituents of the gas being excluded by the heated palladium as effectively as, in other experiments, they are excluded by ignited platinum.

This transmission of hydrogen, through the substance of various metals, is altogether different in character from the transmission of gases in general by the physical processes of transpiration and diffusion. It is evidently dependent upon some special relationship subsisting between the particular gas and metal, and has been shown by Mr. Graham to be preceded by an absorption or occlusion of the gas in the substance of the metal.

II.

Platinum-wire, drawn from the fused and solidified metal, was heated to redness and allowed to cool slowly in a current of dry hydrogen gas. After cooling, it was exposed freely to the air for some time, and then placed in a tube of porcelain or hard glass, which was next exhausted by the Sprengel pump. After complete exhaustion, the tube was heated to redness, when the contained platinum began and continued to give off hydrogen gas, which was delivered by the pump. The quantity of hydrogen, measured cold, amounted to 21 per cent. of the volume of the platinum-wire. That the absorption did not depend upon surface, was shown by drawing out the same wire to four times its original length, and repeating the experiment when the absorption was found not to have increased, but rather to have decreased, as it amounted only to 17 per cent.

To show the effect of texture, a similar experiment was made with spongy platinum, which was found to absorb and deliver 148 per cent. of its volume of hydrogen. Experiments were also made with ordinary wrought platinum, a particular piece of which was found to occlude in three successive experiments, 553, 493, and 383 per cent. of its volume of hydrogen, measured cold, giving a mean of 476 per cent. Thus the intermediate form of platinum, more porous than the fused, but more compact than the spongy form, was found to be the most absorptive. In round numbers, 1 volume of this platinum absorbed about 5 volumes of hydrogen which, at the temperature of the experiment, would amount to some 15 volumes. Now to compress 15 cubic centimetres, for instance, of hydrogen into the space of 1 cubic centimetre would require a pressure of 15 atmospheres. But in this experiment, the 15 cubic centimetres of hydrogen were condensed, not merely into 1 cubic centimetre of space, but into so much of 1 cubic centimetre of space as appeared to be entirely occupied by platinum, and was not really so occupied. So that assuming the pores of the wrought platinum to amount to $\frac{1}{1000}$ of its bulk, the above described condensation of the hydrogen corresponded to that producible by a pressure of 15,000 atmospheres.

To show the force with which hydrogen was retained by platinum, another piece of the wrought metal was charged with hydrogen as before, and then heated very gradually in a vacuous tube. During exposure for an hour to 220°, not a particle of gas was evolved. At a temperature slightly below that of visible redness, there was still no gas evolved. At a temperature sufficient to soften glass (500°), 1·72 c.c. of hydrogen were collected in ten minutes; and, heated for an hour in a combustion furnace, an additional 8·20 c.c. of hydrogen were collected, making altogether 9·92 c.c., or 379 per cent. of the volume of platinum employed in the experiment. The same piece of platinum, charged with hydrogen, was kept for two months, sealed up in a glass tube, which it nearly filled. At the end of that time, the air of the tube was found to be quite free from hydrogen, showing that none had been evolved by the enclosed platinum.

The absorption of hydrogen by platinum took place at a temperature much below that necessary to cause an evolution of the evolved gas. Thus some platinum-foil was found to absorb 76 per cent. of its volume of hydrogen at 100°, and 145 per cent. of its volume at 230°.

III.

Palladium appears to be a metal altogether special in its relations to hydrogen. Foil of wrought palladium that had been maintained at a temperature not exceeding 245°, and allowed to cool slowly in a current of hydrogen, evolved, when afterwards heated in vacuo, no less than 52,600 per cent., or 526 times its volume, of the gas within a quarter of an hour. But even this comparatively low temperature was found to exceed that most favourable to gas absorption. For, maintained at a temperature between 90° and 97° for three hours, and allowed to cool down during an hour and a half, the foil absorbed 643 times its volume of hydrogen, measured cold. Even at ordinary temperatures it absorbed 376 times its volume, provided it had been recently ignited in vacuo. Palladium sponge heated to 200° in a current of hydrogen, and allowed to cool slowly, afterwards yielded no less than 686 times its volume of the gas. Now if the absorption by ignited platinum of 5 times its volume of hydrogen is difficult to realize, how much more difficult is it to realize the absorption of 5 or 6 hundred times its volume of hydrogen by moderately heated palladium? Notwithstanding the levity of the gas, this large absorption of hydrogen by palladium is sufficient to increase recognizably the apparent weight of the metal. The retention, however, of such a charge of gas is not complete, a portion of the condensed hydrogen being slowly evolved or volatilized by exposure of the charged palladium to air. The hydrogen condensed in palladium is capable of exerting those particular reducing actions, which under ordinary circumstances, are producible only when the gas is in the so-called nascent state. Thus the hydrogenized palladium quickly reduces permanganate of potassium, bleaches iodide of starch, throws down prussian blue from ferric ferridcyanide, &c. Further, the absorptive power of palladium is manifested in a varying degree upon different liquids. Thus, 1,000 volumes of palladium-foil were found to absorb 1 volume of water, 5½ volumes of alcohol, and 1½ volumes of ether; results showing a special selective relationship of the metal to these different liquids.

IV.

The absorption of hydrogen by ignited *copper*, in the state of wire, amounted to 30 per cent., and, in the state of sponge, to 60 per cent. *Gold*, in the form of assay cornettes, was found capable of absorbing 48 per cent. of hydrogen, 29 per cent. of carbonic oxide, 16 per cent.

of carbonic anhydride, and 20 per cent. of air; but of this absorbed air, nearly the whole was nitrogen. Before charging the cornettes with the above gases, it was necessary to ignite them for some time in vacuo, in order to expel the gas they had spontaneously absorbed in the muffle. This, which may be termed the natural gas of the cornettes, amounted to 212 per cent., and consisted principally of hydrogen and carbonic oxide. *Silver*, unlike the preconsidered metals, is characterized by its preferential absorption of oxygen. In different experiments, silver-wire heated to redness was found to absorb 74 per cent. of oxygen, and nearly 21 per cent. of hydrogen. Silver-sponge absorbed 722 per cent. of oxygen, 92 per cent. of hydrogen, 52 per cent. of carbonic anhydride, and 15 per cent. of carbonic oxide. A specimen of silver-leaf, exposed to the air at a red heat, absorbed 137 per cent. of oxygen, and 20 per cent. of nitrogen; so that while ordinary atmospheric air contains 21 per cent. of oxygen, and the air absorbed by gold only about 5 per cent., the air absorbed by silver contained no less than 85 per cent. of oxygen.

V.

Iron, though tolerably absorptive of hydrogen, is specially characterized by its absorption of carbonic oxide. Ordinary iron-wire, that had been carefully cleaned and heated in vacuo to expel its natural gas, when afterwards heated in different atmospheres, was found to absorb 46 per cent. by volume of hydrogen, and 415 per cent. of carbonic oxide. The natural gas of wrought-iron, derived from the forge in which it had been heated, proved to consist principally of carbonic oxide, and, in different experiments, ranged from 700 to 1,250 per cent.; so that, in the course of its preparation, iron would appear to occlude upwards of 7 times its volume of carbonic oxide gas, which it carries about with it ever after. The discovery of this absorbability of carbonic oxide by iron has an important bearing upon the theory of acieration. Carbonic oxide (C_2O_2) would appear to be actually absorbed by the substance of the iron, and then decomposed at a different temperature, into carbon (C) which, entering into combination with the iron, converts it into steel, and into carbonic anhydride (CO_2) which, escaping from the surface of the iron, gives rise to the appearance of blistering.

It became a matter of interest to determine whether sidereal iron, that is to say the iron of meteorites, contained any, and, if any, what natural gas. Accordingly, some 45 grammes, or 6 cubic centimetres, of meteoric iron from the Lenarto fall were heated in vacuo for two hours and a half, and found by Mr. Graham to give off 16·5 cubic centimetres of gas, which consisted substantially, not of carbonic oxide, but of hydrogen, to the extent at least of 85·5 per cent. of the entire yield of gas, the remainder being chiefly nitrogen and carbonic oxide. The inference that the meteorite, at some time or other, had been ignited in an atmosphere of which the prevailing constituent was hydrogen, is obvious; and, judging from the volume of gas yielded, the hydrogen atmosphere must have been a highly condensed one. For even under ordinary atmospheric pressure, telluric iron is found to absorb but somewhat less than half its volume; whereas this sidereal iron furnished fully two and a half times its volume of hydrogen. It is known that Father Secchi, in his classification of the stars according to their spectra, has distinguished one class, typified by α Lyræ, as having a spectrum which is essentially that of hydrogen.

VI.

In the year 1823, Mr. Faraday established the general proposition that a gas is nothing else than the vapour of a volatile liquid existing at a temperature considerably above the boiling point of the liquid; and that the condensing points of different gases are merely the boiling points of the liquids producing them. But the boiling point of a liquid, or the condensing point of its gas, is well known to be not a fixed point of temperature, but a point varying with the pressure to which the gas or liquid is subjected. Accordingly, every one of the many different gases known to chemists, with about six exceptions, has been actually condensed into the liquid state by a sufficient increase of pressure; whereby the existing temperature of the gas has ceased to be above the heightened condensing point, or boiling point, corresponding to the increased pressure. And since a gas cannot be reduced by pressure to a bulk less than that corresponding to the pressure necessary to liquefy it, without its becoming liquefied, conversely, the reduction of any gas to a bulk less than that corresponding to the pressure necessary to liquefy it, must be taken as evidence of its liquefaction. Hence, from the extremely minute volume which oxygen, hydrogen, and carbonic oxide occupy, when occluded for instance in silver, platinum, and iron respectively, there can be little doubt but that these gases, though included among the half dozen which have never been liquefied by direct pressure, do nevertheless exist in the liquid state when occluded in the above metals; or, at any rate, do not exist in the gaseous state.

As regards the nature of this absorption and presumable liquefaction of gases by metals, there are facts which seem to indicate that the phenomenon is related, on the one hand, to the absorption of gases by their solution in liquids, or in those soft solids which Mr. Graham has denominated colloids; and, on the other hand, to the absorption of gases by their condensation in the minute pores of hard solids, such as compact charcoal.

[W. O.]

WEEKLY EVENING MEETING,

Friday, May 24, 1867.

John Peter Gassiot, Esq. F.R.S. Vice-President, in the Chair.

A. S. Herschel, Esq. B.A. F.R.A.S.

On the Shooting-stars of the Years 1866–67, *and on the probable Source of certain Luminous Meteors in the material Substance of the Zodiacal Light.*

Regarded as an exhibition of a variable phenomenon, recurring at the end of every cycle of nearly thirty-three years, on a particular date of November, the meteoric shower witnessed on the morning of the 14th of November, 1866, appears to have been a fair example of the average scale of the November meteors at one of their principal returns. While it incomparably surpassed all the commoner displays of shooting-stars that are known to have occurred during the past period of more than thirty years, it nevertheless fell considerably short of the celebrated meteoric shower seen in America on the morning of the 13th of November, 1833. That shower, it will be remembered, took place quite unexpectedly, while one of the distinguishing features of the recent November star-shower was that it afforded a complete verification of the astronomical theory which made their return expected by Olbers, as about to take place in 1867; and more recently by Professor H. A. Newton, who anticipated the recurrence of the shower in 1866.

Although intended only to guide observation, due notice of the shower was timely given by Professor Newton,[*] to the effect that a considerable star-shower might be expected to take place on the morning of the 14th of November, 1866. On the eve of the occurrence the well-timed appeal was repeated in many places in the public papers, and a wide-spread and very intense popular interest in the phenomenon was excited. In England circular letters were addressed to the Members of the Royal Astronomical and Royal Meteorological Societies, by their respective Presidents, Mr. Pritchard and Mr. Glaisher, suggesting to observers the propriety of making concerted observations of the star-shower, during the second hour after midnight on the two mornings of the 13th and 14th of November, so that if possible a number of the meteors might be simultaneously observed at distant places. On the first of those mornings the sky was completely overcast, and on the morning of the 14th of November the meteoric shower made its appearance with, certainly, great beauty, but so exactly during the hour named beforehand for simultaneous observations, that many, even of the most zealous of the confederated observers, as they expressed themselves, "gave up recording, and betook themselves to counting."

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[*] 'American Journal of Science,' 2nd series, p. 60 *et seq.*

Accordant duplicate observations could hardly be expected to be obtained, when meteors were so very numerous, and were so strikingly uniform as to size, as seldom very greatly to exceed first magnitude stars in brightness. Of fireballs sufficiently brilliant to have attracted general attention (at least over Great Britain), there were very few. The shower, accordingly, met with less favourable observation, as regards ascertaining the absolute altitude of the meteors, than that which was observed more successfully for the same purpose in the previous year,* when the heights and velocities of several meteors were determined.

One observation of the kind, obtained in the recent meteoric shower, will, however, shortly be mentioned in detail. On the other hand, most important results were obtained by Mr. Glaisher at Greenwich,† Professor Adams and Dr. Challis at Cambridge,‡ and by other eminent observers and astronomers, both at home and abroad, who gave their best attention to the subject. The vexed question of where the meteors came from, was thus satisfactorily disposed of. The orbit of the meteoric group was finally determined; and, lastly, three recent comets, to which closed orbits have been assigned, now rank, almost certainly, as forming part of the material currents which give rise, respectively, to the meteors of the 10th of August, the 14th of November, and the 20th of April. The November star-shower of 1866, accordingly, both for the astronomical premonition which it fulfilled, and for the novel views which followed it, marks a new era in meteoric astronomy, not unlike that which dawned upon cometary astronomy when Clairault calculated the day of the return of Halley's comet, in the year 1759, and the comet appeared, almost punctually, at the appointed time.

In America the star-shower on the morning of the 14th of November last, was expected to be visible to the best advantage. Only 172 meteors, however, mostly of small size, were counted at the Washington National Observatory on that morning, during an interval of two hours and a half in which the sky was clear; indicating about the same rate of falling as on the preceding night. There was nothing peculiar, either in colour or in motion observed. At Newhaven,

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* 'British Association Report,' 1866, p. 139; and these 'Notices,' vol. iv. p. 648.

† See Diagram showing the average number of meteors per minute at Greenwich. 'Monthly Notices, R.A.S.,' vol. xxvii. p. 54.

‡ Ibid. pp. 75 and 247.

U.S.A.,* the average number of meteors seen by one person of Professor Newton's staff of observers, on the same morning, with a beautifully clear sky, was about thirty-eight meteors in one hour, the average number on the preceding night having been from sixteen to twenty meteors in an hour. So great was the disappointment of astronomers in America, who confidently expected a successful view of the phenomenon, that a telegram, *viâ* the Atlantic cable, which appeared in the New York 'Herald,' announcing the appearance of the shower in England, could hardly be believed, until the arrival of the English newspapers at New York dispelled the doubts of its correctness. Owing to the seasonable notice of the shower finding its way by various channels, into every civilized quarter of the globe, observers in Europe, and elsewhere, were not a whit less expectant than in America; and here, at least, they were not destined to be disappointed, since the star-shower was everywhere conspicuously seen, and witnessed with admiration. The following extract from a letter from Syria, published in the New York 'Tribune' of the 29th of December, realizes the figurative language with which the early Arabian chroniclers sought to adorn their description of the great November star-shower of the year A.D. 1202:—

"I have just received the Arabic newspaper of this week's issue, and find in it the following news about the meteors, which, for your benefit, I will translate literally:—

"'*Beirut Domestic Intelligence.*—There has preceded this a notice, in No. 431 of our journal, of the falling meteors of the 12th and 13th of November, and there happened a marvellous thing of the kind on the night of the 13th. . . . People of Beirut saw thousands of these meteors, mixed in commotion and confusion, and they compared their extent in the heavens to the spreading out of locusts in the sky. And we have news from Damascus, that the same events were seen there, and they compared them to the mighty armies, joined in a fierce strife, from the four quarters of the sky. . . .'

"The Arabic journal then gives a very fanciful letter on the subject from one of the learned men of Damascus, the scholar Solyman Effendi Sooloh, who says:—

"'In this past night the stars began the war from the east to the west, and from the southern to the northern side. They dashed at the pace of fiery steeds and ghouls, so that you could not distinguish the Pleiades from the Hyades from the passing of the meteors across them, and the intensity of the brightness. But you now thought that the two stars in Leo's nose had been dispersed, and the two fishes were eclipsed and immersed, and the spearman of Arcturus had forgotten his spear, and was thinking only of his own safety, and the Adhal was complaining to the bright daughters of Ursa Major about the extent of his wound, and the lofty pole had fallen into the claws of the Eagle, and the Hedrah was prostrate, and the face of night like a leopard's skin; and to sum up all the heavens were like a sphere of fire, or a gleaming of sparks, excepting that the fire and sparks were harmless, not touching the earth, or injuring our safety, as if night's daring horsemen, who continued till morning beating each other in single combat, gave us protection and peace. This I write for his Excellency our Prince, the Sultan Abdul Aziz Khan. May God perpetuate the seat of his government to the end of the world's revolution!'"

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* 'American Journal of Science,' vol. xliii. pp. 78 *et seq.*

The same letter from Syria further mentions that, "on the morning of the 11th (Sunday), at a little after midnight, some young men in Beirut, who were out of doors, saw what they described as a rain of fire, the stars seeming to have got loose, and to be running about the sky in disorder." The occurrence of such a companion shower to the principal display, is not by any means improbable, and well deserves attention.* The meteors on the morning of the 14th of November were also seen in Persia, on the road to Ispahan.†

A letter from Mr. W. Masters,‡ Professor at the Kishnaghur College, about fifty-seven miles due north of Calcutta, in India, to Sir John Herschel, gives the following description of the shower:—

"I looked out about half-past four, or a quarter to five, and . . . after counting fifty in about five minutes, I woke up four other persons to witness the phenomenon, and to give aid in watching and counting. We arranged ourselves looking in different directions, and as each saw a meteor there was a distinct call of the next number, 51, 52, 53, &c.; the stars shooting out sometimes faster than they could be counted. Some were lost on this account, . . . yet in less than half-an-hour we counted 420; had we been all together during the half-hour we would certainly have counted more than 500."

The meteors were visible also at Sealkote, and at Lahore. Dawn appearing, however, put an end to the display, but a bright meteor was still seen at Kishnaghur after daylight had appeared.

A short note of the phenomenon at Yokohama, in Japan, and a private memorandum from Mr. B. V. Marsh, of Philadelphia, U.S.A., received by the speaker, are as follows, and concur in showing that the star-shower was not visible in the extreme east of Asia:—

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* "It appears from observations of Captain Meiraldi, that another meteoric swarm passed over Italy during the night of the 12th–13th, and its characters did not differ from those of the display on the 14th at Urbino: that is to say, a great number in a short space of time, beginning unexpectedly, and ceasing very suddenly."—'Les Mondes,' 2nd ser. vol. xii. p. 644. Private letters inform the writer that a great and sudden outburst of "hundreds of shooting-stars," lasting only a short time, but so sudden and bright as to be terrifying, was seen at Norwich shortly before four o'clock A.M., on the 14th. The same was seen near Staplehurst, in Kent, "on the stroke of four o'clock," on the morning of the 14th, when the regular shower of shooting-stars had almost ceased to be watched. The notes, perhaps, refer to one and the same phenomenon, which must have taken place about four o'clock on the morning of the 14th.—(A. S. H.)

† 'Les Mondes,' 2nd ser. vol. xiii. p. 451.

‡ Royal Astronomical Society's 'Monthly Notices,' vol. xxvii. p. 202.

"Yokohama, 17th November, 1866.—I looked out for shooting stars, but it rained and blew so, that no stars could be seen till the morning of the 14th, and then nothing was to be seen but one or two odd ones.—(J. P. L. Maclear)."

"Philadelphia, 22nd March, 1867.—B. R. Lewis, Dep. U. S. Consul General at Shanghai, writes to me, under date of the 29th of November, that he had not heard that any unusual display had been observed there.—(B. V. Marsh)."

The easternmost limit of visibility of the shower is, indeed, very clearly defined, by the above description of the phenomenon at Kishnaghur, about the Bengal Presidency of India.

The star-shower was well observed at the Cape of Good Hope Observatory, in South Africa, by Mr. G. W. H. Maclear.* The shower was at its height at twelve minutes past two o'clock (Cape time), corresponding to two minutes before one o'clock Greenwich time; and a brief letter from Sir Thomas Maclear ('Edinburgh Quarterly Review,' Jan. 1867, p. 258) gives the following graphic account of its appearance :—

"In the early part of the night of the 13th, few meteors or shooting stars appeared. At 1h. 3m. A.M., on the 14th, the volcano burst forth, with awful grandeur, from the neighbourhood of Regulus; orange-coloured meteors, leaving streaks of green, mingled with ordinary looking shooting stars dashing along in a south-westerly direction. The scene was beyond description, and thus, with little variation, the projectiles continued till daylight. The total number counted amounts to two thousand seven hundred and forty-two."

At Athens,† Rome,‡ Turin,§ Paris,‖ Brussels,¶ and throughout the continent, good observations of the star-shower were made. At Malta the meteors appeared falling like a shower of hail. At Urbino, in Italy, they were compared to a flight of handgrenades, and at Saragossa, in Spain, they reminded the inhabitants of the bombardment of the town. At Haddingham, in England, Mr. Dawes compared the shower with that which he witnessed at Ormskirk near Liverpool, on the 13th of November, 1832.** But the meteors, in 1866, were not so large as on that occasion. At the Royal Observatory, Greenwich, the shower first reached its maximum at seven minutes past one o'clock, and it was most intense for the space of an hour and a half, from half-an-hour after midnight until two o'clock; before and after which times the scale of the phenomenon hardly exceeded a considerable August shower. The altitude of the radiant point at the last-named hour, was thirty-three degrees above the east horizon; and as the shower, as was already mentioned, was not visible in America, some thirty degrees of longitude west of Greenwich, in the Atlantic Ocean, probably terminated the visibility of the shower towards the west.

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* Royal Astronomical Society's 'Monthly Notices,' vol. xxvii. p. 65.

† Vienna Academy, Sitzungsbericht, vol. liv. pt. ii. 6th Dec. 1866.

‡ Bullettino Meteorologico del Collegio Romano, vol. v. p. 121.

§ Stelle Cadenti osservate in Piemonte, nel 1866. By P. Barnabita. Turin, 1867, p. 25.

‖ 'Comptes Rendus,' vol. xliii. p. 906.

¶ Bulletins de l'Académie Royale de Belgique, 2nd ser. vol. xxii. No. 12, 1866.

** 'Monthly Notices,' R.A.S. vol. xxvii. p. 48.

The geographical limits of visibility of the star-shower of 1866, it will at once be seen, coincide with the area over which the November meteors appeared in 1832. The latter shower was seen as far south as the Mauritius, as far east as Arabia and the Persian Gulf, and over the whole continent of Europe, with the British Isles, but it was not visible in America. It was, moreover, a moderate display, but it was followed, twelve months later in America, by the great storm of meteors which suddenly appeared on the morning of the 13th of November, 1833. The recent exhibition may therefore be regarded as the prelude of a similar meteor-rain in America, perhaps partially visible in Europe, as great and bright as the two star-storms seen in America, and partially visible in Europe, in the years 1799 and 1833.

Unless unforeseen curvatures of the meteoric current disturb the geographical boundaries of the display, the first symptoms of the approaching star-shower will be perceived at day-break in England, on the morning of the 14th of November, 1867, when the light of the moon, then three days past the full, and of dawn appearing, will detract something from the numbers and brightness of the meteors. But the same oscillation of the curves in an opposite direction, it should be borne in mind, will bring Great Britain into full view of the centre of the shower, and make the principal spectacle of the meteors visible in Europe *before* day-break, as well as in America.

A leading feature of the great display in 1866, was the surprisingly brief duration of the shower, and the almost sudden rapidity of its appearance and disappearance. Speaking of the remarkable scarcity of meteors on the morning of the 13th, Mr. Serpieri at Urbino writes to Mr. Secchi at Rome:—"I began to doubt whether even the ordinary meteors of the November epoch would not this year be altogether wanting. But in the end, it seemed as if all those ordinary meteors had gathered themselves together into one dense array, to make their transit in the shortest possible time!"* The definable character of the shower led to a large number of accurate observations being made, on the moment of its maximum abundance. This was generally observed in England to have taken place first, about ten minutes past one o'clock; while a second maximum, hardly less marked, was observed at twenty minutes past one. At the Cape of Good Hope Observatory the shower reached its maximum at two minutes before one o'clock (Greenwich time), and afterwards pretty steadily and very rapidly declined. The difference, which amounts to about a quarter of an hour, is easily explained, if the oblique direction is considered, in which the earth at this juncture traversed the meteoric current. The radiant point of the shower was determined on this occasion with more than ordinary exactness, and few observers differ far from fixing it near the small star, *x* (Bode) at the centre of Leo's sickle, being the very position assigned to it by Professor Twining, at the last great appearance of the shower on the 13th of November, 1833.

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* 'Les Mondes,' 2nd ser. vol. xii. p. 641.

It was early pointed out by Encke that this position of the radiant point is almost vertically over the point of the ecliptic, towards which the earth is moving at the moment. Supposing a pointer laid against a flat horizontal ring, to indicate the direction in which the earth is moving in its orbit on the 14th of November. If the pointer is then inclined a little upwards (about 10½ degrees), it indicates the position of the radiant point, or the direction from which the meteors appear to come. But as the earth itself is advancing to meet the shower, the real slope of the meteoric current is less oblique (about 17°) than it appears to be, at the point of the ecliptic where the earth encounters it. The *southern side* of the earth must evidently meet the sloping current first, after which the equatorial parts, and lastly the northern side of the earth, will be plunged into the stream. Twenty-four minutes would be required for the whole earth to become immersed, and thirteen minutes should elapse (which was very nearly the interval observed) from the time when the maximum display was experienced at the Cape of Good Hope, until the like should be perceived in England. A more favourable opportunity could hardly have been expected for turning such observations to account; and the general mode of apprehending the phenomenon is shown to be substantially correct, by the satisfactory manner in which they answer to the test.

In twenty-four minutes the whole width of the earth's diameter would enter the stream. As the denser part of the shower lasted an hour and a half, its thickness was nearly equal to four diameters of the earth, or about 30,000 miles.

Only two or three of the brightest meteors observed at any one station, were brighter than the planets. One such was seen after sunrise at the Observatory at Athens. An equally bright meteor was seen by Mr. Crumplen as late as nine o'clock in the morning, at Primrose Hill, in London. The scale of magnitudes of eighty-one meteors, whose paths were recorded by the speaker, with the assistance of Mr. A. Macgregor, at the Glasgow Observatory, were as follows :—

As bright as Jupiter, or brighter . . .	2	meteors	= 3	per cent.
As bright as Sirius	14	,,	= 17	,,
1st mag. stars	39	,,	= 48	,,
2nd mag. stars	26	,,	= 32	,,
	81		100	

The meteors were in general white, occasionally inclining to blue, and frequently to tints of orange-red. Every meteor left a peculiarly hard and solid-looking straight streak of light upon its whole course, *ahead* of which the head was occasionally observed to shoot in its flight, very seldom leaving any large sparks upon its track, and rarely, if ever, terminating with an explosion, but disappearing by degrees, as if the material substance of the meteor was expended. The streaks were lance-like, tapering to the extremities; and commonly faded in a few seconds, from the ends towards the centre, without losing their lance-like form. But in the case of a good many streaks which remained visible for some minutes, the most persistent part of the train diffused itself into spiral, snake-like, scymitar-shaped, and every possible variety of nebulous-looking clouds, of silvery white, or in some very long-enduring cases, of flame-coloured light. Of the latter kind was the stationary streak over Dundee, in Scotland, whose real height and position are described, approximately, on the next page. Dr. Schmidt describes a streak at Athens, which remained visible in the sky fifty-one minutes, like a red cumulus cloud, only effaced by the approach of daylight. The evident tendency of the most persistent streaks seen in England, was to drift with a pretty rapid motion towards the south, or to a few points west of south, until they disappeared. A very persistent light-cloud left by a large meteor of the shower *in America* on the 13th of November, 1833, was calculated by Professor Twining to have drifted from its place *eastwards* with a speed of three or four miles in a minute.

An inquiry projected last year,* with reference to analyzing the light of the meteors by their spectrum, was put in practice; and meteoric spectra were observed both on the nights of the 9th and 10th of August, and on the morning of the 14th of November, 1866. Meteor spectroscopes were constructed by Mr. Browning in good time for the first of those occasions, and seventeen views of the spectra of the meteor streaks and nuclei were obtained. The prevailing character, in the freshly deposited streaks, was a continuous spectrum of considerable width, but destitute of colour. But as soon as some of the streaks began to fade, in eight cases there remained nothing but an extremely slender and bright yellow line of light, manifestly the light of some self-luminous gas, of which the nearest analogue in terrestrial flames is the light of incandescent sodium vapour. The spectra of the nuclei generally presented all the brighter colours of the prismatic spectrum. When feeble, the spectrum was still continuous, although destitute of colour, but three examples of nuclei were observed, which presented nearly homogeneous yellow light; one entirely destitute, and the other two accompanied only by a very faint continuous spectrum.

The nuclei of the November meteors presented to Mr. Browning the same peculiar features in the spectroscope as those which the speaker had already noted in the spectra of the nuclei of the August meteors; namely, a marked preponderance of a line, or broad band of yellow light, which sometimes appeared to form the totality of their spectrum; with the addition that two nuclei were observed by Mr. Browning, among the November meteors, whose light was equally homogeneous, but green. The light of the streaks, which was mostly blue, green, or steel-grey, generally appeared homogeneous,* and this observation the speaker was able to confirm. Although the experiments presented peculiar difficulties from the rapidly-fading character of the light-streaks, yet the homogeneous appearance of the light of the November meteor-streaks in the spectroscope was, in certain cases, so unequivocal, that their bluish-green colour immediately suggested to him the suspicion, which was, at the time, nothing more, but which now appears highly probable, that an analogy exists between their light and that of the gaseous nebulæ, and, particularly, of the nucleus of Tempel's comet.

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* Proceedings, R. I. vol. iv. p. 650.

The principal result of the meteor-spectroscopic observations was, that the total absence of the presumed bright line of sodium in the spectra of the streaks of the November meteors establishes a specific difference between the meteoric substances of the two currents, to which the August and November shooting-stars belong.†

Observations of one of the largest meteors of the shower were obtained at Sunderland, in England, and at Edinburgh, Aberdeen, and Glasgow, in Scotland.‡ The meteor appeared at twenty minutes before three o'clock, and the streak, to which the observations principally refer, remained visible a quarter of an hour. The nebulous cloud of light very nearly marked the point of disappearance of the meteor, and its altitude was ascertained to be between fifty-one and fifty-seven miles above the earth's surface, over a spot a few miles distant from Dundee.

In a series of letters addressed to Padre Secchi, the celebrated astronomer of Rome,§ shortly before the recent appearance of the November shower, a fortunate theory of periodical meteors, announced by Sig. G. V. Schiaparelli, the astronomer of the Brera College at Milan, completely removes the obscurity of their origin, and claims for its distinguished author the praise of having, for the first time since the cosmical hypothesis of Chladni, laid a broad and sure foundation for a new science of meteoric astronomy.

Supposing that a cosmical cloud of particles should be drawn from stellar space by the sun's attraction, it is shown that it could not cross the earth's orbit in any other form than as a parabolic current.

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* 'Monthly Notices,' R.A.S. vol. xxvii. p. 78.

† The orange tint of the nucleus of Comet II., 1862 (now supposed to be a member of the material current which furnishes the August meteors) occurred to the writer as being very remarkable to the naked eye, at the time of its appearance. It was noted with the telescope by Mr. Knott, in his 'Observations of the Comet at the Woodcroft Observatory, Cuckfield, Sussex.' See 'Monthly Notices,' R.A.S. vol. xxiii. p. 31.

‡ 'Proceedings of the Glasgow Philosophical Society,' vol. vi. p. 207.

§ 'Bullettino Meteorologico del Collegio Romano,' vol. v. Nos. 8, 10, 11, 12, and vol. vi. No. 2.

Assuming, for example, that such a cloud, of the sun's size, was originally moving at an aphelion distance of 20,000 times the earth's distance from the sun, with a velocity of only one hundred yards in a minute, and with no relative motions, or mutual attractions, existing among its particles, the cloud would represent at that distance a nebula of only one-tenth of a second of arc in angular width. In about one-and-a-half million years the cloud would arrive at less than the earth's distance from the sun, being then in the perihelion point of its orbit, which would there be undistinguishable from a parabola. By the inevitable laws of the sun's attraction, the cloud will be gradually deformed in its progress, until on the point of its perihelion passage it will become a current twenty-three miles broad, one hundred yards deep, in a direction measured from the sun, and extending upwards of six hundred millions of miles along the arc of the parabola. The perihelion passage of the current will occupy more than a year (387 days) and its particles will be four hundred million times more closely packed together than they were in the cosmical cloud before its deformation by the sun's attraction. As there are nebulæ in the sky larger than the sun's apparent disc, if that width only were adopted for the apparent magnitude of the cosmical cloud at its aphelion, it would be transformed into a parabolic current, which would occupy twenty thousand years in its perihelion passage. The transverse width of the current would at the same time be proportionately greater than in the previous case, yet not so large but that the earth will pass through it in a few hours, or at most, in one or two days. In this manner, avoiding all impossible assumptions, meteoric currents may clearly be accounted for, which, like that of August, have been visible for hundreds or thousands of years.

On the other hand a meteoric stream like that of November, which is visible for two or three years in succession at the end of every cycle of about thirty-three years, must be moving in a very much shorter ellipse, and must occupy a certain arc of that ellipse with the requisite materials for a meteoric shower. Mr. Le Verrier supposed * that a cosmical cloud, like those assumed to exist by Mr. Schiaparelli, was thrown into such an elliptic orbit by the action of the planet Uranus, and that the cloud must actually have passed close to Uranus in the year A.D. 126. The action of the planet having caused some of its parts to move faster than the rest, the cloud is gradually becoming transformed into a ring, but it is not yet such an ancient member of the solar system as, if the same kind of hypothesis could in that case be entertained, Mr. Le Verrier supposes the ring of the August meteors to represent.

It may be remarked that the earth passes periodically through the current of the November meteors, without sufficient attractive efficacy, while scattering some of the meteors, to deflect the main body of the current from its course. That the earth, however, in common with

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* 'Comptes Rendus,' vol. xliv. p. 94.

the larger planets, must produce a sensible effect upon the direction of the stream, is certain; and Mr. Adams has shown,* as the result of very elaborate calculation, that assuming the orbit of the November meteoric current to be a long ellipse extending to near the orbit of Uranus, with a periodic time of 33·25 years, the joint effect of all the more important planets upon its course, must be to retard the date of the appearance of the shower one day at the end of every such cycle. The modern November star showers, of the 12th of November, 1799, the 13th of November, 1832, and 1833, and the 14th of November, 1866, as well as the much more decisive evidence derived from earlier displays of the same shower completely establish the correctness of this hypothetical form of the orbit; while other calculations show that no other possible orbit which the meteors might pursue consistently with the existence of a thirty-three-year period in the date of their returns, would agree, as the former orbit does, in a precise and accurate manner, with the very prominent variation of the date.

Periodical meteors are accordingly found to move in very eccentric orbits, inclined like the orbits of the comets at all possible obliquities to the ecliptic. Nor is this similarity of their orbits merely a conjecture. In the fourth of his series of letters to Padre Secchi,† Mr. Schiaparelli announces the important discovery, that a conspicuous and well-remembered comet (Comet III., 1862), which was visible to the naked eye for several weeks in August and September, 1862, and which Stämpfer calculated to have a periodic time of 113 years, coincides almost exactly in its path with the stream, supposed nearly parabolic, of the Perseids, as the 10th of August meteors are styled by Schiaparelli.

A period of revolution of 108 years is assigned to the August meteors by Schiaparelli, and one of 103 years was proposed by the speaker‡ last year, as bringing the August star-showers of A.D. 830, 933, 1243, and 1451, into conjunction with the remarkable star-shower of the 10th of August, 1863. The near coincidence of the time of the comet's return with the last-named great display of the "Perseids," affords a strong presumption that a connection of an intimate kind exists between the two important classes of bodies. Not less striking is the coincidence of the return of another periodical comet (Comet I., 1866), known as Tempel's comet, visible with the telescope, which passed its perihelion in January, 1866, with the recent reappearance of the November meteors. The November meteors and Tempel's comet, as calculated by Oppolzer, pass at the same time close to the earth's orbit, at the same ecliptic longitude, at the same obliquity to the ecliptic, and in the same direction round the sun. The periodic time of their return is also the same, so that their orbits must, evidently, coincide throughout their whole extent. Dr. Galle, the director of the Observatory at Breslau, has recently pointed out a coincidence which exists between the elements of the orbit of a comet (Comet I., 1861) visible to the naked eye, and the orbit, supposed nearly parabolic, of the meteors of the 20th of April. The closeness of the coincidence makes it probable that the periodic time of 415 years, assigned to the comet, is also the periodic time of revolution of the April ring of meteors.

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* 'Monthly Notices, R.A.S.' vol. xxvii. p. 247.

† 'Bullettino Meteorologico del Collegio Romano,' vol. v. No. 12.

‡ Proceedings R.I. vol. iv. p. 646.

Of remarkable meteors visible in the past year, although only twice as bright as Sirius, one, having an extraordinary length of path, appeared at ten o'clock on the evening of the 11th of January, 1866. It shot almost horizontally across the English Channel from eighty-five miles over Paris to ninety-five miles over Cork, performing a path of not less than four hundred and fifty miles in about six or eight seconds.* The most experienced observers were astonished at the length of its luminous track, and compared it to "a bombshell fired from London into Ireland."

At midnight on the 10th of March, 1866,† a detonating meteor was seen in Hanoverian Prussia, which, the investigation of Dr. Heis informs us, shot from thirteen miles over Miete to three miles and a half over Lübbecke, a distance of thirty miles, in four or five seconds, with a speed of seven miles in a second. The detonation was so loud at Lübbecke as to awaken many from their sleep, with the noise, which was like a clap of thunder.

A similar meteor to the last appeared at eleven o'clock in the forenoon (Paris time) on the 20th of June, 1866, over Calais and Boulogne. It left a smoke-like train, that remained visible in broad daylight for several minutes.‡

The path of this fireball was from fifteen miles over Calais to four and a half miles over the neighbourhood of Boulogne and Montreuil-in-Somme. The meteor pursued a path of thirty miles in three and a half seconds, with a speed of eight or nine miles in a second, disappearing with a detonation which was heard, startlingly loud, as far as Maidstone in Kent, and Hastings in Sussex. At St. Omer it was at first believed that the neighbouring powder-mill of Esquerbes had exploded.

Three detonating meteors, to which allusion was made last year,§ made their appearance in England between the 19th and the 21st of November, during the five years 1861–65. The altitudes and other particulars of these meteors were defined. A fourth detonating fireball may now be added to the same list, if the following announcement in the New York 'World' of the 24th of November, 1866, relates to a "meteorological phenomenon" which actually took place as there described:—

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* 'British Association Report,' 1866, pp. 84 and 126. As seen at Ticehurst, in Sussex, the meteor passed over 160° of the sky, appearing very near the horizon in the east, shooting overhead, and disappearing close to the west horizon.

† Twenty minutes after midnight, on the morning of the 11th of March, Münster time. A pamphlet by Prof. Heis, with map of meteor's course (8vo, Halle, 1866, H. W. Schmidt).

‡ British Association Report, 1866, pp. 126 and 128.

§ Proceedings R.I. vol. iv. p. 649.

"At Nashville, about four o'clock last Tuesday morning (the 20th of November, 1866), a meteor, lighting the whole heavens, was seen in the direction of Rome, Ga., moving rapidly south-west. It appeared like a ball of fire as large as the sun. It exploded apparently ten miles off, with a tremendous report, like a 40lb. cannon, that shook the earth, and made the windows rattle."

In addition to two stonefalls, alluded to last year,* which happened, one at Shergotty, in India, and the other at Aumale, in Algeria, on the 25th of August, 1865, three other stonefalls took place, one an aërolite weighing about 5lbs., which fell at Dundrum,† in Ireland, with the usual accompaniment of a detonation, followed by a humming sound, on the 12th day of the same month and year. The second was an aërolite weighing about 3lbs., which fell at Bheenwall,‡ in Bengal, on the 20th of August; and the third, a fall of two stones at Muddoor,§ in India, on the 7th of September in the same year, making, with the two stonefalls already mentioned, a total of five aërolites in four weeks.

The next accounts of the same kind which have been received relate to a stonefall at St. Mesmin, in France, on the 30th of May, 1866, followed in ten days by the shower of stones at Knyahinya, in Hungary, on the 9th of June, 1866. At St. Mesmin, in the valley of the Seine, there fell, about four o'clock in the morning, three aërolites, one of them with such a clattering, and buzzing, and shrieking sound, that a signal man in the railway cutting, where it fell, remained for some time mute with affright.

The stone when picked up weighed 1lb., and a small indentation, about half-an-inch in width, with a fresh surface, covered with thin thread-like lines of the fused crust, marks a spot where an angle must have been broken off from the meteorite during its flight through the air.‖

The second and last stonefall recorded in the year is the shower of stones which happened between four and five o'clock in the afternoon at Knyahinya, near the northern frontier of Hungary.¶ It ranks as one of the greatest events of the kind, since the fall of about two hundred stones at Stannern, in Moravia, which strewed themselves over an area nine miles long by six miles wide. The largest stone at Knyahinya weighed six hundred-weights, and excavated a hole four feet deep and five feet wide, where it penetrated the ground to an extent of twelve feet in a slightly slanting direction from seventy-six degrees east of north to seventy-six degrees west of south. Other fragments, weighing from seven to thirty-seven, and one of them ninety pounds, were picked up near the larger mass, and about a thousand fragments, weighing together near upon a thousand pounds, were scattered over an area nine miles long, from north-east to south-west, and four miles wide. All the fragments are completely crusted over, and the meteor was seen, and the detonation was heard over a distance of sixty or eighty miles in a south-west direction.*

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* Proceedings R.I. vol. iv. p. 649.

† Scientific Papers from the Royal Irish Academy's Proceedings, vol. i. p. 230.

‡ British Association Report, 1866, p. 133.

§ 'Meteors and Aërolites.' By Dr. Phipson, p. 227.

‖ 'Comptes Rendus,' vol. xlii. 1866, June 18th.—A small lath of heavy wood astened to the end of a piece of string, and whirled round with the hand so as to describe a pretty wide circle, gives out a loud humming noise. The sounds which may be produced in this manner are evidently of the same kind as those which aërolites give out in falling through the air. Angular pieces of iron projected from a common sling produce in the same way a strange variety of humming, buzzing, and "shrieking" noises.—(A. S. H.)

¶ Vienna Academy, 'Sitzungsbericht,' vol. liv. 12th July, 1866.

As fragments weighing a few pounds, or a few hundred-weights only, could not exist as a group with intervals of more than a hundred yards between them, at the earth's distance from the sun, without being disjoined by the sun's attractive force; and as it is difficult to imagine how such a compact group could be formed, even beyond the sphere of the sun's attraction, capable of resisting the sun's disturbing action at the earth, it is more probable that the shower of aërolites entered the atmosphere as a single stone. This was certainly the case with fragments of the meteorite of Butsura, two of which, picked up at a distance of more than two miles apart, were completely crusted over; and yet, when placed side by side, they have a nearly close-fitting junction, and are obviously seen to belong to one piece, by a vein of iron, which runs directly across the junction through the mass.

The most probable theory of the zodiacal light makes it to consist of numberless small bodies, revolving like planets round the sun, whose joint reflection of the sun's rays gives the appearance of a luminous haze stretching along the zodiac, and brightest in the neighbourhood of the sun. On particularly clear nights in the tropics, it sometimes appears to complete the circle of the midnight sky, from east to west, having been described, and frequently seen thus by Mr. G. Jones, on the long voyage of the Japan expedition from New York.†

The earth is, therefore, situated within its outer border. But if the bodies were spherical, and uniformly scattered between the earth and the sun, the theory of their phases, and maximum brightness, as seen from the earth, would lead us to expect a species of mock-suns, about fifty-five degrees distant from the true sun, one at each extremity of the light. As this is not observed, and as, on the contrary, both terminations of the light are diffuse, the bodies which compose it must, certainly, be more thinly scattered towards its edge, and it is even probable that they must consist of shapeless fragments, which is exactly what the circumstances of the falls of aërolites would lead us to expect.

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* Vienna Academy, 'Sitzungsbericht,' vol. liv. 11th October, 1866.

† Observations on the Zodiacal Light, from April 2, 1853, to April 22, 1855. By Rev. George Jones, A.M. 'United States Japan Expedition,' vol. iii. p. 84.

The general presence of nickel, and the remarkable deficiency of oxygen in meteorites, cannot be urged against the probability of their originating in a boundless field of bodies, like the zodiacal light, because the sun itself, which is the largest body of the system, is shown by twenty or more lines of the solar spectrum to contain nickel; and the abundance of oxygen on the earth's surface, far from being a general proof of its redundancy in planetary and other stars, may be a peculiar circumstance of this planet, and perhaps of few other members of the material universe. The particular exhibition of an excess of free oxygen upon the surface of the earth will appear a much more exceptional condition, if we assign a wider field to the origin of aërolites, by accepting Mr. Schiaparelli's ninth postulate, in which he thus sums up his views regarding their extraction:—

"Since it may be regarded as certain that falling-stars, bolides and aërolites, differ from each other only in their magnitudes, we must conclude that the substance fallen from the sky *is a sample of that of which the stellar universe is composed.* And since in such substance there is no chemical element unknown upon the earth, the similarity of the composition of all the visible bodies of the universe, already rendered probable by researches with the spectroscope, receives a new argument of credibility." *

[A. S. H.]

WEEKLY EVENING MEETING,

Friday, May 31, 1867.

Sir Henry Holland, Bart. M.D. D.C.L. F.R.S. in the Chair.

T. Sterry Hunt, Esq. LL.D. F.R.S.

On the Chemistry of the Primeval Earth.

The natural history of our planet, to which we give the name of geology, is, necessarily, a very complex science, including, as it does, the concrete sciences of mineralogy, of botany, and zoology, and the abstract sciences of chemistry and physics. These latter sustain a necessary and very important relation to the whole process of development of our earth, from its earliest ages, and we find that the same chemical laws which have presided over its changes, apply also to those of extra-terrestrial matter. Recent investigations show the presence in the sun, and even in the fixed stars—suns of other systems—the same chemical elements as in our own planet. The spectroscope, that marvellous instrument, has, in the hands of modern investigators, thrown new light upon the composition of the farthest bodies of the universe, and has made clear many points which the telescope was impotent to resolve. The results of extra-terrestrial spectroscopic research have lately been set forth in an admirable manner by one of its most successful students, Mr. Huggins. We see, by its aid, matter in all its stages, and trace the process of condensation and the formation of worlds. It is long since Herschel, the first of his illustrious name, conceived the nebulæ, which his telescope could not resolve, to be the uncondensed matter from which worlds are made. Subsequent astronomers, with more powerful glasses, were able to show that many of these nebulæ are really groups of stars, and thus a doubt was thrown over the existence in space of nebulous luminous matter; but the spectroscope has now placed the matter beyond doubt. By its aid we find in the heavens, planets, bodies like our earth, shining only by reflected light; suns, self-luminous, radiating light from solid matter; and, moreover, true nebulæ, or masses of luminous gaseous matter. These three forms represent three distinct phases in the condensation of the primeval matter, from which our own and other planetary systems have been formed.

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* 'Bullettino Meteorologico del Collegio Romano,' vol. v. No. 11.

This nebulous matter is conceived to be so intensely heated as to be in the state of true gas or vapour, and, for this reason, feebly luminous when compared with the sun. It would be out of place, on the present occasion, to discuss the detailed results of spectroscopic investigation, or the beautiful and ingenious methods by which modern science has shown the existence in the sun, and in many other luminous bodies in space, of the same chemical elements that are met with in our earth, and even in our own bodies.

Calculations based on the amount of light and heat radiated from the sun show that the temperature which reigns at its surface is so great that we can hardly form an adequate idea of it. Of the chemical relations of such intensely heated matter, modern chemistry has made known to us some curious facts, which help to throw light on the constitution and luminosity of the sun. Heat, under ordinary conditions, is favourable to chemical combination, but a higher temperature reverses all affinities. Thus, the so-called noble metals, gold, silver, mercury, &c., unite with oxygen and other elements; but these compounds are decomposed by heat, and the pure metals are regenerated. A similar reaction was many years since shown by Mr. Grove with regard to water, whose elements—oxygen and hydrogen—when mingled and kindled by flame, or by the electric spark, unite to form water, which, however, at a much higher temperature, is again resolved into its component gases. Hence, if we had these two gases existing in admixture at a very high temperature, cold would actually effect their combination precisely as heat would do if the mixed gases were at the ordinary temperature, and literally it would be found that "frost performs the effect of fire." The recent researches of Henry Ste.-Claire Deville and others go far to show that this breaking up of compounds, or dissociation of elements by intense heat, is a principle of universal application; so that we may suppose that all the elements which make up the sun or our planet, would, when so intensely heated as to be in that gaseous condition which all matter is capable of assuming, remain uncombined—that is to say, would exist together in the condition of what we call chemical elements, whose further dissociation in stellar or nebulous masses may even give us evidence of matter still more elemental than that revealed by the experiments of the laboratory, where we can only conjecture the compound nature of many of the so-called elementary substances.

The sun, then, is to be conceived as an immense mass of intensely heated gaseous and dissociated matter, so condensed, however, that, notwithstanding its excessive temperature, it has a specific gravity not much below that of water; probably offering a condition analogous to that which Cagniard de la Tour observed for volatile bodies when submitted to great pressure at temperatures much above their boiling point. The radiation of heat, going on from the surface of such an intensely heated mass of uncombined gases, will produce a superficial cooling, which will permit the combination of certain elements and the production of solid or liquid particles, which, suspended in the still dissociated vapours, become intensely luminous and form the solar photosphere. The condensed particles, carried down into the intensely heated mass, again meet with a heat of dissociation; so that the process of combination at the surface is incessantly renewed, while the heat of the sun may be supposed to be maintained by the slow condensation of its mass; a diminution by $\frac{1}{1000}$th of its present diameter being sufficient, according to Helmholtz, to maintain the present supply of heat for 21,000 years.

This hypothesis of the nature of the sun and of the luminous process going on at its surface is the one lately put forward by Faye, and, although it has met with opposition, appears to be that which accords best with our present knowledge of the chemical and physical conditions of matter, such as we must suppose it to exist in the condensing gaseous mass, which, according to the nebular hypothesis, should form the centre of our solar system. Taking this, as we have already done, for granted, it matters little whether we imagine the different planets to have been successively detached as rings during the rotation of the primal mass, as is generally conceived, or whether we admit with Chacornac a process of aggregation or concretion, operating within the primal nebular mass, resulting in the production of sun and planets. In either case we come to the conclusion that our earth must at one time have been in an intensely heated gaseous condition, such as the sun now presents, self-luminous, and with a process of condensation going on at first at the surface only, until by cooling it must have reached the point where the gaseous centre was exchanged for one of combined and liquefied matter.

Here commences the chemistry of the earth, to the discussion of which the foregoing considerations have been only preliminary. So long as the gaseous condition of the earth lasted, we may suppose the whole mass to have been homogeneous; but when the temperature became so reduced that the existence of chemical compounds at the centre became possible, those which were most stable at the elevated temperature then prevailing, would be first formed. Thus, for example, while compounds of oxygen with mercury or even with hydrogen could not exist, oxides of silicon, aluminium, calcium, magnesium, and iron might be formed and condense in a liquid form at the centre of the globe. By progressive cooling, still other elements would be removed from the gaseous mass, which would form the atmosphere of the non-gaseous nucleus. We may suppose an arrangement of the condensed matters at the centre according to their respective specific gravities, and thus the fact that the density of the earth as a whole is about twice the mean density of the matters which form its solid surface may be explained. Metallic or metalloidal compounds of elements, grouped differently from any compounds known to us, and far more dense, may exist in the centre of the earth.

The process of combination and cooling having gone on until those elements which are not volatile in the heat of our ordinary furnaces, were condensed into a liquid form, we may here inquire what would be the result, upon the mass, of a further reduction of temperature. It is generally assumed that in the cooling of a liquid globe of mineral matter, congelation would commence at the surface, as in the case of water; but water offers an exception to most other liquids, inasmuch as it is denser in the liquid than in the solid form. Hence ice floats on water, and freezing water becomes covered with a layer of ice, which protects the liquid below. With most other matters, however, and notably with the various mineral and earthy compounds analogous to those which may be supposed to have formed the fiery-fluid earth, numerous and careful experiments show that the products of solidification are much denser than the liquid mass; so that solidification would have commenced at the centre, whose temperature would thus be the congealing point of these liquid compounds. The important researches of Hopkins and Fairbairn on the influence of pressure in augmenting the melting point of such compounds as contract in solidifying, are to be considered in this connection.

It is with the superficial portions of the fused mineral mass of the globe that we have now to do; since there is no good reason for supposing that the deeply seated portions have intervened in any direct manner in the production of the rocks which form the superficial crust. This, at the time of its first solidification, presented probably an irregular, diversified surface, from the result of contraction of the congealing mass, which at last formed a liquid bath of no great depth, surrounding the solid nucleus. It is to the composition of this crust that we must direct our attention, since therein would be found all the elements (with the exception of such as were still in the gaseous form) now met with in the known rocks of the earth. This crust is now everywhere buried beneath its own ruins, and we can only from chemical considerations attempt to reconstruct it. If we consider the conditions through which it has passed, and the chemical affinities which must have come into play, we shall see that they are just what would now result if the solid land, sea, and air were made to react upon each other under the influence of intense heat. To the chemist it is at once evident that from this would result the conversion of all carbonates, chlorides, and sulphates into silicates, and the separation of the carbon, chlorine, and sulphur in the form of acid gases, which, with nitrogen, watery vapour, and a probable excess of oxygen, would form the dense primeval atmosphere. The resulting fused mass would contain all the bases as silicates, and must have much resembled in composition certain furnace-slags or volcanic glasses. The atmosphere, charged with acid gases which surrounded this primitive rock must have been of immense density. Under the pressure of such a high barometric column, condensation would take place at a temperature much above the present boiling point of water, and the depressed portions of the half-cooled crust would be flooded with a highly heated solution of hydrochloric acid, whose action in decomposing the silicates is easily intelligible to the chemist. The formation of chlorides of the various bases, and the separation of silica, would go on until the affinities of the acid were satisfied, and there would be a separation of silica, taking the form of quartz, and the production of a sea-water holding in solution, besides the chlorides of sodium, calcium, and magnesium, salts of aluminium and other metallic bases. The atmosphere, being thus deprived of its volatile chlorine and sulphur compounds, would approximate to that of our own time, but differ in its greater amount of carbonic acid.

We next enter into the second phase in the action of the atmosphere upon the earth's crust. This, unlike the first, which was subaqueous, or operative only on the portion covered with the precipitated water, is sub-aerial, and consists in the decomposition of the exposed parts of the primitive crust under the influence of the carbonic acid and moisture of the air, which convert the complex silicates of the crust into a silicate of alumina, or clay, while the separated lime, magnesia, and alkalies, being converted into carbonates, are carried down into the sea in a state of solution. The first effect of these dissolved carbonates would be to precipitate the dissolved alumina and the heavy metals, after which would result a decomposition of the chloride of calcium of the sea-water, resulting in the production of carbonate of lime or limestone, and chloride of sodium or common salt. This process is one still going on at the earth's surface, slowly breaking down and destroying the hardest rocks, and, aided by mechanical processes, transforming them into clays; although the action, from the comparative rarity of carbonic acid in the atmosphere, is less energetic than in earlier times, when the abundance of this gas, and a higher temperature, favoured the chemical decomposition of the rocks. But now, as then, every clod of clay formed from the decay of a crystalline rock corresponded to an equivalent of carbonic acid abstracted from the atmosphere, and equivalents of carbonate of lime and common salt formed from the chloride of calcium of the sea-water.

It is very instructive, in this connection, to compare the composition of the waters of the modern ocean with that of the sea in ancient times, whose composition we learn from the fossil sea-waters which are still to be found in certain regions, imprisoned in the pores of the older stratified rocks. These are vastly richer in salts of lime and magnesia than those of the present sea, from which have been separated, by chemical processes, all the carbonate of lime of our limestones, with the exception of that derived from the sub-aerial decay of calcareous and magnesian silicates belonging to the primitive crust.

The gradual removal, in the form of carbonate of lime, of the carbonic acid from the primeval atmosphere, has been connected with great changes in the organic life of the globe. The air was doubtless at first unfit for the respiration of warm-blooded animals, and we find the higher forms of life coming gradually into existence as we approach the present period of a purer air. Calculations lead us to conclude that the amount of carbon thus removed in the form of carbonic acid has been so enormous, that we must suppose the earlier forms of air-breathing animals to have been peculiarly adapted to live in an atmosphere which would probably be too impure to support modern reptilian life. The agency of plants in purifying the primitive atmosphere was long since pointed out by Brongniart, and our great stores of fossil fuel have been derived from the decomposition, by the ancient vegetation, of the excess of carbonic acid of the early atmosphere, which through this agency was exchanged for oxygen gas. In this connection the vegetation of former periods presents the curious phenomenon of plants allied to those now growing beneath the tropics, flourishing within the polar circles. Many ingenious hypotheses have been proposed to account for the warmer climate of earlier times, but are at best unsatisfactory, and it appears to me that the true solution of the problem may be found in the constitution of the early atmosphere, when considered in the light of Dr. Tyndall's beautiful researches on radiant heat. He has found that the presence of a few hundredths of carbonic acid gas in the atmosphere, while offering almost no obstacle to the passage of the solar rays, would suffice to prevent almost entirely the loss by radiation of obscure heat, so that the surface of the land beneath such an atmosphere would become like a vast orchard-house, in which the conditions of climate necessary to a luxuriant vegetation would be extended even to the polar regions. This peculiar condition of the early atmosphere cannot fail to have influenced in many other ways the processes going on at the earth's surface. To take a single example: one of the processes by which gypsum may be produced at the earth's surface involves the simultaneous production of carbonate of magnesia. This, being more soluble than the gypsum, is not always now found associated with it; but we have indirect evidence that it was formed and subsequently carried away, in the case of many gypsum deposits, whose thickness indicates a long continuance of the process under conditions much more perfect and complete than we can attain under our present atmosphere. While studying this reaction I was led to inquire whether the carbonic acid of the earlier periods might not have favoured the formation of gypsum; and I found, by repeating the experiments in an artificial atmosphere impregnated with carbonic acid, that such was really the case. We may thence conclude that the peculiar composition of the primeval atmosphere was the essential condition under which the great deposits of gypsum, generally associated with magnesian limestones, were formed.

The reactions of the atmosphere which we have considered, would have the effect of breaking down and disintegrating the surface of the primeval globe, covering it everywhere with beds of stratified rock of mechanical or of chemical origin. These would now so deeply cover the partially cooled surface that the amount of heat escaping from below is inconsiderable, although in earlier times it was very much greater, and the increase of temperature met with in descending into the earth must have been many times more rapid than now. The effect of this heat upon the buried sediments would be to soften them, producing new chemical reactions between their elements, and converting them into what are known as crystalline or metamorphic rocks, such as gneiss, greenstone, granite, &c. We are often told that granite is the primitive rock or substratum of the earth, but this is not only unproved, but extremely improbable. As I endeavoured to show in the early part of this discourse, the composition of this primitive rock, now everywhere hidden, must have been very much like that of a slag or lava; and there are excellent chemical reasons for maintaining that granite is in every case a rock of sedimentary origin—that is to say, it is made up of materials which were deposited from water, like beds of modern sand and gravel, and includes in its composition quartz, which, so far as we know, can only be generated by aqueous agencies, and at comparatively low temperatures.

The action of heat upon many buried sedimentary rocks, however, not only softens or melts them, but gives rise to a great disengagement of gases, such as carbonic and hydrochloric acids, and sulphur compounds, all results of the reaction of the elements of sedimentary rocks, heated in presence of the water which everywhere filled their pores. In the products thus generated we have a rational explanation of the chemical phenomena of volcanoes, which are vents through which these fused rocks and confined gases find their way to the surface of the earth. In some cases, as where there is no disengagement of gases, the fused or half-fused rocks solidify *in situ*, or in rents or fissures in the overlying strata, and constitute eruptive or plutonic rocks like granite and basalt.

This theory of volcanic phenomena was put forward in germ by Sir John F. W. Herschel thirty years since, and, as I have during the past few years endeavoured to show, it is the one most in accordance with what we know both of the chemistry and the physics of the earth.

That all volcanic and plutonic phenomena have their seat in the deeply buried and softened zone of sedimentary deposits of the earth, and not in its primitive nucleus, accords with the conclusions already arrived at relative to the solidity of that nucleus, with the geological relations of these phenomena as I have elsewhere shown; and also with the remarkable mathematical and astronomical deductions of the late Mr. Hopkins, of Cambridge, based upon the phenomena of precession and nutation; those of Archdeacon Pratt; and those of Professor Thompson on the theory of the tides; all of which lead to the same conclusion—namely, that the earth, if not solid to the centre, must have a crust several hundred miles in thickness, which would practically exclude it from any participation in the plutonic phenomena of the earth's surface, except such as would result from its high temperature communicated by conduction to the sedimentary strata reposing upon it.

The old question between the plutonists and the neptunists, which divided the scientific world in the last generation, was, in brief, this—whether fire or water had been the great agent in giving origin and form to the rocks of the earth's crust. While some maintained the direct igneous origin of such rocks as gneiss, mica-schist, and serpentine, and ascribed to fire the filling of metallic veins, others—the neptunian school—were disposed to shut their eyes to the evidences of igneous action on the earth, and even sought to derive all rocks from a primal aqueous magma. In the light of the exposition which I have laid before you this evening, we can, I think, render justice to both of these opposing schools. We have seen how actions dependent on water and acid solutions have operated on the primitive plutonic mass, and how the resulting aqueous sediments, when deeply buried, come again within the domain of fire, to be transformed into crystalline and so-called plutonic or volcanic rocks.

The scheme which I have endeavoured to put before you in the short time allotted, is, as I have endeavoured to show, in strict conformity with known chemical laws and the facts of physical and geological science. Did time permit, I would gladly have attempted to demonstrate at greater length its adaptation to the explanation of the origin of the various classes of rocks, of metallic veins and deposits, of mineral springs, and of gaseous exhalations. I shall not, however, have failed in my object, if, in the hour which we have spent together, I shall have succeeded in showing that chemistry is able to throw a great light upon the history of the formation of our globe, and to explain in a satisfactory manner some of the most difficult problems of geology; and I feel that there is a peculiar fitness in bringing such an exposition before the members of this Royal Institution, which has been for so many years devoted to the study of pure science, and whose glory it is, through the illustrious men who have filled, and those who now fill, its professorial chairs, to have contributed more than any other school in the world to the progress of modern chemistry and physics.

[T. S. H.]

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[T. S. H.]

GENERAL MONTHLY MEETING,

Monday, June 3, 1867.

Sir Henry Holland, Bart. M.D. D.C.L. F.R.S. President, in the Chair.

The Hon. John William Strutt, and
Mrs. Susan Lumley

were *elected* Members of the Royal Institution.

Frederick James Chester, Esq.

was *admitted* a Member of the Royal Institution.

The special thanks of the Members were returned for the following addition to "the Donation Fund for the Promotion of Experimental Researches."

Sir Henry Holland, Bart. the President (9th Annual Donation) .	£40.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Imperial Government of France (through M. Duruy, Minister of Public Instruction)—Documents Inédits sur l'Histoire de France :
  Négociations Diplomatiques de la France avec la Toscane. Tome III. 4to.
  Lettres, &c., du Cardinal Richelieu. Tome V. 4to. 1863.
 Œuvres de Lavoisier. Tomes I. III. 4to. 1864–65.
 Œuvres d'Augustin Fresnel. Tome I. 4to. 1866.
 Histoire Générale de Paris. Topographie Historique du Vieux Paris. Par A. Berty. Tome I. 4to. 1866.
Agricultural Society of England, Royal—Journal. New Series. No. 5. 8vo. 1867.
Architects, Royal Institute of British—Proceedings, 1867. 4to.
Asiatic Society of Bengal—Proceedings, 1866, Nos. 4–13; and 1867, No. 1. 8vo.
Astronomical Society, Royal—Monthly Notices, Vol. XXVII. No. 6, 7. 8vo. 1867.
Blackie, Professor J. S. (the Author)—On Forms of Government. (K 94) 8vo. 1867.
Chemical Society—Journal for May, 1867. 8vo.
Dobell, Horace, M.D. (the Author)—The True First Stage of Consumption. 16to. 1867.
Editors—Artizan for May, 1867. 4to.
 Athenæum for May, 1867. 4to.
 British Journal of Photography for May, 1867. 4to.
 Chemical News for May, 1867. 4to.
 Engineer for May, 1867. fol.
 Horological Journal for May, 1867. 8vo.
 Journal of Gas-Lighting for May, 1867. 4to.
 Mechanics' Magazine for May, 1867. 8vo
 Pharmaceutical Journal for May, 1867.

Figueroa, Senhor Rafael Pardo de (the Author)—Critica del Regiemēto de Navigacio, &c. (L 14) 8vo. 1867.

Frankland, Professor E., F.R.S. (the Author)—Course of Six Lectures on Coal Gas. (K 94) 8vo. 1867.

Franklin Institute, Philadelphia—Journal, No. 496. 8vo. 1867.

Georgifili, Reale Accademia de'—Atti. Vol. XIII. 8vo. 1866.

Henry, William C., M.D. F.R.S. M.R.I. (the Author)—Biographical Notice of the late Very Rev. Richard Dawes, Dean of Hereford. 8vo. 1867.

Jablonowski'sche Gesellschaft, Leipzig—Preisschriften XII. 4to. 1867.

Jones, Henry Bence, M.D. F.R.S. Hon. Sec. R.I. (the Author)—Lectures on some of the Applications of Chemistry and Mechanics to Pathology and Therapeutics. 8vo. 1867.

Leeds Literary and Philosophical Society—Annual Report, 1865-66. 8vo. 1866.

Macfarren, G. A. Esq. (the Author)—Six Lectures on Harmony. Delivered at the Royal Institution, before Easter, 1867. 8vo. 1867.

Royal Academy of Sciences, Madrid—Libros del Saber de Astronomia del Rey Alfonso X. de Castilla. Tomo IV. fol. 1866.

Royal Society of London—Proceedings, No. 92. 8vo. 1867.

Philosophical Transactions for 1866. Vol. CLVI. Part 2. 4to. 1867.

Photographic Society—Journal, No. 181. 8vo. 1867.

Scharf, George, Esq. F.S.A. (the Author)—Observations on the Westminster Abbey Portrait and other Representations of Richard II. 8vo. 1867.

Smithsonian Institution, Washington—Annual Report for 1864. 8vo. 1865.

Sofka, Dr. Franz O. (the Author)—Die kosmischen Abkühlungen, ein meteorologisches Prinzip. (K 94) 8vo. 1867.

Taylor, Rev. W., F.R.S. M.R.I.—Ob der Künig vss Engelland ein lügner sey oder der Luther. [Von Thomas Murner.] 4to. [Strasburg, 1522.]

West Riding of Yorkshire Geological and Polytechnic Society—Proceedings, 1865–66. 8vo. 1867.

WEEKLY EVENING MEETING,

Friday, June 7, 1867.

SIR HENRY HOLLAND, Bart. M.D. D.C.L. F.R.S. President, in the Chair.

JOHN RUSKIN, Esq. M.R.I.

On the present State of Modern Art with reference to the advisable Arrangements of a National Gallery.

[No Abstract received.]

WEEKLY EVENING MEETING.

Friday, June 21, 1867.

Sir Henry Holland, Bart. M.D. D.C.L. F.R.S. President,
in the Chair.

John Tyndall, Esq. LL.D. F.R.S.
PROFESSOR OF NATURAL PHILOSOPHY, R.I.

On some Experiments of Faraday, Biot, and Savart.

The discourse was delivered at the request of the excellent president of the Royal Institution. The speaker had no new discovery to make known, and the utmost he could hope to achieve was to give a few old discoveries such a form as would interest an intellectual audience.

A few of the more striking phenomena of electro-magnetism were first exhibited by means of a helix and a core of soft iron. The question arose, "suppose that core to be transparent, what would be the effect of its magnetization upon a beam of light passing through it?" Probably such a question presented itself to the mind of Faraday. But iron was not transparent, and our great experimentalist had to seek long before he found a transparent substance, which enabled him to demonstrate the action of magnetism upon light.

Light in its natural condition was not sensibly affected by magnetism. The speaker then defined and illustrated, by means of a Foucault's prism and a plate of tourmaline, the action of "plane polarized light." He then showed the chromatic phenomena produced when a plate of rock-crystal, cut at right angles to the axis, was placed between the polarizer and analyzer of a polariscope. He also defined and illustrated, by means of the tourmaline, what was meant by the plane of vibration and the plane of polarization.

A plate of quartz, composed of two semicircles, the one belonging to a right-handed and the other to a left-handed crystal, was placed in front of the electric lamp, and through the plate was sent a beam of plane polarized light. The beam then passed through two perforated masses of iron, which rested on the two ends of a powerful electro-magnet—these pieces of iron were in fact the movable poles of the magnet. Beyond the furthest pole was placed a Foucault's prism, through which the beam also passed, being finally received upon a white screen. A lens was introduced between the circular plate of quartz and the magnet; and by this lens a magnified image of the plate of quartz was thrown upon the screen.

The speaker showed the changes of colour produced when the plane of polarization was caused to rotate. Bringing, for example, the entire image of the quartz plate to a delicate puce, the slightest rotation of the Foucault's prism coloured one of the semicircles a vivid red and the other a vivid green. Restoring the puce colour, and placing a bar of the heavy glass with which Faraday first demonstrated the action of magnetism upon light from pole to pole of the magnet, the beam was transmitted by the glass, and the image upon the screen was unchanged.

On now exciting the magnet, the uniformity of the colour disappeared; one semicircle ran rapidly into a vivid red, the other into a vivid green. The relative position of the colours changed when the direction of the current was changed: when the current was interrupted, the puce colour was restored. Thus it was proved that the act of magnetization produced the same effect as the mechanical rotation of the plane of polarization; and this is the celebrated experiment which Faraday described as the magnetization of a ray of light.

The beautiful experiment of Biot on the influence of sonorous vibrations on plane polarized light was next thrown into a form which allowed the whole audience to see the effect. A rectangle of glass, 6 feet long, 2 inches wide, and about ¼ of an inch thick, was clasped by a clamp at its centre, being so placed between the polarizer and analyzer that the beam crossed the glass rectangle near its centre. The polarizing prisms were placed so as to darken the field of view. A sweep of a wet cloth over the distant half of the glass rectangle brought out its tone, and immediately a luminous disk a yard in diameter flashed out upon the screen. Every sweep of the cloth threw the glass into sonorous vibration and illuminated the screen.

A plate of selenite was so placed between the polarizer and analyzer as to show a system of vividly coloured rings. By a suitable arrangement of the experiment, the colours were wholly obliterated when the glass rectangle was thrown into longitudinal vibration.

None of these effects could be produced when the polarized beam passed through the rectangle near one of its ends; for here, as is well known, the necessary strains and pressures were absent.

The remaining experiments had reference to the action of sonorous vibrations upon jets of water. A vein was discharged obliquely from the nipple of an ordinary gas burner. The vein broke into scattered drops. By the light of the electric lamp, a dense shadow of the vein was thrown upon a white screen; on sounding an organ pipe, or a tuning-fork of the proper pitch, the drops suddenly gathered themselves together, forming an apparently continuous band several feet in length. On the suspension of the sound the drops broke asunder as before. The minuteness of the vibration, which is competent to produce this effect upon the vein, is extraordinary. After a tuning-fork had ceased to be heard, when placed against the support of the nipple from which the vein issued, the drops gathered themselves together, and remained in coalescence long subsequent to the apparent subsidence of the motion.

A jet of water was permitted to descend vertically. Its two portions, the continuous and the discontinuous, were described. An arrangement was devised by which the vein was vividly illuminated from above. The continuous portion was of dazzling brilliancy; the point of rupture being thus rendered strikingly manifest. On sounding the proper note, the continuous vein shrunk almost up to its aperture. The effect of beats was very fine; as they addressed the ear, the lengthening and shortening of the luminous cylinder, in perfect synchronism with the beats, went on. Here also the amount of motion, if only of the proper quality, which influences the vein, may be infinitesimal: the vein, in fact, declares the existence of the beats long after the ear has ceased to hear them.

[J. T.]

GENERAL MONTHLY MEETING,

Monday, July 1, 1867.

WILLIAM POLE, Esq. M.A. F.R.S. in the Chair.

John Andrew Baumbach, Esq.
Louis J. Crossley, and
Joseph Ince, Esq. F.L.S.

were *elected* Members of the Royal Institution.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

Architects, Royal Institute of British—Paris Exhibition, 1867: Catalogue. 4to.
Acland, T. D. Esq. M.P. (the Author)—The Encouragement of Mathematics at Oxford considered. (K 94) 8vo. 1867.
American Academy of Natural Sciences—Proceedings for 1866. 8vo.
Bakewell, F. C. Esq. (the Author)—A Dynamical Theory of the Figure of the Earth. (K 94) 8vo. 1867.
Bavarian Academy of Science, Royal—Sitzungsberichte, 1867. Band I. 1–3. 8vo.
Brooke, Charles, Esq. F.R.S. M.R.I (the Author)—Elements of Natural Philosophy: based on the Treatise by Golding Bird. 6th ed. 16to. 1867.
Remarks on the Nature of Energy. (From "Elements of Nat. Phil." 1867.)
Chemical Society—Journal for June, 1867. 8vo.
Denison, Edmund B. Esq. F.R.S. M.R.I. (the Author)—Astronomy without Mathematics. 4th ed. 16to. 1867.

Editors—American Journal of Science and Arts. May, 1867. 8vo.
Artizan for June, 1867. 4to.
Athenæum for June, 1867. 4to.
British Journal of Photography for June, 1867. 4to.
Chemical News for June, 1867. 4to.
Engineer for June, 1867. fol.
Horological Journal for June, 1867. 8vo.
Journal of Gas-Lighting for June, 1867. 4to.
Mechanics' Magazine for June, 1867. 8vo.
Pharmaceutical Journal for June, 1867.
Elliot, John Lettsom, Esq. M.R.I. Easie Rules on Earlie Rising, by a Late Philosopher. Illustrated by Lady Frances Bushby. 4to. 1866.
Franklin Institute—Journal, No. 497. 8vo. 1867.
Gladstone, John Hall, Esq. Ph. D. F.R.S. M.R.I. (the Author)—Theology and Natural Science; their Mutual Relations. (K 94) 8vo. 1867.
Madras Literary Society—Journal, 1856-66. 7 vols. 8vo.
Marcet, W., M.D. F.R.S. (the Author)—On a New Process for Preparing Meat for Weak Stomachs. (K 94) 8vo. 1867.
Packe, Edmund, Esq. M.R.I.—Guide to the Pyrenees, by Charles Packe. 2nd ed. 16to. 1867.
Photographic Society—Journal, No. 182. 8vo. 1867.
Roma, Accademia dei Nuovi Lincei—Atti. Anno XIX. 4to. 1866.
Royal Society of London—Proceedings, No. 93. 8vo. 1867.
St. Petersburg Académie des Sciences—Mémoires. 7^{e} Série. Tome X. Nos. 3-15. 4to. 1866.
Bulletins. Tome X. et Tome XI. Nos. 1, 2. 4to. 1866.
United Service Institution, Royal—Journal, No. 42. 8vo. 1867.

Faraday, Professor—Three Bars of Heavy Glass made by him, and employed in his Experimental Demonstration of the Magnetization of a Ray of Light.

Royal Institution of Great Britain.

ROYAL COLLEGE OF SURGEONS OF ENGLAND

1867.

GENERAL MONTHLY MEETING,

Monday, November 4, 1867.

Sir Henry Holland, Bart. M.D. D.C.L. F.R.S. President,
in the Chair.

Thomas Anthony Denny, Esq.
The Honourable John C. Erskine,
Captain Alexander McNeile,
The Honourable Thomas J. Wynn,

were *elected* Members of the Royal Institution.

The Hon. Secretary having announced the decease of **Professor Faraday**, on the 25th of last August:—

"**Resolved**, That the Members of the Royal Institution sympathize most deeply with Mrs. Faraday in the loss of their Professor and Friend.

"His energy and genius were rewarded by discoveries that have made their Institution renowned throughout the world; whilst his judgment and kindness were so frequently and so well shown in all that related to the good of the Members, that they feel his departure from among them is a misfortune which no words can adequately express."

The Secretary reported that **Professor Faraday** had bequeathed various MSS. and Books to the Royal Institution in the following terms:—

"Various Philosophical Notes of experimental investigation on foolscap paper, paged in series, and partly bound in five volumes; a quarto MS. book of Philosophical Notes; a second larger quarto of similar notes; some of my Printed Papers, collected in two bound volumes and illustrated by letters, &c. (the one 4to, the other 8vo); and a bound copy of Davy's 'Chemical Elements,' being a copy of that which, whilst abroad in 1814–5, he prepared for a second edition (which was never published);—these I offer for the Library of the Royal Institution, if the Managers should think them worth a place; if not, to remain at the disposal of my executors. . . . . .

"16 *Jan.* 1855. M. Faraday."

1. Professor Faraday's Philosophical Notes, MS. (Sept. 1820–14 Oct. 1832.) 2 vols. 4to.
2. His Experimental Notes, MS. (2 Feb. 1831–12 March, 1862.) 8 vols. fol.
3. His Papers, printed in the Philosophical Transactions of the Royal Society, with MS. and other insertions. 1821–31. 1 vol. 4to.

4. His Papers, printed in the "Quarterly Journal of Science" and the "Philosophical Magazine" (with MS. insertions). 1817-32. 8vo.
5. Sir Humphry Davy's "Elements of Chemical Philosophy" (with MS. insertions). 8vo. 1812.

The following MS. and Books were presented by **Mrs. Faraday**:—

1. Professor Faraday's MS. Memoranda relating to his Lectures and Friday Evening Discourses (1825-37). 4 vols. 4to.
2. His MS. Lecture Notes, viz.:—
    After Easter Lectures, 1831-53. (25 thin books.)
    Christmas Juvenile Lectures, 1841-61. (9 thin books.)
    Friday Evening Discourses, 1837-62. (Separate MS.)
3. His Experimental Researches in Electricity. Series I.-XXX. (Papers extracted from the Philosophical Transactions of the Royal Society); and Papers inserted in the "Proceedings of the Royal Institution" and the "Philosophical Magazine." With MS. and other insertions. 1831-57. 5 vols. 4to. (*The continuation of Nos.* 3 *and* 4 *of Professor Faraday's bequest.*)
4. General Index to "Experimental Researches." Prepared by himself. 8vo.
5. Life of Sir Humphry Davy, by Dr. J. A. Paris (with MS. Letters of Sir H. Davy inserted). 4to. 1831.

Connected with the Royal Institution.

Memoranda of Friday Evening Meetings. 1826-36. 1 vol. 4to.
"Helps,"—Memoranda respecting the House, Servants, &c. 2 vols. 4to and 16to.

Printed Books.

R. Boyle, Experiments and Notes on the Prodvctiveness of Chymicall Principles. 16to. 1680.
M. J. Brisson, Elements of the Natural History of Mineral Substances. 8vo. 1800.
J. Lyon, Experiments and Observations on Electricity, with MS. Notes. 4to. 1780.
Bound by Professor Faraday.
J. French, Art of Distillation. 4to. 1650.
Philosophical Conferences of the French Virtuosi. fol. 1665.
H. Power, Experimental Philosophy. 4to. 1664.
R. Hooke, Microscopic Observations. New ed. fol. 1780.
M. Frezier, Traité des Feux d'Artifice. 8vo. 1747.
P. F. Baddeley, on Dust-Storms of India. 4to. 1852-60.
Spalanzini, Nouveaux Recherches sur les Découvertes Microscopiques; avec Notes par Needham. 8vo. 1769.
Sir H. Davy, On the Safety-Lamps for Coal-Mines, with some Researches on Flame. 8vo. 1818.
Philosophical and Miscellaneous Tracts, arranged by Professor Faraday. 10 vols. 8vo. 1803-34.
Thames Tunnel. Plates, with Descriptions. fol. 1824.
Notices sur Phares et Fanaux Lenticulaires. 4to. 1858.

"Resolved, That the Honorary Secretary be desired to give to Mrs. Faraday the Special Thanks of the President, Managers, and Members for the Present of Books, the Memorials of Professor Faraday. The Donation which he made them of MS. Notes, they value in the highest degree, and they will give directions that they shall be preserved in safety, apart from the general Library, and be always open to Mrs. Faraday, or to any of the Members of the family who may desire to see them."

The Special Thanks of the Members were returned to George Warington, Esq. for his Present of "Mémoires de Physique et de Chimie de la Société d'Arcueil.' 3 vols. 8vo. 1807–17. (*Vols. I. and II. were bound by Professor Faraday.*)

The following Presents were laid on the table, and the thanks of the Members returned for the same:—

Académie Impériale des Sciences de St. Petersburg—Mémoires. Tome X. No. 16. Tome XI. Nos. 1–8. 4to. 1867.
Bulletins. Tome XI. Nos. 3, 4. Tome XII. No. 1. 4to. 1866–7.
Académie Royale de Belgique—Bulletins. Tomes XXII.–XXIII. 8vo. 1866–7.
Annuaire. 1867. 16to.
Actuaries, Institute of—Journal, Nos. 68, 69. 8vo. 1867.
Agricultural Society of England, Royal—Journal. New Series. No. 6. 8vo. 1867.
American Academy of Arts and Sciences—Proceedings. Vol. VII. Nos. 13–23. 8vo. 1866.
American Institute, New York—Annual Report, 1865–66. 8vo. 1866.
American Philosophical Society—Proceedings, No. 76. 8vo. 1861.
Asiatic Society of Bengal—Proceedings, 1866, Title, &c.; and 1867, Nos. 2–7. 8vo.
Journal, Nos. 136, 138, 139. 8vo. 1867.
Astronomical Society, Royal—Monthly Notices, Vol. XXVII. Nos. 8, 9. 8vo. 1867.
Author—Present State of the Practice of Physic. (K 94) 8vo. 1867.
Bavarian Academy of Science, Royal—Sitzungsberichte, 1867. Band I. Heft 4. Band II. Heft 1. 8vo.
Abhandlungen. Band X. Abth. 1. 4to. 1866.
Bayley, Francis, Esq. M.R.I.—Archæologia Cantiana; being Transactions of the Kent Archæological Society. Vols. I.–VI. 8vo. 1858–66.
Beamish, Richard, Esq. F.R.S. M.R.I. (the Author)—The Psychonomy of the Hand; or, the Hand an Index of Mental Development. 2nd ed. 1865. 4to.
Bigsby, J. J. M.D. M.R.I. (the Author)—Brief Account of the Thesaurus Siluricus. (Proc. R. S. Vol. XV.) 8vo. 1867.
British Association for the Advancement of Science—Report of the Thirty-Sixth Meeting at Nottingham, Sept. 1866. 8vo. 1867.
Boston Society of Natural History, U.S.—Memoirs. Vol. I. Parts 1, 2. 4to. 1866–7. Proceedings. Vol. X. Nos. 19–27. Vol. XI. Nos. 1–6. 8vo. 1866–7.
Condition and Doings. 8vo. 1866.
Chemical Society—Journal for July to Oct. 1867. 8vo.
Connecticut Academy of Arts and Sciences—Transactions. Vol. I. Part 1. 8vo. 1866.
Cornwall Polytechnic Society, Royal—Thirty-fourth Annual Report. 8vo. 1866.
Devonshire Association for the Advancement of Science—Report and Transactions. Vol. II. Part 1. 8vo. 1867.
Editors—American Journal of Science and Arts. July and Sept. 1867. 8vo.
Artizan for July to Oct. 1867. 4to.
Athenæum for July to Oct. 1867. 4to.
British Journal of Photography for July to Oct. 1867. 4to.
Chemical News for July to Oct. 1867. 4to.
Engineer for July to Oct. 1867. fol.
Geological and Natural History Repository. Aug.–Oct. 1867. 8vo.
Horological Journal for July to Oct. 1867. 8vo.
Journal of Gas-Lighting for July to Oct. 1867. 4to.
Mechanics' Magazine for July to Oct. 1867. 8vo.
Pharmaceutical Journal for July to Oct. 1867.
Photographic News for July to Oct. 1867. 4to.
Practical Mechanics' Journal for July to Oct. 1867. 4to.
Revue des Cours Scientifiques et Littéraires. Juillet–Oct. 1867.

Elliot, Lady—History of India, as told by its own Historians. Edited from the Posthumous Papers of the late Sir H. M. Elliot, by Professor John Dawson. Vol. I. 8vo. 1867.

Essex Institute, U.S.—Proceedings. Vol. IV. and Vol. V. Nos. 1, 2. 1864–7.

Faraday, Professor, D.C.L. F.R.S. M.R.I.—Oversigt over det kon. Danske Videnskabernes. Selskabs: 1865. Nos. 1–4. 1866. Nos. 1–6. 1867. Nos. 1–3. 8vo.

R. Accademia delle Scienze di Torino:—Memoria. 2ª Serie. Tomo XXII. 4to. 1865. Atti. Vol. I. Disp. 3–7. Vol. II. Disp. 1–3. 8vo. 1866–7.

Natuurkundige Verhandelingen. Tome XXIV. 1–3. Tome XXV. 1. 4to. 1866.

Archives Néerlandaises des Sciences. Tome I. Liv. 5. Tome II. Liv. 1, 2. 8vo. 1866–7.

Franklin Institute—Journal, Nos. 498, 499, 500, 501. 8vo. 1867.

Geneva—Société de Physique, Mémoires. Tome XIX. 1re Partie. 4to. 1867.

Geographical Society, Royal—Proceedings, Vol. XI. Nos. 3, 4, 5. 8vo. 1867.

Geological Institute, Imperial, Vienna—Jahrbuch, 1867. Nos. 1, 2. 8vo. 1867.

Geological Society—Quarterly Journal, No. 91. 8vo. 1867.

Journal, Vol. XXXVI. 8vo. 1867.

Geological Society of Ireland, Royal—Journal. Vol. I. Part 3. 8vo. 1867.

Georgifili, Reale Accademia de'—Atti. Vol. XIV. Disp. 1, 2. 8vo. 1867.

Horticultural Society, Royal—Proceedings, No. 8. 8vo. 1867.

Hull Royal Institution—Annual Report, 1867. 8vo. 1867.

Leeds Literary and Philosophical Society—Annual Report, 1866–67. 8vo. 1867.

Levi, Leone, Esq. F.S.S. F.S.A. (the Author)—Wages and Earnings of the Working Classes. 8vo. 1867.

Linnean Society—Transactions, Vol. XXV. Part 3. 4to. 1867.

Index to Transactions. Vol. I.–XXV. 4to. 1867.

Journal, Nos. 36, 40, 41. 8vo. 1867.

Marriott, Montague, Esq. M.R.I. (the Editor)—Willich's Popular Tables. 6th ed. 16to. 1867.

Mechanical Engineers' Institution, Birmingham—Proceedings, August, Part 4 Nov. 1866. 8vo.

Medical and Chirurgical Society, Royal—Proceedings. Vol. V. No. 8. 8vo. 1867.

Meteorological Society—Proceedings, No. 32. 8vo. 1867.

Moore, C. H. Esq. M.R.I. (the Author)—Rodent Cancer. 16to. 1867.

Murchison, Sir Roderick I. Bart. K.C.B. M.R.I. (the Author)—Address to the Royal Geographical Society, 27 May, 1867. 8vo.

Siluria. 4th ed. 8vo. 1867.

Photographic Society—Journal, Nos. 183–186. 8vo. 1866.

Reeves, C. E. M.D. (the Author)—Softening of the Stomach in Children in Australia. (K 94) 8vo. 1867.

Royal College of Surgeons—Calendar, &c. 8vo. 1867.

Royal Society of London—Proceedings, Nos. 94, 95. 8vo. 1867.

Philosophical Transactions, 1867. Vol. CLVII. Part 1. 4to. 1867.

Royal Society of Tasmania—Results of Twenty-five Years' Meteorological Observations for Hobart Town, &c. by Francis Abbott. 4to. 1866.

Sargent, Fred. Esq. (the Author)—Compendium of Biblical Criticism. 8vo. 1865.

Smithsonian Institution, U.S.—Annual Report, 1865. 8vo. 1866.

Smithsonian Miscellaneous Collections. Vols. VI. and VII. 8vo. 1867.

St. Bartholomew's Hospital, Treasurer—St. Bartholomew's Hospital Reports. Vol. III. 8vo. 1867.

Statistical Society of London—Journal, Vol. XXX. Parts 2, 3. 8vo. 1867.

Surgeon-General War Department, U.S.—Report on Epidemic Cholera. 4to. 1867.

Symons, G. J. Esq. (the Author)—Symons' Monthly Meteorological Magazine, July to Oct. 1867. 8vo.

Teyler Foundation, Haarlem—Archives du Musée Teyler. Vol. I. Fasc. 2. 8vo. 1867.

Tyndall, Professor, LL.D. F.R.S. M.R.I. (the Author)—Sound: a Course of Eight Lectures, delivered at the Royal Institution of Great Britain. 8vo. 1867.

United Service Institution, Royal—Journal, Nos. 43, 44. 8vo. 1867.

United States Naval Observatory—Astronomical Observations in 1851 and 1852. 4to. 1867.

University College, London—Calendar. 1867–8. 8vo.

Vereins zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, Jan. Feb. 1866. 4to.

Warington, George, Esq.—Mémoires de Physique et de Chimie de la Société d'Arcueil. 3 vols. 8vo. 1807–17. (*Vols. I. and II. were bound by Professor Faraday.*)

Zoological Society of London—Transactions, Vol. VI. Parts 1–3. 4to. 1866–7. Proceedings, 1866. 8vo.

GENERAL MONTHLY MEETING,

Monday, December 2, 1867.

Sir Henry Holland, Bart. M.D. D.C.L. F.R.S. President, in the Chair.

George Willoughby Hemans, Esq. M.I.C.E. F.R.G.S.
William Daniel Michell, Esq.
Morgan Bransby Williams, Esq. M.I.C.E.

were *elected* Members of the Royal Institution.

The following Lecture Arrangements for the ensuing Season were announced:—

Professor Tyndall, LL.D. F.R.S.—Six Lectures (*adapted to a Juvenile Auditory*), 'On Heat and Cold.' On December 26th, 28th, 31st, 1867; January 2nd, 4th, 7th, 1868.

Professor Tyndall, LL.D. F.R.S.—Nine Lectures, 'On the Discoveries of Faraday.' On Tuesdays and Thursdays: Thursday, Jan. 30th to Feb. 27th.

Professor Roscoe, F.R.S.—Eleven Lectures, 'On the Chemistry of the Non-Metallic Elements.' On Saturdays, January 25th to April 4th.

George Scharf, Esq. F.S.A.—Six Lectures, 'On Historical Portraiture of Various Times and Countries.' On Tuesdays and Thursdays, March 3rd to 19th.

Dr. Michael Foster.—Four Lectures, 'On the various Modes of the Developement of Animals.' On Tuesdays and Thursdays, March 24th to April 2nd.

After Easter.

Dr. Michael Foster.—Eight Lectures (in continuation), 'On the various Modes of the Developement of Animals.' On Tuesdays, April 21st to June 9th.

Professor Odling, F.R.S.—Four Lectures, 'On Chemical Combination.' On Thursdays and Saturdays, April 23rd to May 2nd.

Professor Bain.—Four Lectures, 'On Popular Errors.' On Thursdays and Saturdays, May 7th to 16th.

Professor ROBERT GRANT, LL.D. F.R.S.—Four Lectures, 'On Astronomy, viewed in connexion with the establishment of the Theory of Gravitation.' On Thursdays and Saturdays, May 21st to 30th.

Sir JOHN LUBBOCK, Bart. F.R.S.—Four Lectures, 'On Savages.' On Thursdays and Saturdays, June 4th to 13th.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

Chemical Society—Journal for Nov. 1867. 8vo.
Corporation of London—Catalogue of the Library. 7th supp. 8vo. 1867.
De la Rive, Professor A. (the Author)—Notice sur Michel Faraday. 8vo. 1867.
Dublin Society, Royal—Journal, No. 36. 8vo. 1867.
Editors—American Journal of Science and Arts. Nov. 1867. 8vo.
  Artizan for Nov. 1867. 4to.
  Athenæum for Nov. 1867. 4to.
  British Journal of Photography for Nov. 1867. 4to.
  Chemical News for Nov. 1867. 4to.
  Engineer for Nov. 1867. fol.
  Horological Journal for Nov. 1867. 8vo.
  Journal of Gas-Lighting for Nov. 1867. 4to.
  Mechanic's Magazine for Nov. 1867. 8vo.
  Pharmaceutical Journal for Nov. 1867.
  Photographic News for Nov. 1867. 4to.
  Practical Mechanic's Journal for Nov. 1867. 4to.
  Revue des Cours Scientifiques et Littéraires. Nov. 1867.
Franklin Institute—Journal, No. 502. 8vo. 1867.
Geological Institute, Imperial, Vienna—Jahrbuch, 1867. No. 3. 8vo. 1867.
Geological Society—Quarterly Journal, No. 92. 8vo. 1867.
Glasgow Philosophical Society—Proceedings. Vol. VI. No. 3. 1867.
Grove, W. R. Esq. Q.C. F.R.S. M.R.I. (the Author)—On Taxation of Permanent and Precarious Incomes. (K 95) 8vo. 1867.
Gyll, Gordon W. J. Esq. M.R.I. (the Translator)—Galatea: a Pastoral Romance by Miguel de Cervantes Saavedra. 8vo. 1867.
Lee, Robert J. M.B. M.R.I. (the Author)—Explanation of the Movements of the Iris. 8vo. 1867.
Linnean Society—Proceedings, 1866–7. 8vo.
  Journal, No. 37. 8vo. 1867.
Mechanical Engineers' Institution, Birmingham—Proceedings, Jan. 1867. 8vo.
Morris, James, M.D. (the Author)—Germinal Matter and the Contact Theory. 2nd ed. 16to. 1867.
Photographic Society—Journal, Nos. 186, 187. 8vo. 1867.
Society of Arts—Journal for Nov. 1867. 8vo.
Surgeon-General United States' Army—Catalogue of the Surgical Section of the U.S. Army. 4to. 1866.
Symons, G. J. Esq. (the Author)—Symons' Monthly Meteorological Magazine, Nov. 1867. 8vo.
United Service Institution, Royal—Journal, No. 45. 8vo. 1867.
Zoological Society of London—Transactions, Vol. VI. Part 4. 4to. 1867.
  Proceedings, Parts 1 and 2. 1867. 8vo.

1868.

WEEKLY EVENING MEETINGS,

Fridays, January 17 and 24, 1868.

Sir Henry Holland, Bart. M.D. D.C.L. F.R.S. President, in the Chair.

John Tyndall, Esq. LL.D. F.R.S.
PROFESSOR OF NATURAL PHILOSOPHY, ROYAL INSTITUTION.

On Faraday as a Discoverer.

Parentage : Introduction to the Royal Institution : Earliest Experiments : First Royal Society Paper : Marriage.

It has been thought desirable to give you and the world some image of MICHAEL FARADAY, as a scientific investigator and discoverer. The attempt to respond to this desire has been to me a labour of difficulty, if also a labour of love. For however well acquainted I may be with the researches and discoveries of that great master,—however numerous the illustrations which occur to me of the loftiness of Faraday's character and the beauty of his life,—still to grasp him and his researches as a whole; to seize upon the ideas which guided him, and connected them; to gain entrance into that strong and active brain, and read from it the riddle of the world—this is a work not easy of performance, and all but impossible amid the distraction of duties of another kind. That I should at one period or another speak to you regarding Faraday and his work, is natural, if not inevitable; but I did not expect to be called upon to speak so soon. Still the bare suggestion that this is the fit and proper time for speech sent me immediately to my task: from it I have returned with such results as I could gather, and also with the wish that those results were more worthy than they are of the greatness of my theme.

It is not my intention to lay before you a *life* of Faraday in the ordinary acceptation of the term. The duty I have to perform is to give you some notion of what he has done in the world; dwelling incidentally on the spirit in which his work was executed, and introducing such personal traits as may be necessary to the completion of your picture of the *philosopher*, though by no means adequate to give you a complete idea of the *man*.

The newspapers have already informed you that Michael Faraday was born at Newington Butts, on the 22nd of September, 1791, and that he fell finally asleep at Hampton Court, on the 25th of August, 1867. Believing, as I do, in the general truth of the doctrine of hereditary transmission—sharing the opinion of Mr. Carlyle, that "a really able man never proceeded from entirely stupid parents"—I once used the privilege of my intimacy with Mr. Faraday to ask him whether his parents showed any signs of unusual ability. He could remember none. His father, I believe, was a great sufferer during the later years of his life, and this might have masked whatever intellectual power he possessed. When thirteen years old, that is to say in 1804, Faraday was apprenticed to a bookseller and bookbinder in Blandford-street, Manchester-square: here he spent eight years of his life, after which he worked as a journeyman elsewhere.

You have also heard the account of Faraday's first contact with the Royal Institution: that he was introduced by one of the members to Sir Humphry Davy's last lectures; that he took notes of those lectures, wrote them fairly out, and sent them to Davy, entreating him at the same time to enable him to quit trade, which he detested, and to pursue science, which he loved. Davy was helpful to the young man, and this should never be forgotten: he at once wrote to Faraday, and afterwards, when an opportunity occurred, made him his assistant.* Mr. Gassiot has lately favoured me with the following reminiscence of this time:—

"CLAPHAM COMMON, SURREY,
"28*th November*, 1867.

"MY DEAR TYNDALL,

"Sir H. Davy was accustomed to call on the late Mr. Pepys in the Poultry on his way to the London Institution, of which Pepys was one of the original managers; the latter told me that on one occasion, Sir H. Davy, showing him a letter said, 'Pepys, what am I *to do*, here is a letter from a young man named Faraday; he has been attending my lectures and wants me to give him employment at the Royal Institution, *what can I do?*' '*Do*,' replied Pepys, 'put him to wash bottles; if he is good for anything, he will do it directly; if he refuses, he is good for nothing.' 'No, no,' replied Davy; 'we must try him with something better than that.' The result was, that Davy engaged him to assist in the Laboratory at *weekly* wages.

"Davy held the joint office of Professor of Chemistry, and Director of the Laboratory; he ultimately gave up the former to the late Professor Brande, but he insisted that Faraday should be appointed Director of the Laboratory, and, as Faraday told me, this enabled him on subsequent occasions to hold a definite position in the Institution, in which he was always supported by Davy. I believe he held that office to the last.

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* Here is Davy's recommendation of Faraday, presented to the managers of the Royal Institution, at a meeting on the 18th of March, 1813, Charles Hatchett, Esq., in the chair:—

"Sir Humphry Davy has the honour to inform the managers that he has found a person who is desirous to occupy the situation in the Institution lately filled by William Payne. His name is Michael Faraday. He is a youth of twenty-two years of age. As far as Sir H. Davy has been able to observe or ascertain, he appears well fitted for the situation. His habits seem good; his disposition active and cheerful, and his manner intelligent. He is willing to engage himself on the same terms as given to Mr. Payne at the time of quitting the Institution.

"*Resolved*,—That Michael Faraday be engaged to fill the situation lately occupied by Mr. Payne, on the same terms."

"Believe me, my dear Tyndall, yours truly,

"Dr. Tyndall." "J. P. Gassiot.

From a letter written by Faraday himself soon after his appointment as Davy's assistant, I extract the following account of his introduction to the Royal Institution:—

"London, *Sept.* 13*th*, 1813.

"As for myself I am absent (from home) nearly day and night except occasional calls, and it is likely shall shortly be absent entirely, but this (having nothing more to say and at the request of my mother) I will explain to you. I was formerly a bookseller and binder, but am now turned philosopher,* which happened thus:—Whilst an apprentice, I, for amusement, learnt a little chemistry and other parts of philosophy, and felt an eager desire to proceed in that way further. After being a journeyman for six months under a disagreeable master, I gave up my business, and through the interest of a Sir H. Davy, filled the situation of chemical assistant to the Royal Institution of Great Britain, in which office I now remain; and where I am constantly employed in observing the works of nature, and tracing the manner in which she directs the order and arrangement of the world. I have lately had proposals made to me by Sir Humphry Davy, to accompany him in his travels through Europe and Asia as philosophical assistant. If I go at all I expect it will be in October next—about the end, and my absence from home will perhaps be as long as three years. But as yet all is uncertain."

This account is supplemented by the following letter, written by Faraday to his friend De la Rive,† on the occasion of the death of Mrs. Marcet. The letter is dated 2nd Sept., 1858:—

"My dear Friend,

"Your subject interested me deeply every way; for Mrs. Marcet was a good friend to me, as she must have been to many of the human race. I entered the shop of a bookseller and bookbinder at the age of 13, in the year 1804, remained there eight years, and during the chief part of the time bound books. Now it was in those books, in the hours after work, that I found the beginning of my philosophy. There were two that especially helped me, the 'Encyclopædia Britannica,' from which I gained my first notions of electricity, and Mrs. Marcet's 'Conversations on Chemistry,' which gave me my foundation in that science. . . . .

"Do not suppose that I was a very deep thinker, or was marked as a precocious person. I was a very lively, imaginative person, and could believe in the 'Arabian Nights' as easily as in the 'Encyclopædia.' But facts were important to me, and saved me. I could trust a fact, and always cross-examined an assertion. So when I questioned Mrs. Marcet's book by such little experiments as I could find means to perform, and found it true to the facts as I could understand them, I felt that I had got hold of an anchor in chemical knowledge, and clung fast to it. Thence my deep veneration for Mrs. Marcet—first, as one who had conferred great personal good and pleasure on me; and then as one able to convey the truth and principle of those boundless fields of knowledge which concern natural things, to the young, untaught, and inquiring mind.

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* Faraday loved this word and employed it to the last; he had an intense dislike to the modern term *physicist*.

† To whom I am indebted for a copy of the original letter.

"You may imagine my delight when I came to know Mrs. Marcet personally; how often I cast my thoughts backward, delighting to connect the past and the present; how often, when sending a paper to her as a thank-offering, I thought of my first instructress, and such like thoughts will remain with me.

"I have some such thoughts even as regards *your own father;* who was, I may say, the first who personally at Geneva, and afterwards by correspondence, encouraged, and by that sustained me."

Twelve or thirteen years ago Mr. Faraday and myself quitted the Institution one evening together, to pay a visit in Baker-street. He took my arm at the door, and, pressing it to his side in his warm genial way, said, "Come, Tyndall, I will now show you something that will interest you." We walked northwards, passed the house of Mr. Babbage, which drew forth a reference to the famous evening parties once assembled there. We reached Blandford-street, and after a little looking about, he paused before a stationer's shop, and then went in. On entering the shop, his usual animation seemed doubled; he looked rapidly at everything it contained. To the left on entering was a door, through which he looked down into a little room, with a window in front facing Blandford-street. Drawing me towards him, he said eagerly, "Look there, Tyndall; that was my working-place. I bound books in that little nook." A respectable-looking woman stood behind the counter: his conversation with me was too low to be heard by her, and he now turned to the counter to buy some cards as an excuse for our being there. He asked the woman her name—her predecessor's name—his predecessor's name. "That won't do," he said, with good-humoured impatience, who was *his* predecessor?" "Mr. Riebau," she replied, and immediately added, as if suddenly recollecting herself, "He, sir, was the master of Sir Charles Faraday." "Nonsense!" he responded, "there is no such person." Great was her delight when I told her the name of her visitor; but she assured me that as soon as she saw him running about the shop, she felt—though she did not know why—that it must be "Sir Charles Faraday."

Faraday did, as you know, accompany Davy to Rome; he was re-engaged by the managers of the Royal Institution on the 15th of May, 1815. Here he made rapid progress in chemistry, and after a time was entrusted with easy analyses by Davy. In those days the Royal Institution published 'The Quarterly Journal of Science,' the precursor of our own 'Proceedings.' Faraday's first contribution to science appeared in that journal in 1816. It was an analysis of some caustic lime from Tuscany, which had been sent to Davy by the Duchess of Montrose. Between this period and 1818 various notes and papers were published by Faraday. In 1818 he experimented upon "Sounding Flames." Professor Auguste De la Rive, father of our present excellent De la Rive, had investigated those sounding flames, and had applied to them an explanation which completely accounted for a class of sounds discovered by De la Rive himself. By a few simple and conclusive experiments Faraday proved that the explanation was insufficient. It is an epoch in the life of a young man when he finds himself correcting a person of eminence, and in Faraday's case, where its effect was to develop a modest self-trust, such an event could not fail to act profitably.

From time to time between 1818 and 1820 Faraday published scientific notes and notices of minor weight. At this time he was acquiring, not producing; working hard for his master and storing and strengthening his own mind. He assisted Mr. Brande in his lectures, and so quietly, skilfully, and modestly was his work done, that Mr. Brande's vocation at the time was pronounced "lecturing on velvet." In 1820 Faraday published a chemical paper "on two new compounds of chlorine and carbon, and on a new compound of iodine, carbon, and hydrogen." This paper was read before the Royal Society on the 21st of Dec. 1820, and it was the first of his that was honoured with a place in the 'Philosophical Transactions.'

On the 12th of June, 1821, he married, and obtained leave to bring his young wife into his rooms at the Royal Institution. There for forty-six years they lived together, occupying the suite of apartments which had been previously in the successive occupancy of Young, Davy, and Brande. At the time of her marriage Mrs. Faraday was twenty-one years of age, he being nearly thirty. Regarding this marriage I will at present limit myself to quoting an entry written in Faraday's own hand in his book of diplomas, which caught my eye while in his company some years ago. It ran thus:—

> "*25th January*, 1847.
>
> "Amongst these records and events, I here insert the date of one which, as a source of honour and happiness, far exceeds all the rest. We were *married* on the 12th of June, 1821.
>
> "M. Faraday."

Then follows the copy of the minutes, dated 21st May, 1821, which gave him additional rooms, and thus enabled him to bring his wife to the Royal Institution. A feature of Faraday's character which I have often noticed makes itself apparent in this entry. In his relations to his wife he added *chivalry* to affection.

Early Researches: Magnetic Rotations: Liquefaction of Gases: Heavy Glass: Charles Anderson: Contributions to Physics.

Oersted, in 1820, discovered the action of a voltaic current on a magnetic needle; and immediately afterwards the splendid intellect of Ampère succeeded in showing that every magnetic phenomenon then known might be reduced to the mutual action of electric currents. The subject occupied all men's thoughts; and in this country Dr. Wollaston sought to convert the deflection of the needle by the current into a permanent *rotation* of the needle round the current. He also hoped to produce the reciprocal effect of causing a current to rotate round a magnet. In the early part of 1821 Wollaston attempted to realize this idea in the presence of Sir Humphry Davy in the laboratory of the Royal Institution. This was well calculated to attract Faraday's attention to the subject. He read much about it; and in the months of July, August, and September he wrote "a history of the progress of electro-magnetism," which he published in Thomson's 'Annals of Philosophy.' Soon afterwards he took up the subject of "Magnetic Rotations," and on the morning of Christmas day, 1821, he called his wife to witness for the first time the revolution of a magnetic needle round an electric current. Incidental to the "historic sketch" he repeated almost all the experiments there referred to; and these, added to his own subsequent work, made him practical master of all that was then known regarding the voltaic current. In 1821 he also touched upon a subject which subsequently received his closer attention—the vaporization of mercury at common temperatures; and immediately afterwards conducted, in company with Mr. Stodart, experiments on the alloys of steel. He was accustomed in after years to present to his friends razors formed from one of the alloys then discovered.

During Faraday's hours of liberty from other duties he took up subjects of inquiry for himself; and in the spring of 1823, thus self-prompted, he began the examination of a substance which had long been regarded as the chemical element chlorine, in a solid form, but which Sir Humphry Davy, in 1810, had proved to be a hydrate of chlorine, that is, a compound of chlorine and water. Faraday first analyzed this hydrate, and wrote out an account of its composition. This account was looked over by Davy, who suggested the heating of the hydrate under pressure in a sealed glass tube. This was done. The hydrate fused at a blood-heat, the tube became filled with a yellow atmosphere and was found to contain two liquid substances. Dr. Paris happened to enter the laboratory while Faraday was at work. Seeing the oily liquid in his tube he rallied the young chemist for his carelessness in employing soiled vessels. On filing off the end of the tube its contents exploded and the oily matter vanished. Early next morning Dr. Paris received the following note:—

> "DEAR SIR,
>
> "The *oil* you noticed yesterday turns out to be liquid chlorine.
>
> "Yours faithfully,
>
> "M. FARADAY." *

The gas had been liquefied by its own pressure. Faraday then tried compression with a syringe, and succeeded thus in liquefying the gas.

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* Paris: 'Life of Davy,' p. 391.

To the published account of this experiment Davy added the following note:—"In desiring Mr. Faraday to expose the hydrate of chlorine in a closed glass tube, it occurred to me that one of three things would happen: that it would become fluid as a hydrate; that decomposition of water would occur; . . . or that the chlorine would separate in a fluid state." Davy, moreover, immediately applied the method of self-compressing atmospheres to the liquefaction of muriatic gas. Faraday continued the experiments and succeeded in reducing a number of gases till then deemed permanent to the liquid condition. In 1844 he returned to the subject, and considerably expanded its limits. These important investigations established the fact that gases are but the vapours of liquids possessing a very low boiling-point, and gave a sure basis to our views of molecular aggregation. The account of the first investigation was read before the Royal Society on the 10th of April, 1823, and was published, in Faraday's name, in the 'Philosophical Transactions.' The second memoir was sent to the Royal Society on the 19th of December, 1844. I may add that while he was conducting his first experiments on the liquefaction of gases, thirteen pieces of glass were on one occasion driven by an explosion into Faraday's eye.

Some small notices and papers, including the observation that glass readily changes colour in sunlight, follow here. In 1825 and 1826 Faraday published papers in the 'Philosophical Transactions' on "new compounds of carbon and hydrogen," and on "sulphonaphthalic acid." In the former of these papers he announced the discovery of Benzol, which, in the hands of modern chemists, has become the foundation of our splendid aniline dyes. But he swerved incessantly from chemistry into physics; and in 1826 we find him engaged in investigating the limits of vaporization, and showing, by exceedingly strong and apparently conclusive arguments, that even in the case of mercury such a limit exists; much more he conceived it to be certain that our atmosphere does not contain the vapour of the fixed constituents of the earth's crust. This question, I may say, is likely to remain an open one. Mr. Rankine, for example, has lately drawn attention to the odour of certain metals; whence comes this odour, if it be not from the vapour of the metal?

In 1825 Faraday became a member of a committee, to which Sir John Herschel and Mr. Dollond also belonged, appointed by the Royal Society to examine, and if possible improve, the manufacture of glass for optical purposes. Their experiments continued till 1829, when the account of them constituted the subject of a "Bakerian Lecture." This lectureship, founded in 1774 by Henry Baker, Esq., of the Strand, London, provides that every year a lecture shall be given before the Royal Society, the sum of four pounds being paid to the lecturer. The Bakerian Lecture, however, has long since passed from the region of pay to that of honour, papers of mark only being chosen for it by the council of the Society. Faraday's first Bakerian Lecture, "On the Manufacture of Glass for Optical Purposes," was delivered at the close of 1829. It is a most elaborate and conscientious description of processes, precautions, and results: the details were so exact and so minute, and the paper consequently so long, that three successive sittings of the Royal Society were taken up by the delivery of the lecture.[*] This glass did not turn out to be of important practical use, but it happened afterwards to be the foundation of two of Faraday's greatest discoveries.[†]

The experiments here referred to, were commenced at the Falcon Glass Works, on the premises of Messrs. Green and Pellatt, but Faraday could not conveniently attend to them there. In 1827 therefore a furnace was erected in the yard of the Royal Institution; and it was at this time, and with a view of assisting him at the furnace, that Faraday engaged Sergeant Anderson, of the Royal Artillery, the respectable, truthful, and altogether trustworthy man whose appearance here is so fresh in our memories. Anderson continued to be the reverential helper of Faraday and the faithful servant of this Institution for nearly forty years.[‡]

In 1831 Faraday published a paper "On a peculiar class of Optical Deceptions," to which I believe the beautiful optical toy called the Chromatrope owes its origin. In the same year he published a paper on Vibrating Surfaces, in which he solved an acoustical problem which, though of extreme simplicity *when solved*, appears to have baffled many eminent men. The problem was to account for the fact that light bodies, such as the seed of lycopodium, collected at the vibrating parts of sounding plates, while sand ran to the nodal lines. Faraday showed that the light bodies were entangled in the little whirlwinds formed in the air over the places of vibration, and through which the heavier sand was readily projected. Faraday's resources as an experimentalist were so wonderful, and his delight in experiment was so great, that he sometimes almost ran into excess in this direction. I have heard him say that this paper on vibrating surfaces was too heavily laden with experiments.

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[*] *Viz.* November 19, December 3 and 10.

[†] I make the following extract from a letter from Sir John Herschel, written to me from Collingwood, on the 3rd of November, 1867:—

"I will take this opportunity to mention that I believe myself to have originated the suggestion of the employment of borate of lead for *optical* purposes. It was somewhere in the year 1822, as well as I can recollect, that I mentioned it to Sir James (then Mr.) South; and, in consequence, the trial was made in his laboratory in Blackman-street, by precipitating and working a large quantity of borate of lead, and fusing it under a muffle in a porcelain evaporating dish. A very limpid (though slightly yellow) glass resulted, the refractive index 1·866! (which you will find set down in my table of refractive indices in my article 'Light,' 'Encyclopædia Metropolitana'). It was, however, too soft for optical use as an object glass. This Faraday overcame, at least to a considerable degree, by the introduction of silica."

[‡] Regarding Anderson, Faraday writes thus in 1845:—"I cannot resist the occasion that is thus offered to me of mentioning the name of Mr. Anderson, who came to me as an assistant in the glass experiments, and has remained ever since in the laboratory of the Royal Institution. He assisted me in all the researches into which I have entered since that time; and to his care, steadiness, exactitude, and faithfulness in the performance of all that has been committed to his charge, I am much indebted.—M. F."—*Exp. Researches*, vol. iii., p. 3, footnote.

Discovery of Magneto-electricity: Explanation of Arago's Magnetism of Rotation: Terrestrial Magneto-electric Induction: The Extra Current.

The work thus far referred to, though sufficient of itself to secure no mean scientific reputation, forms but the vestibule of Faraday's achievements. He had been engaged within these walls for eighteen years.* During part of the time he had drunk in knowledge from Davy, and during the remainder he continually exercised his capacity for independent inquiry. In 1831 we have him at the climax of his intellectual strength, forty years of age, stored with knowledge and full of original power. Through reading, lecturing, and experimenting, he had become thoroughly familiar with electrical science: he saw where light was needed and expansion possible. The phenomena of ordinary electric induction belonged, as it were, to the alphabet of his knowledge: he knew that under ordinary circumstances the presence of an electrified body was sufficient to excite, by induction, an unelectrified body. He knew that the wire which carried an electric current was an electrified body, and still that all attempts had failed to make it excite in other wires a state similar to its own.

What was the reason of this failure? Faraday never could work from the experiments of others, however clearly described. He knew well that from every experiment issued a kind of radiation, luminous in different degrees to different minds, and he hardly trusted himself to reason upon an experiment that he had not seen. In the autumn of 1831 he began to repeat the experiments with electric currents, which, up to that time, had produced no positive result. And here, for the sake of younger inquirers, if not for the sake of us all, it is worth while to dwell for a moment on a power which Faraday possessed in an extraordinary degree. He united vast strength with perfect flexibility. His momentum was that of a river which combines weight and directness with the ability to yield to the flexures of its bed. The intentness of his vision in any direction did not apparently diminish his power of perception in other directions; and when he attacked a subject, expecting results, he had the faculty of keeping his mind alert, so that results different from those which he expected should not escape him through pre-occupation.

He began his experiments "on the induction of electric currents" by composing a helix of two insulated wires, which were wound side by side round the same wooden cylinder One of these wires he connected with a voltaic battery of ten cells, and the other with a sensitive galvanometer. When connection with the battery was made, and while the current flowed, no effect whatever was observed at the galvanometer. But he never accepted an experimental result, until he had applied to it the utmost power at his command. He raised his battery from 10 cells to 120 cells, but without avail. The current flowed calmly through the battery wire without producing, during its flow, any sensible result upon the galvanometer.

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* He used to say that it required twenty years of work to make *a man* in Physical Science; the previous period being one of *infancy*.

"During its flow," and this was the time when an effect was expected—but here Faraday's power of lateral vision, separating, as it were, from the line of expectation, came into play—he noticed that a feeble movement of the needle always occurred at the moment when he made contact with the battery; that the needle would afterwards return to its former position and remain quietly there, unaffected by the *flowing* current. At the moment, however, when the circuit was interrupted the needle again moved, and in a direction opposed to that observed on the completion of the circuit.

This result and others of a similar kind led him to the conclusion "that the battery current through the one wire did in reality induce a similar current through the other; but that it continued for an instant only, and partook more of the nature of the electric wave from a common Leyden jar than of the current from a voltaic battery." The momentary currents thus generated were called *induced currents*, while the current which generated them was called the *inducing* current. It was immediately proved that the current generated at making the circuit was always opposed in direction to its generator, while that developed on the rupture of the circuit coincided in direction with the inducing current. It appeared as if the current on its first rush through the primary wire sought a purchase in the secondary one, and, by a kind of kick, impelled backward through the latter an electric wave, which subsided as soon as the primary current was fully established.

Faraday for a time believed that the secondary wire, though quiescent when the primary current had been once established, was not in its natural condition, its return to that condition being declared by the current observed at breaking the circuit. He called this hypothetical state of the wire the *electro-tonic state:* he afterwards abandoned this hypothesis, but seemed to return to it in later life. The term electro-tonic is also preserved by Professor Du Bois Reymond to express a certain electric condition of the nerves, and Professor Clerk Maxwell has ably defined and illustrated the hypothesis in the tenth volume of the 'Transactions of the Cambridge Philosophical Society.'

The mere approach of a wire forming a closed curve to a second wire through which a voltaic current flowed was then shown by Faraday to be sufficient to arouse in the neutral wire an induced current, opposed in direction to the inducing current; the withdrawal of the wire also generated a current having the same direction as the inducing current; those currents existed only during the time of approach or withdrawal, and when neither the primary nor the secondary wire was in motion, no matter how close their proximity might be, no induced current was generated.

Faraday has been called a purely inductive philosopher. A great deal of nonsense is, I fear, uttered in this land of England about induction and deduction. Some profess to befriend the one, some the other, while the real vocation of an investigator, like Faraday, consists in the incessant marriage of both. He was at this time full of the theory of Ampère, and it cannot be doubted that numbers of his experiments were executed merely to test his deductions from that theory. Starting from the discovery of Oersted, the celebrated French philosopher had shown that all the phenomena of magnetism then known might be reduced to the mutual attractions and repulsions of electric currents. Magnetism had been produced from electricity, and Faraday, who all his life long entertained a strong belief in such reciprocal actions, now attempted to effect the evolution of electricity from magnetism. Round a welded iron ring he placed two distinct coils of covered wire, causing the coils to occupy opposite halves of the ring. Connecting the ends of one of the coils with a galvanometer, he found that the moment the ring was magnetized by sending a current through *the other coil*, the galvanometer needle whirled round four or five times in succession, the action, as before, was that of a pulse which vanished immediately. On interrupting the circuit, a whirl of the needle in the opposite direction occurred. It was only during the time of magnetization or demagnetization that these effects were produced. The induced currents declared a *change* of condition only, and they vanished the moment the act of magnetization or demagnetization was complete.

The effects obtained with the welded ring were also obtained with straight bars of iron. Whether the bars were magnetized by the electric current, or were excited by the contact of permanent steel magnets, induced currents were always generated during the rise and during the subsidence of the magnetism. The use of iron was then abandoned, and the same effects obtained by merely thrusting a permanent steel magnet into a coil of wire. A rush of electricity through the coil accompanied the insertion of the magnet; an equal rush in the opposite direction accompanied its withdrawal. The precision with which Faraday describes these results, and the completeness with which he defines the boundaries of his facts, are wonderful. The magnet, for example, must not be passed quite through the coil, but only half through, for if passed wholly through, the needle is stopped as by a blow, and then he shows how this blow results from a reversal of the electric wave in the helix. He next operated with the powerful permanent magnet of the Royal Society, and obtained with it, in an exalted degree, all the foregoing phenomena.

And now he turned the light of these discoveries upon the darkest physical phenomenon of that day. Arago had discovered in 1824, that a disk of non-magnetic metal had the power of bringing a vibrating magnetic needle suspended over it rapidly to rest; and that on causing the disk to rotate the magnetic needle rotated along with it. When both were quiescent, there was not the slightest measurable attraction or repulsion exerted between the needle and the disk; still when in motion the disk was competent to drag after it not only a light needle but a heavy magnet. The question had been probed and investigated with admirable skill by both Arago and Ampère, and Poisson had published a theoretic memoir on the subject; but no cause could be assigned for so extraordinary an action. It had also been examined in this country by two celebrated men, Mr. Babbage and Sir John Herschel; but it still remained a mystery. Faraday always recommended the suspension of judgment in cases of doubt. "I have always admired," he says, "the prudence and philosophical reserve shown by M. Arago in resisting the temptation to give a theory of the effect he had discovered, so long as he could not devise one which was perfect in its application, and in refusing to assent to the imperfect theories of others." Now, however, the time for theory had come. Faraday saw mentally the rotating disk under the operation of the magnet flooded with his induced currents; and from the known laws of interaction between currents and magnets he hoped to deduce the motion observed by Arago. That hope he realized, showing by actual experiment that when his disk rotated currents passed through it, their position and direction being such as must, in accordance with the established laws of electro-magnetic action, produce the observed rotation.

Introducing the edge of his disk between the poles of the large horseshoe magnet of the Royal Society, and connecting the axis and the edge of the disk, each by a wire with a galvanometer, he obtained when the disk was turned round a constant flow of electricity. The direction of the current was determined by the direction of the motion, the current being reversed when the rotation was reversed. He now states the law which rules the production of currents in both disks and wires, and in so doing uses for the first time a phrase which has since become famous. When iron filings are scattered over a magnet, the particles of iron arrange themselves in certain determinate lines called magnetic curves. In 1831, Faraday for the first time called these curves "lines of magnetic force;" and he showed that to produce induced currents neither approach to nor withdrawal from a magnetic source, or centre, or pole, was essential, but that it was only necessary to cut appropriately the lines of magnetic force. Faraday's first paper on magneto-electric induction, which I have here endeavoured to condense, was read before the Royal Society on the 24th of November, 1831.

On the 12th of January, 1832, he communicated to the Royal Society a second paper on Terrestrial Magneto-electric Induction, which was chosen as the Bakerian Lecture for the year. He placed a bar of iron in a coil of wire, and lifting the bar into the direction of the dipping needle, he excited by this action a current in the coil. On reversing the bar, a current in the opposite direction rushed through the wire. The same effect was produced, when, on holding the helix in the line of dip, a bar of iron was thrust into it. Here, however, the earth acted on the coil through the intermediation of the bar of iron. He abandoned the bar and simply set a copper-plate spinning in a horizontal plane; he knew that the earth's lines of magnetic force then crossed the plate at an angle of about 70°. When the plate spun round, the lines of force were intersected and induced currents generated, which produced their proper effect when carried from the plate to the galvanometer. "When the plate was in the magnetic meridian, or in any other plane coinciding with the magnetic dip, then its rotation produced no effect upon the galvanometer."

At the suggestion of a mind fruitful in suggestions of a profound and philosophic character—I mean that of Sir John Herschel—Mr. Barlow, of Woolwich, had experimented with a rotating iron shell. Mr. Christie had also performed an elaborate series of experiments on a rotating iron disk. Both of them had found that when in rotation the body exercised a peculiar action upon the magnetic needle, deflecting it in a manner which was not observed during quiescence; but neither of them was aware at the time of the agent which produced this extraordinary deflection. They ascribed it to some change in the magnetism of the iron shell and disk.

But Faraday at once saw that his induced currents must come into play here, and he immediately obtained them from an iron disk. With a hollow brass ball, moreover, he produced the effects obtained by Mr. Barlow. Iron was in no way necessary: the only condition of success was that the rotating body should be of a character to admit of the formation of currents in its substance: it must, in other words, be a conductor of electricity. The higher the conducting power, the more copious were the currents. He now passes from his little brass globe to the globe of the earth. He plays like a magician with the earth's magnetism. He sees the invisible lines along which its magnetic action is exerted, and sweeping his wand across these lines he evokes this new power. Placing a simple loop of wire round a magnetic needle he bends its upper portion to the west: the north pole of the needle immediately swerves to the east: he bends his loop to the east, and the north pole moves to the west. Suspending a common bar magnet in a vertical position, he causes it to spin round its own axis. Its pole being connected with one end of a galvanometer wire, and its equator with the other end, electricity rushes round the galvanometer from the rotating magnet. He remarks upon the "*singular independence*" of the magnetism and the body of the magnet which carries it. The steel behaves as if it were isolated from its own magnetism.

And then his thoughts suddenly widen, and he asks himself whether the rotating earth does not generate induced currents as it turns round its axis from west to east. In his experiment with the twirling magnet the galvanometer wire remained at rest; one portion of the circuit was in motion *relatively* to *another portion*. But in the case of the twirling planet the galvanometer wire would necessarily be carried along with the earth; there would be no relative motion. What must be the consequence? Take the case of a telegraph wire with its two terminal plates dipped into the earth, and suppose the wire to lie in the magnetic meridian. The ground underneath the wire is influenced like the wire itself by the earth's rotation; if a current from south to north be generated in the wire, a similar current from south to north would be generated in the earth under the wire; these currents would run against the same terminal plate, and thus neutralize each other.

This inference appears inevitable, but his profound vision perceived its possible invalidity. He saw that it was at least possible that the difference of conducting power between the earth and the wire might give one an advantage over the other, and that thus a residual or differential current might be obtained. He combined wires of different materials, and caused them to act in opposition to each other: but found the combination ineffectual. The more copious flow in the better conductor was exactly counterbalanced by the resistance of the worst. Still though experiment was thus emphatic he would clear his mind of all discomfort by operating on the earth itself. He went to the round lake near Kensington Palace, and stretched 480 feet of copper wire, north and south, over the lake, causing plates soldered to the wire at its ends to dip into the water. The copper wire was severed at the middle, and the severed ends connected with a galvanometer. No effect whatever was observed. But though quiescent water gave no effect, moving water might. He therefore worked at London Bridge for three days during the ebb and flow of the tide, but without any satisfactory result. Still he urges, "Theoretically it seems a necessary consequence, that where water is flowing there electric currents should be formed. If a line be imagined passing from Dover to Calais through the sea, and returning through the land, beneath the water, to Dover, it traces out a circuit of conducting matter one part of which, when the water moves up or down the channel, is cutting the magnetic curves of the earth, whilst the other is relatively at rest. . . . There is every reason to believe that currents do run in the general direction of the circuit described, either one way or the other, according as the passage of the waters is up or down the Channel." This was written before the submarine cable was thought of, and he once informed me that actual observation upon that cable had been found to be in accordance with his theoretic deduction.*

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* I am indebted to a friend for the following exquisite morsel:—"A short time after the publication of Faraday's first researches in magneto-electricity, he attended the meeting of the British Association at Oxford, in 1832.—On this occasion he was requested by some of the authorities to repeat the celebrated experiment of eliciting a spark from a magnet, employing for this purpose the large magnet in the

Three years subsequent to the publication of these researches, that is to say on the 29th of January, 1835, Faraday read before the Royal Society a paper "On the influence by induction of an electric current upon itself." A shock and spark of a peculiar character had been observed by a young man named William Jenkin, who must have been a youth of some scientific promise, but who, as Faraday once informed me, was dissuaded by his own father from having anything to do with science. The investigation of the fact noticed by Mr. Jenkin led Faraday to the discovery of the *extra current*, or the current *induced in the primary wire itself* at the moments of making and breaking contact, the phenomena of which he described and illustrated in the beautiful and exhaustive paper referred to.

Seven and thirty years have passed since the discovery of magneto-electricity; but, if we except the *extra current*, until quite recently nothing of moment was added to the subject. Faraday entertained the opinion that the discoverer of a great law or principle had a right to the "spoils"—this was his term—arising from its illustration; and guided by the principle he had discovered, his wonderful mind, aided by his wonderful ten fingers, overran in a single autumn this vast domain, and hardly left behind him the shred of a fact to be gathered by his successors.

And here the question may arise in some minds, What is the use of it all? The answer is, that if man's intellectual nature thirsts for knowledge, then knowledge is useful because it satisfies this thirst. If you demand practical ends, you must, I think, expand your definition of the term practical, and make it include all that elevates and enlightens the intellect, as well as all that ministers to the bodily health and comfort of men. Still, if needed, an answer of another kind might be given to the question, "What is its use?" As far as electricity has been applied for medical purposes it has been almost exclusively Faraday's electricity. You have noticed those lines of wire which cross the streets of London. It is Faraday's currents that speed from place to place through these wires. Approaching the point of Dungeness the mariner sees an unusually brilliant light, and from

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Ashmolean Museum. To this he consented, and a large party assembled to witness the experiments, which, I need not say, were perfectly successful. Whilst he was repeating them a dignitary of the university entered the room, and addressing himself to Professor Daniell, who was standing near Faraday, inquired what was going on. The Professor explained to him as popularly as possible this striking result of Faraday's great discovery. The Dean listened with attention and looked earnestly at the brilliant spark, but a moment after he assumed a serious countenance and shook his head; 'I am sorry for it,' said he as he walked away; in the middle of the room he stopped for a moment and repeated, 'I am sorry for it;' then walking towards the door, when the handle was in his hand he turned round and said, '*Indeed* I am sorry for it; it is putting new arms into the hands of the incendiary.' This occurred a short time after the papers had been filled with the doings of the hay-rick burners. An erroneous statement of what fell from the Dean's mouth was printed at the time in one of the Oxford papers. He is there wrongly stated to have said, 'It is putting new arms into the hands of the infidel.'"

the noble *phares* of La Hève the same light flashes across the sea. These are Faraday's sparks exalted by suitable machinery to sunlike splendour. At the present moment the Board of Trade and the Brethren of the Trinity House, as well as the Commissioners of Northern Lights, are contemplating the introduction of the magneto-electric light at numerous points upon our coasts; and future generations will be able to refer to those guiding stars in answer to the question, What has been the practical use of the labours of Faraday? But I would again emphatically say that his work needs no such justification, and that if he had allowed his vision to be disturbed by considerations regarding the practical use of his discoveries, those discoveries would never have been made by him. "I have rather," he writes in 1831, "been desirous of discovering new facts and new relations dependent on magneto-electric induction, than of exalting the force of those already obtained; being assured that the latter would find their full development hereafter."

In 1817, when lecturing before a private society in London on the element chlorine, Faraday thus expresses himself with reference to this question of utility:—"Before leaving this subject, I will point out the history of this substance, as an answer to those who are in the habit of saying to every new fact, 'What is its use?' Dr. Franklin says to such, 'What is the use of an infant?' The answer of the experimentalist is, 'Endeavour to make it useful.' When Scheele discovered this substance it appeared to have no use; it was in its infancy and useless state, but having grown up to maturity, witness its powers, and see what endeavours to make it useful have done."

Points of Character.

A point highly illustrative of the character of Faraday now comes into view. He gave an account of his discovery of magneto-electricity in a letter to his friend M. Hachette, of Paris, who communicated the letter to the Academy of Sciences. The letter was translated and published; and immediately afterwards two distinguished Italian philosophers took up the subject, made numerous experiments, and published their results before the complete memoirs of Faraday had met the public eye. This evidently irritated him. He reprinted the paper of the learned Italians in the 'Philosophical Magazine,' accompanied by sharp critical notes from himself. He also wrote a letter dated Dec. 1st, 1832, to Gay Lussac, who was then one of the editors of the 'Annales de Chimie,' in which he analyzed the results of the Italian philosophers, pointing out their errors, and defending himself from what he regarded as imputations on his character. The style of this letter is unexceptionable, for Faraday could not write otherwise than as a gentleman; but the letter shows that had he willed it he could have hit hard. We have heard much of Faraday's gentleness and sweetness and tenderness. It is all true, but it is very incomplete. You cannot resolve a powerful nature into these elements, and Faraday's character would have been less admirable than it was had it not embraced forces and tendencies to which the silky adjectives "gentle" and "tender" would by no means apply. Underneath his sweetness and gentleness was the heat of a volcano. He was a man of excitable and fiery nature; but through high self-discipline he had converted the fire into a central glow and motive power of life, instead of permitting it to waste itself in useless passion. "He that is slow to anger," saith the sage, "is greater than the mighty, and he that ruleth his own spirit than he that taketh a city." Faraday was *not* slow to anger, but he completely ruled his own spirit, and thus, though he took no cities, he captivated all hearts.

As already intimated, Faraday had contributed many of his minor papers—including his first analysis of caustic lime—to the 'Quarterly Journal of Science.' In 1832 he collected those papers and others together in a small octavo volume, labelled them, and prefaced them thus:—

"PAPERS, NOTES, NOTICES, &c., &c.,
published in octavo,
up to 1832.
M. FARADAY."

"*Papers* of mine, published in octavo, in the *Quarterly Journal of Science*, and elsewhere, since the time that Sir H. Davy encouraged me to write the analysis of caustic lime.

"Some, I think, (at this date) are good; others moderate; and some bad. But I have put *all* into the volume, because of the utility they have been of to me,—and none more than the bad,—in pointing out to me in future, or rather, after times, the faults it became me to watch and to avoid.

"As I never looked over one of my papers a year after it was written, without believing both in philosophy and manner it could have been much better done, I still hope the collection may be of great use to me.

"*Aug.* 18, 1832." "M. FARADAY.

"None more than the bad!" This is a bit of Faraday's innermost nature; and as I read these words, I am almost constrained to retract what I have said regarding the fire and excitability of his character. But is he not all the more admirable, through his ability to tone down and subdue that fire and that excitability, so as to render himself able to write thus as a little child? I once took the liberty of censuring the conclusion of a letter of his to the Dean of St. Paul's. He subscribed himself "humbly yours," and I objected to the adverb. "Well, but, Tyndall," he said, "I *am* humble; and still it would be a great mistake to think that I am not also proud." This duality ran through his character. A democrat in his defiance of all authority which unfairly limited his freedom of thought, and still ready to stoop in reverence to all that was really worthy of reverence, in the customs of the world or the characters of men.

And here, as well as elsewhere, may be introduced a letter which bears upon this question of self-control, written long years subsequent to the period at which we have now arrived. I had been at Glasgow in 1855, at a meeting of the British Association. On a certain day, I communicated a paper to the physical section, which was followed by a brisk discussion. Men of great distinction took part in it, the late Dr. Whewell among the number, and it waxed warm on both sides. I was by no means content with this discussion; and least of all with my own part in it. This discontent affected me for some days, during which I wrote to Faraday, giving him no details, but expressing in a general way my dissatisfaction. I give the following extract from his reply:—

"SYDENHAM, 6*th Oct.*, 1855.

"MY DEAR TYNDALL,

"These great meetings, of which I think very well altogether, advance science chiefly by bringing scientific men together, and making them to know and be friends with each other; and I am sorry when that is not the effect in every part of their course. I know nothing except from what you tell me, for I have not yet looked at the reports of the proceedings; but let me, as an old man, who ought by this time to have profited by experience, say that when I was younger, I found I often misinterpreted the intentions of people, and found they did not mean what at the time I supposed they meant; and, further, that as a general rule, it was better to be a little dull of apprehension, where phrases seemed to imply pique, and quick in perception, when on the contrary they seemed to imply kindly feeling. The real truth never fails ultimately to appear; and opposing parties if wrong are sooner convinced when replied to forbearingly, than when overwhelmed. All I mean to say is, that it is better to be blind to the results of partisanship, and quick to see good will. One has more happiness in oneself, in endeavouring to follow the things that make for peace. You can hardly imagine how often I have been heated in private when opposed, as I have thought unjustly and superciliously, and yet I have striven, and succeeded I hope, in keeping down replies of the like kind. And I know I have never lost by it. I would not say all this to you did I not esteem you as a true philosopher and friend.*

"Yours, very truly,

"M. FARADAY."

Identity of Electricities: First Researches on Electro-Chemistry.

I have already once used the word "discomfort" in reference to the occasional state of Faraday's mind when experimenting. It was to him a discomfort to reason upon data which admitted of doubt. He hated what he called "doubtful knowledge," and ever tended either to transfer it into the region of undoubtful knowledge, or of certain and definite ignorance. Pretence of all kinds, whether in life or in philosophy, was hateful to him. He wished to know the reality of our nescience as well as of our science. "Be one thing or the other," he seemed to say to an unproved hypothesis; "come out as a solid truth, or disappear as a convicted lie." After making the great discovery which I have attempted to describe, a doubt seemed to beset him as regards the identity of electricities. "Is it right," he seemed to ask, "to call this agency which I have discovered electricity at all? Are there perfectly conclusive grounds for believing that the electricity of the machine, the pile, the gymnotus and torpedo, magneto-electricity and thermo-electricity, are merely different manifestations of one and the same agent?" To answer this question to his own satisfaction, he formally reviewed the knowledge of that day. He added to it new experiments of his own, and finally decided in favour of the "Identity of Electricities." His paper upon this subject was read before the Royal Society on the 10th and 17th of January, 1833.

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* Faraday would have been rejoiced to learn that, during its last meeting at Dundee, the British Association illustrated in a striking manner the function which he here describes as its principal one. In my own case, a brotherly welcome was everywhere manifested. In fact, the differences of really honourable and sane men are never beyond healing.

After he had proved to his own satisfaction the identity of electricities, he tried to compare them quantitatively together. The terms quantity and intensity, which Faraday constantly used, need a word of explanation here. He might charge a single Leyden jar by twenty turns of his machine, or he might charge a battery of ten jars by the same number of turns. The *quantity* in both cases would be sensibly the same, but the *intensity* of the single jar would be the greatest, for here the electricity would be less diffused. Faraday first satisfied himself that the needle of his galvanometer was caused to swing through the same arc by the same quantity of machine electricity, whether it was condensed in a small battery or diffused over a large one. Thus the electricity developed by thirty turns of his machine produced, under very variable conditions of battery surface, the same deflection. Hence he inferred the possibility of comparing, as regards quantity, electricities which differ greatly from each other in intensity.

His object now is to compare frictional with voltaic electricity. Moistening bibulous paper with the iodide of potassium—a favourite test of his—and subjecting it to the action of machine electricity, he decomposed the iodide, and formed a brown spot where the iodine is liberated. Then he immersed two wires, one of zinc, the other of platinum, each $\frac{1}{18}$th of an inch in diameter, to a depth of $\frac{5}{8}$ths of an inch in acidulated water during eight beats of his watch, or ~~$\frac{3}{20}$ths of a second~~, and found that the needle of his galvanometer swung through the same arc, and coloured his moistened paper to the same extent, as thirty turns of his large electrical machine. Twenty-eight turns of the machine produced an effect distinctly less than that produced by his two wires. Now, the quantity of water decomposed by the wires in this experiment totally eluded observation; it was immeasurably small; and still that amount of decomposition involved the development of a quantity of electric force which, if applied in a proper form, would kill a rat, and no man would like to bear it.

In his subsequent researches "on the absolute quantity of electricity associated with the particles or atoms of matter," he endeavours to give an idea of the amount of electrical force involved in the decomposition of a single grain of water. He is almost afraid to mention it, for he estimates it at 800,000 discharges of his large

Leyden battery. This, if concentrated in a single discharge, would be equal to a very great flash of lightning; while the chemical action of a single grain of water on four grains of zinc would yield electricity equal in quantity to a powerful thunderstorm. Thus his mind rises from the minute to the vast, expanding involuntarily from the smallest laboratory fact till it embraces the largest and grandest natural phenomena.[*]

In reality, however, he is at this time only clearing his way, and he continues laboriously to clear it for some time afterwards. He is digging the shaft, guided by that instinct towards the mineral lode which was to him a rod of divination. "*Er riecht die Wahrheit*," said the lamented Kohlrausch, an eminent German, once in my hearing:—"He smells the truth." His eyes are now steadily fixed on this wonderful voltaic current, and he must learn more of its mode of transmission.

On the 23rd of May, 1833, he read a paper before the Royal Society "On a new Law of Electric Conduction." He found that though the current passed through water, it did not pass through ice:—why not, since they are one and the same substance? Some years subsequently he answered this question by saying that the liquid condition enables the molecule of water to turn round so as to place itself in the proper line of polarization, while the rigidity of the solid condition prevents this arrangement. This polar arrangement must precede decomposition, and decomposition is an accompaniment of conduction. He then passed on to other substances; to oxides and chlorides, and iodides and salts, and sulphurets, and found them all insulators when solid, and conductors when fused. In all cases, moreover, except one—and this exception he thought might be apparent only—he found the passage of the current across the fused compound to be accompanied by its decomposition. Is then the act of decomposition essential to the act of conduction in these bodies? Even recently this question was warmly contested. Faraday was very cautious latterly in expressing himself upon this subject; but as a matter of fact he held that an infinitesimal quantity of electricity might pass through a compound liquid without producing its decomposition. De la Rive, who has been a great worker on the chemical phenomena of the pile, is very emphatic on the other side. Experiment, according to him and others, establishes in the most conclusive manner that no trace of electricity can pass through a liquid compound without producing its equivalent decomposition.[†]

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[*] Buff finds the quantity of electricity associated with one milligramme of hydrogen in water, to be equal to 45,480 charges of a Leyden jar, with a height of 480 millimetres, and a diameter of 160 millimetres. Weber and Kohlrausch have calculated that if the quantity of electricity associated with one milligramme of hydrogen in water, were diffused over a cloud at a height of 1000 metres above the earth, it would exert upon an equal quantity of the opposite electricity at the earth's surface an attractive force of 2,268,000 kilogrammes.—*Electrolytische Maasbestimmungen*, 1866, p. 262.

[†] 'Faraday, sa Vie et ses Travaux,' p. 20.

Faraday has now got fairly entangled amid the chemical phenomena of the pile, and here his previous training under Davy must have been of the most important service to him. Why, he asks, should decomposition thus take place? what force is it that wrenches the locked constituent of these compounds asunder? On the 20th of June, 1833, he read a paper before the Royal Society "On Electro-Chemical Decomposition," in which he seeks to answer these questions. The notion had been entertained that the poles, as they are called, of the decomposing cell, or in other words the surfaces by which the current enters and quits the liquid, exercised electric attractions upon the constituents of the liquid and tore them asunder. Faraday combats this notion with extreme vigour. Litmus reveals, as you know, the action of an acid by turning red, turmeric reveals the action of an alkali by turning brown. Sulphate of soda, you know, is a salt compounded of the alkali soda and sulphuric acid. The voltaic current passing through a solution of this salt so decomposes it, that sulphuric acid appears at one pole of the decomposing cell and alkali at the other. Faraday steeped a piece of litmus paper and a piece of turmeric paper in a solution of sulphate of soda: placing each of them upon a separate plate of glass, he connected them together by means of a string moistened with the same solution. He then attached one of them to the positive conductor of an electric machine, and the other to the gas-pipes of this building. These he called his "discharging train." On turning the machine the electricity passed from paper to paper through the string, which might be varied in length from a few inches to seventy feet without changing the result. The first paper was reddened, declaring the presence of sulphuric acid; the second was browned, declaring the presence of the alkali soda. The dissolved salt, therefore, arranged in this fashion, was decomposed by the machine exactly as it would have been by the voltaic current. When instead of using the positive conductor he used the negative; the positions of the acid and alkali were reversed. Thus he satisfied himself that chemical decomposition by the machine is obedient to the laws which rule decomposition by the pile.

And now he gradually abolishes those so-called poles to the attraction of which electric decomposition had been ascribed. He connected a piece of turmeric paper moistened with the sulphate of soda with the positive conductor of his machine; then he placed a metallic point in connection with his discharging train opposite the moist paper, so that the electricity shall discharge through the air towards the point. The turning of the machine caused the corners of the piece of turmeric paper opposite to the point to turn brown, thus declaring the presence of alkali. He changed the turmeric for litmus paper, and placed it, not in connection with his conductor, but with his discharging train, a metallic point connected with the conductor being fixed at a couple of inches from the paper; on turning the machine, acid was liberated at the edges and corners of the litmus. He then placed a series of pointed pieces of paper, each separate piece being composed of two halves, one of litmus and the other of turmeric paper, and all moistened with sulphate of soda, in the line of the current from the machine. The pieces of paper were separated from each other by spaces of air. The machine was turned; and it was always found that at the point where the electricity entered the paper, litmus was reddened, and at the point where it quitted the paper, turmeric was browned. "Here," he urges, "the poles are entirely abandoned, but we have still electro-chemical decomposition." It is evident to him that instead of being *attracted* by the poles, the bodies separated are *ejected* by the current. The effects thus obtained with poles of air he also succeeded in obtaining with poles of water. The advance in Faraday's own ideas made at this time is indicated by the word "ejected." He afterwards reiterates this view: the evolved substances are *expelled* from the decomposing body and "*not drawn out by an attraction.*"

Having abolished this idea of polar attraction, he proceeds to enunciate and develop a theory of his own. He refers to Davy's celebrated Bakerian Lecture given in 1806, which he says "is almost entirely occupied in the consideration of electro-chemical decompositions." The facts recorded in that lecture Faraday regards as of the utmost value. But "the mode of action by which the effects take place is stated very generally; so generally indeed, that probably a dozen precise schemes of electro-chemical action might be drawn up, differing essentially from each other, yet all agreeing with the statement there given."

It appears to me that these words might with justice be applied to Faraday's own researches at this time. They furnish us with results of permanent value; but little help can be found in the theory advanced to account for them. It would, perhaps, be more correct to say that the theory itself is hardly presentable in any tangible form to the intellect. Faraday looks, and rightly looks, into the heart of the decomposing body itself: he sees, and rightly sees, active within it the forces which produce the decomposition, and he rejects, and rightly rejects, the notion of external attraction; but beyond the hypothesis of decompositions and recompositions, enunciated and developed by Grothuss and Davy, he does not, I think, help us to any definite conception as to how the force reaches the decomposing mass and acts within it. Nor, indeed, can this be done, until we know the true physical process which underlies what we call an electric current.

Faraday conceives of that current as "*an axis of power having contrary forces exactly equal in amount in opposite directions;*" but this definition, though much quoted and circulated, teaches us nothing regarding the current. An "axis" here can only mean a direction; and what we want to be able to conceive of is, not the axis along which the power acts, but the nature and mode of action of the power itself. He objects to the vagueness of De la Rive; but the fact is that both he and De la Rive labour under the same difficulty. Neither wishes to commit himself to the notion of a current compounded of two electricities flowing in two opposite directions; but the time had not come, nor is it yet come, for the displacement of this provisional fiction by the true mechanical conception. Still, however indistinct the theoretic notions of Faraday at this time may be, the facts which are rising before him and around him are leading him gradually, but surely, to results of incalculable importance in relation to the philosophy of the voltaic pile.

He had always some great object of research in view, but in the pursuit of it he frequently alighted on facts of collateral interest, to examine which he sometimes turned aside from his direct course. Thus we find the series of his researches on electro-chemical decomposition interrupted by an inquiry into "the power of metals and other solids, to induce the combination of gaseous bodies." This inquiry, which was received by the Royal Society on the 30th of November, 1833, though not so important as those which precede and follow it, illustrates throughout his strength as an experimenter. The power of spongy platinum to cause the combination of oxygen and hydrogen had been discovered by Döbereiner in 1823, and had been applied by him in the construction of his well-known philosophic lamp. It was shown subsequently by Dulong and Thenard that even a platinum wire, when perfectly cleansed, may be raised to incandescence by its action on a jet of cold hydrogen.

In his experiments on the decomposition of water, Faraday found that the positive platinum plate of the decomposing cell possessed in an extraordinary degree the power of causing oxygen and hydrogen to combine. He traced the cause of this to the perfect cleanness of the positive plate. Against it was liberated oxygen, which with the powerful affinity of the "nascent state," swept away all impurity from the surface against which it was liberated. The bubbles of gas liberated on one of the platinum plates or wires of a decomposing cell are always much smaller, and they rise in much more rapid succession than those from the other. Knowing that oxygen is sixteen times heavier than hydrogen, I have more than once concluded, and, I fear, led others into the error of concluding, that the smaller and more quickly rising bubbles must belong to the lighter gas. The thing appeared so obvious that I did not give myself the trouble of looking at the battery, which would at once have told me the nature of the gas. But Faraday would never have been satisfied with a deduction if he could have reduced it to a fact. And he has taught me that the fact here is the direct reverse of what I supposed it to be. The small bubbles are oxygen, and their smallness is due to the perfect cleanness of the surface on which they are liberated. The hydrogen adhering to the other electrode swells into large bubbles, which rise in much slower succession; but when the current is reversed the hydrogen is liberated upon the cleansed wire, and then its bubbles also become small.

Laws of Electro-Chemical Decomposition.

In our conceptions and reasonings regarding the forces of nature, we perpetually make use of symbols which, when they possess a high representative value we dignify with the name of theories. Thus prompted by certain analogies we ascribe electrical phenomena to the action of a peculiar fluid, sometimes flowing, sometimes at rest. Such conceptions have their advantages and their disadvantages; they afford peaceful lodging to the intellect for a time, but they also circumscribe it, and by and by, when the mind has grown too large for its lodging, it often finds difficulty in breaking down the walls of what has become its prison instead of its home.*

No man ever felt this tyranny of symbols more deeply than Faraday, and no man was ever more assiduous than he to liberate himself from them and the terms which suggested them. Calling Dr. Whewell to his aid in 1833, he endeavoured to displace by others all terms tainted by a foregone conclusion. His paper on Electro-chemical decomposition, received by the Royal Society on the 9th of January, 1834, opens with the proposal of a new terminology. He would avoid the word "current" if he could.† He does abandon the word "poles" as applied to the ends of a decomposing cell, because it suggests the idea of attraction, substituting for it the perfectly neutral term *electrodes*. He applied the term *electrolyte* to every substance which can be decomposed by the current, and the act of decomposition he calls *electrolysis*. All these terms have become current in science. He called the positive electrode the *Anode*, and the negative one the *Cathode*, but these terms, though frequently used, have not enjoyed the same currency as the others. The terms *Anion* and *Cation*, which he applied to the constituents of the decomposed electrolyte, and the term *ion*, which included both anions and cations, are still less frequently employed.

Faraday now passes from terminology to research; he sees the necessity of quantitative determinations, and seeks to supply himself with a measure of voltaic electricity. This he finds in the quantity of water decomposed by the current. He tests this measure in all possible ways, to assure himself that no error can arise from its employment. He places in the course of one and the same current a series of cells with electrodes of different sizes, some of them plates of platinum, others merely platinum wires, and collects the gas liberated on each distinct pair of electrodes. He finds the quantity of gas to be the same for all. Thus he concludes that when the same quantity of electricity is caused to pass through a series of cells containing acidulated water, the electro-chemical action is independent of the size of the electrodes. He next proves that variations in intensity do not interfere with this equality of action. Whether his battery is charged with strong acid or with weak; whether it consists of five pairs or of fifty pairs; in short, whatever be its source, when the same current is sent through his series of cells, the same amount of decomposition takes place in all. He next assures himself that the strength or weakness of his dilute acid does not interfere with this law. Sending the same current through a series of cells containing mixtures of sulphuric acid and water of different strengths, he finds, however the proportion of acid to water might vary, the same amount of gas to be collected in all the cells. A crowd of facts of this character forced upon Faraday's mind the conclusion that the amount of electro-chemical decomposition depends, not upon the size of the electrodes, not upon the intensity of the current, not upon the strength of the solution, but solely upon the quantity of electricity which passes through the cell. The quantity of electricity he concludes is proportional to the amount of chemical action. On this law Faraday based the construction of his celebrated voltameter, or measurer of voltaic electricity.

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* I copy these words from the printed abstract of a Friday evening lecture, given by myself, because they remind me of Faraday's voice, responding to the utterance by an emphatic *hear! hear!*—*Proceedings of the Royal Institution*, vol. ii., p. 132.

† In 1838 he expresses himself thus:—"The word current is so expressive in common language that when applied in the consideration of electrical phenomena, we can hardly divest it sufficiently of its meaning, or prevent our minds from being prejudiced by it."—*Exp. Researches*, vol. i., p. 515. (§ 1617.)

But before he can apply this measure he must clear his ground of numerous possible sources of error. The decomposition of his acidulated water is certainly a *direct* result of the current; but as the varied and important researches of MM. Becquerel, De la Rive, and others had shown, there are also *secondary* actions, which may materially interfere with and complicate the pure action of the current. These actions may occur in two ways: either the liberated *ion* may seize upon the electrode against which it is set free, forming a chemical compound with that electrode; or it may seize upon the substance of the electrolyte itself, and thus introduce into the circuit chemical actions over and above those due to the current. Faraday subjected these secondary actions to an exhaustive examination. Instructed by his experiments, and rendered competent by them to distinguish between primary and secondary results, he proceeds to establish the doctrine of "definite electro-chemical decomposition."

Into the same circuit he introduced his voltameter, which consisted of a graduated tube filled with acidulated water and provided with platinum plates for the decomposition of the water, and also a cell containing chloride of tin. Experiments already referred to had taught him that this substance, though an insulator when solid, is a conductor when fused, the passage of the current being always accompanied by the decomposition of the chloride. He wished now to ascertain what relation this decomposition bore to that of the water in his voltameter.

Completing his circuit, he permitted the current to continue until "a reasonable quantity of gas" was collected in the voltameter. The circuit was then broken, and the quantity of tin liberated compared with the quantity of gas. The weight of the former was 3·2 grains, that of the latter 0·49742 of a grain. Oxygen, as you know, unites with hydrogen in the proportion of 8 to 1 to form water. Calling the equivalent, or, as it is sometimes called, the atomic weight of hydrogen 1, that of oxygen is 8; that of water is consequently 8 + 1, or 9. Now if the quantity of water decomposed in Faraday's experiment be represented by the number 9, or in other words by the equivalent of water, then the quantity of tin liberated from the fused chloride is found by an easy calculation to be 57·9, which is almost exactly the chemical equivalent of tin. Thus both the water and the chloride were broken up in proportions expressed by their respective equivalents. The amount of electric force which wrenched asunder the constituents of the molecule of water was competent, and neither more nor less than competent, to wrench asunder the constituents of the molecules of the chloride of tin. The fact is typical. With the indications of his voltameter he compared the decomposition of other substances both singly and in series. He submitted his conclusions to numberless tests. He purposely introduced secondary actions. He endeavoured to hamper the fulfilment of those laws which it was the intense desire of his mind to see established. But from all these difficulties emerged the golden truth, that under every variety of circumstances the decompositions of the voltaic current are as definite in their character as those chemical combinations which gave birth to the atomic theory. This law of electro-chemical decomposition ranks, in point of importance, with that of definite combining proportions in chemistry.

Origin of Power in the Voltaic Pile.

In one of the public areas of the town of Como stands a statue, with no inscription on its pedestal save that of a single name, "Volta." The bearer of that name occupies a place for ever memorable in the history of science. To him we owe the discovery of the voltaic pile, to which, for a brief interval, we must now turn our attention.

The objects of scientific thought being the passionless laws and phenomena of external nature, one might suppose that their investigation and discussion would be completely withdrawn from the region of the feelings, and pursued by the cold dry light of the intellect alone. This, however, is not always the case. Man carries his heart with him into all his works. You cannot separate the moral and emotional from the intellectual; and thus it is that the discussion of a point of science may rise to the heat of a battle-field. The fight between the rival optical theories of Emission and Undulation was of this fierce character; and scarcely less fierce for many years was the contest as to the origin and maintenance of the power of the voltaic pile. Volta himself supposed it to reside in the contact of different metals. Here was exerted his "electro-motive force," which tore the combined electricities asunder and drove them as currents in opposite directions. To render the circulation of the current possible, it was necessary to connect the metals by a moist conductor; for when any two metals were connected by a third, their relation to each other was such that a complete neutralization of the electric motion was the result. Volta's theory of metallic contact was so clear, so beautiful, and apparently so complete, that the best intellects of Europe accepted it as the expression of natural law.

Volta himself knew nothing of the chemical phenomena of the pile; but as soon as these became known, suggestions and intimations appeared that chemical action, and not metallic contact, might be the real source of voltaic electricity. This idea was expressed by Fabroni in Italy and by Wollaston in England. It was developed and maintained by those "admirable electricians," Becquerel, of Paris, and De la Rive, of Geneva. The contact theory, on the other hand, received its chief development and illustration in Germany. It was long the scientific creed of the great chemists and natural philosophers of that country, and to the present hour there may be some of them unable to liberate themselves from the fascination of their first-love.

After the researches which I have endeavoured to place before you, it was impossible for Faraday to avoid taking a side in this controversy. He did so in a paper "On the Electricity of the Voltaic Pile," received by the Royal Society, on the 7th of April, 1834. His position in the controversy might have been predicted. He saw chemical effects going hand-in-hand with electrical effects, the one being proportional to the other; and, in the paper now before us, he proved that when the former were excluded, the latter were sought for in vain. He produced a current without metallic contact; he discovered liquids which, though competent to transmit the feeblest currents—competent therefore to allow the electricity of contact to flow through them if it were able to form a current—were absolutely powerless when chemically inactive.

One of the very few experimental mistakes of Faraday occurred in this investigation. He thought that with a single voltaic cell he had obtained the spark *before the metals touched*, but he subsequently discovered his error. To enable the voltaic spark to pass through air before the terminals of the battery were united, it was necessary to exalt the electro-motive force of the battery by multiplying its elements; but all the elements Faraday possessed were unequal to the task of urging the spark across the shortest measurable space of air. Nor, indeed, could the action of the battery, the different metals of which were in contact with each other, decide the point in question. Still as regards the identity of electricities from various sources, it was at that day of great importance to determine whether or not the voltaic current could jump as a spark across an interval before contact. Faraday's friend, Mr. Gassiot, solved this problem. He erected a battery of 4000 cells, and with it urged a stream of sparks from terminal to terminal, when separated from each other by a measurable space of air.

The memoir on the "Electricity of the Voltaic Pile," published in 1834, appears to have produced but little impression upon the supporters of the contact theory. These indeed were men of too great intellectual weight and insight lightly to take up, or lightly to abandon a theory. Faraday therefore resumed the attack in a paper communicated to the Royal Society, on the 6th of February, 1840. In this paper he hampered his antagonists by a crowd of adverse experiments. He hung difficulty after difficulty about the neck of the contact theory, until in its efforts to escape from his assaults it so changed its character as to become a thing totally different from the theory proposed by Volta. The more persistently it was defended, however, the more clearly did it show itself to be a congeries of devices, bearing the stamp of dialectic skill rather than that of natural truth.

In conclusion, Faraday brought to bear upon it an argument which, had its full weight and purport been understood at the time, would have instantly decided the controversy. "The contact theory," he urged, "assumes that a force which is able to overcome powerful resistance, as for instance that of the conductors, good or bad, through which the current passes, and that again of the electrolytic action where bodies are decomposed by it, *can arise out of nothing:* that without any change in the acting matter, or the consumption of any generating force, a current shall be produced which shall go on for ever against a constant resistance, or only be stopped, as in the voltaic trough, by the ruins which its exertion has heaped up in its own course. This would indeed be *a creation of power*, and is like no other force in nature. We have many processes by which the *form* of the power may be so changed, that an apparent *conversion* of one into the other takes place. So we can change chemical force into the electric current, or the current into chemical force. The beautiful experiments of Seebeck and Peltier show the convertibility of heat and electricity; and others by Oersted and myself show the convertibility of electricity and magnetism. *But in no case, not even in those of the Gymnotus and Torpedo, is there a pure creation or a production of power without a corresponding exhaustion of something to supply it.*"

These words were published more than two years before either Mayer printed his brief but celebrated essay on the Forces of Inorganic Nature, or Mr. Joule published his first famous experiments on the Mechanical Value of Heat. They illustrate the fact that before any great scientific principle receives distinct enunciation by individuals, it dwells more or less clearly in the general scientific mind. The intellectual plateau is already high, and our discoverers are those who, like peaks above the plateau, rise a little above the general level of thought at the time.

But many years prior, even to the foregoing utterance of Faraday, a similar argument had been employed. I quote here with equal pleasure and admiration the following passage written by Dr. Roget so far back as 1829. Speaking of the contact theory, he says:—"If there could exist a power having the property ascribed it to by the hypothesis, namely, that of giving continual impulse to a fluid in one constant direction, without being exhausted by its own action, it would differ essentially from all the known powers in nature. All the powers and sources of motion with the operation of which we are acquainted, when producing these peculiar effects, *are expended in the same proportion as those effects are produced; and hence arises the impossibility of obtaining by their agency a perpetual effect; or in other words a perpetual motion.* But the electro-motive force, ascribed by Volta to the metals, when in contact, is a force which, as long as a free course is allowed to the electricity it sets in motion, is never expended, and continues to be excited with undiminished power in the production of a never-ceasing effect. Against the truth of such a supposition the probabilities are all but infinite." When this argument, which he employed independently, had clearly fixed itself in his mind, Faraday never cared to experiment further on the source of electricity in the voltaic pile. The argument appeared to him "to remove *the foundation itself* of the contact theory," and he afterwards let it crumble down in peace.*

Researches on Frictional Electricity: Induction: Conduction: Specific Inductive Capacity: Theory of Contiguous Particles.

The burst of power which had filled the four preceding years with an amount of experimental work unparalleled in the history of science partially subsided in 1835, and the only scientific paper contributed by Faraday in that year was a comparatively unimportant one, "On an improved Form of the Voltaic Battery." He brooded for a time: his experiments on electrolysis had long filled his mind; he looked, as already stated, into the very heart of the electrolyte, endeavouring to render the play of its atoms visible to his mental eye. He had no doubt that in this case what is called "the electric current" was propagated from particle to particle of the electrolyte; he accepted the doctrine of decomposition and recomposition which, according to Grothuss and Davy, ran from electrode to electrode. And the thought impressed him more and more that ordinary electric induction was also transmitted and sustained by the action of "*contiguous particles.*"

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* To account for the *electric current,* which was really the core of the whole discussion, Faraday demonstrated the impotence of the contact theory as then enunciated and defended. Still, it is certain that two different metals, when brought into contact, charge themselves, the one with positive and the other with negative electricity. I had the pleasure of going over this ground with Kohlrausch in 1849, and his experiments left no doubt upon my mind that the contact electricity of Volta was a reality, though it could produce no current. With one of the beautiful instruments devised by himself, Sir William Thomson has rendered this point capable of sure and easy demonstration; and he and others now hold what may be called *a* contact theory, which, while it takes into account the action of the metals, also embraces the chemical phenomena of the circuit. Helmholtz, I believe, was the first to give the contact theory this new form, in his celebrated essay, *Ueber die Erhaltung der Kraft,* p. 45.

His first great paper on frictional electricity was sent to the Royal Society on the 30th of November, 1837. We here find him face to face with an idea which beset his mind throughout his whole subsequent life,—the idea of *action at a distance*. It perplexed and bewildered him. In his attempts to get rid of this perplexity he was often unconsciously rebelling against the limitations of the intellect itself. He loved to quote Newton upon this point: over and over again he introduces his memorable words, "That gravity should be innate, inherent, and essential to matter, so that one body may act upon another at a distance through a *vacuum* and without the mediation of anything else, by and through which this action and force may be conveyed from one to another, is to me so great an absurdity, that I believe no man who has in philosophical matters a competent faculty of thinking can ever fall into it. Gravity must be caused by an agent acting constantly according to certain laws; but whether this agent be material or immaterial I have left to the consideration of my readers."*

Faraday does not see the same difficulty in his contiguous particles. And yet by transferring the conception from masses to particles we simply lessen size and distance, but we do not alter the quality of the conception. Whatever difficulty the mind experiences in conceiving of action at sensible distances, besets it also when it attempts to conceive of action at insensible distances. Still the investigation of the point whether electric and magnetic effects were wrought out through the intervention of contiguous particles or not, had a physical interest altogether apart from the metaphysical difficulty. Faraday grapples with the subject experimentally. By simple intuition he sees that action at a distance must be exerted in straight lines. Gravity, he knows, will not turn a corner, but exerts its pull along a right line; hence his aim and effort to ascertain whether electric action ever takes place in curved lines. This once proved, it would follow that the action is carried on *by means of a medium* surrounding the electrified bodies. His experiments in 1837, reduced, in his opinion, this point to demonstration. He then found that he could electrify by induction an insulated sphere placed completely in the shadow of a body which screened it from direct action. He pictured the lines of electric force bending round the edges of the screen, and reuniting on the other side of it; and he proved that in many cases the augmentation of the distance between his insulated sphere and the inducing body, instead of lessening, increased the charge of the sphere. This he ascribed to the coalescence of the lines of electric force at some distance behind the screen.

Faraday's theoretic views on this subject have not received general acceptance, but they drove him to experiment, and experiment with him was always prolific of results. By suitable arrangements he places a metallic sphere in the middle of a large hollow sphere, leaving a space of something more than half-an-inch between them. The interior sphere was insulated, the external one uninsulated. To the former he communicated a definite charge of electricity. It acted by induction upon the concave surface of the latter, and he examined how this act of induction was effected by placing insulators of various kinds between the two spheres. He tried gases, liquids, and solids, but the solids alone gave him positive results. He constructed two instruments of the foregoing description, equal in size and similar in form. The interior sphere of each communicated with the external air by a brass stem ending in a knob. The apparatus was virtually a Leyden jar, the two coatings of which were the two spheres, with a thick and variable insulator between them. The amount of charge in each jar was determined by bringing a proof-plane into contact with its knob, and measuring by a torsion balance the charge taken away. He first charged one of his instruments, and then dividing the charge with the other, found that when air intervened in both cases, the charge was equally divided. But when shell-lac, sulphur, or spermaceti was interposed between the two spheres of one jar, while air occupied this interval in the other, then he found that the instrument occupied by the "solid dielectric" ~~takes~~ *more than half* the original charge. A portion of the charge was absorbed in the dielectric itself. The electricity took time to penetrate the dielectric. Immediately after the discharge of the apparatus no trace of electricity was found upon its knob. But after a time electricity was found there, the charge having gradually returned from the dielectric in which it had been lodged. Different insulators possess this power of permitting the charge to enter them in different degrees. Faraday figured their particles as polarized, and he concluded that the force of induction is propagated from particle to particle of the dielectric from the inner sphere to the outer one. This power of propagation possessed by insulators he calls their "*Specific Inductive Capacity.*"

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* Newton's third letter to Bentley.

Faraday visualizes with the utmost clearness the state of his contiguous particles; one after another they become charged, each succeeding particle depending for its charge upon its predecessor. And now he seeks to break down the wall of partition between conductors and insulators. "Can we not," he says, "by a gradual chain of association carry up discharge from its occurrence in air through spermaceti and water to solutions, and then on to chlorides, oxides, and metals, without any essential change in its character?" Even copper, he urges, offers a resistance to the transmission of electricity. The action of its particles differs from those of an insulator only in degree. They are charged like the particles of the insulator, but they discharge with greater ease and rapidity; and this rapidity of molecular discharge is what we call conduction. Conduction then is always preceded by atomic induction; and when through some quality of the body, which Faraday does not define, the atomic discharge is rendered slow and difficult, conduction passes into insulation.

Though they are often obscure, a fine vein of philosophic thought runs through those investigations. The mind of the philosopher dwells amid those agencies which underlie the visible phenomena of Induction and Conduction; and he tries by the strong light of his imagination to see the very molecules of his dielectrics. It would, however, be easy to criticize these researches, easy to show the looseness, and sometimes the inaccuracy, of the phraseology employed; but this critical spirit will get little good out of Faraday. Rather let those who ponder his works seek to realize the object he set before him, not permitting his occasional vagueness to interfere with their appreciation of his speculations. We may see the ripples, and eddies, and vortices of a flowing stream, without being able to resolve all these motions into their constituent elements; and so it sometimes strikes me that Faraday clearly saw the play of fluids and ethers and atoms, though his previous training did not enable him to resolve what he saw into its constituents, or describe it in a manner satisfactory to a mind versed in mechanics. And then again occur, I confess, dark sayings, difficult to be understood, which disturb my confidence in this conclusion. It must, however, always be remembered that he works at the very boundaries of our knowledge, and that his mind habitually dwells in the "boundless contiguity of shade" by which that knowledge is surrounded.

In the researches now under review the ratio of speculation and reasoning to experiment is far higher than in any of Faraday's previous works. Amid much that is entangled and dark we have flashes of wondrous insight and utterances which seem less the product of reasoning than of revelation. I will confine myself here to one example of this divining power:—By his most ingenious device of a rapidly rotating mirror, Wheatstone had proved that electricity required time to pass through a wire, the current reaching the middle of the wire later than its two ends. "If," says Faraday, "the two ends of the wire in Professor Wheatstone's experiments were immediately connected with two large insulated metallic surfaces exposed to the air, so that the primary act of induction, after making the contact for discharge, might be in part removed from the internal portion of the wire at the first instance, and disposed for the moment on its surface jointly with the air and surrounding conductors, then I venture to anticipate that the middle spark would be more retarded than before. And if those two plates were the inner and outer coatings of a large jar or Leyden battery, then the retardation of the spark would be much greater." This was only a *prediction*, for the experiment was not made.* Sixteen years subsequently, however, the proper conditions came into play, and Faraday was able to show that the observations of Werner Siemens, and Latimer Clark, on subterraneous and submarine wires were illustrations, on a grand scale, of the principle which he had enunciated in 1838. The wires and the surrounding water act as a Leyden jar, and the retardation of the current predicted by Faraday manifests itself in every message sent by such cables.

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* If Sir Charles Wheatstone could be induced to take up his measurements once more, varying the substances through which, and the conditions under which the current is propagated, he might render great service to science, both theoretic and experimental.

The meaning of Faraday in these memoirs on Induction and Conduction is, as I have said, by no means always clear; and the difficulty will be most felt by those who are best trained in ordinary theoretic conceptions. He does not know the reader's needs, and he therefore does not meet them. For instance, he speaks over and over again of the impossibility of charging a body with one electricity, though the impossibility is by no means evident. The key to the difficulty is this. He looks upon every insulated conductor as the inner coating of a Leyden jar. An insulated sphere in the middle of a room is to his mind such a coating; the walls are the outer coating, while the air between both is the insulator, across which the charge acts by induction. Without this reaction of the walls upon the sphere you could no more, according to Faraday, charge it with electricity than you could charge a Leyden jar, if its outer coating were removed. Distance with him is immaterial. His strength as a generalizer enables him to dissolve the idea of magnitude; and if you abolished the walls of the room—even the earth itself—he would make the sun and planets the outer coating of his jar. I dare not contend that Faraday in these memoirs made all his theoretic positions good. But a pure vein of philosophy runs through these writings; while his experiments and reasonings on the forms and phenomena of electrical discharge are of imperishable importance.

Rest needed—Visit to Switzerland.

The last of these memoirs was dated from the Royal Institution in June, 1838. It concludes the first volume of his 'Experimental Researches on Electricity.' In 1840, as already stated, he made his final assault on the contact theory, from which it never recovered.* He was now feeling the effects of the mental strain to which he had been subjected for so many years. During these years he repeatedly broke down. His wife alone witnessed the extent of his prostration, and to her loving care we, and the world, are indebted for the enjoyment of his presence here so long. He found occasional relief in a theatre. He frequently quitted London and went to Brighton and elsewhere, always choosing a situation which commanded a view of the sea, or of some other pleasant horizon, where he could sit and gaze and feel the gradual revival of the faith that

> "Nature never did betray
> The heart that loved her."

But very often for some days after his removal to the country he would be unable to do more than sit at a window and look out upon the sea and sky.

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* See note, p. 226.

In 1841, his state became more serious than it had ever been before. A published letter to Mr. Richard Taylor, dated March 11, 1843, contains an allusion to his previous condition. "You are aware," he says, "that considerations regarding health have prevented me from working or reading on science for the last two years." This, at one period or another of their lives, seems to be the fate of most great investigators. They do not know the limits of their constitutional strength until they have transgressed them. It is, perhaps, right that they should transgress them, in order to ascertain where they lie. Faraday, however, though he went far towards it, did not push his transgression beyond his power of restitution. In 1841 Mrs. Faraday and he went to Switzerland, under the affectionate charge of her brother, Mr. George Barnard, the artist. This time of suffering throws fresh light upon his character. I have said that sweetness and gentleness were not its only constituents; that he was also fiery and strong. At the time now referred to his fire was low and his strength distilled away; but the residue of his life was neither irritability nor discontent. He was unfit to mingle in society, for conversation was a pain to him; but let us observe the great Man-child when alone. He is at the village of Interlaken, enjoying Jungfrau sunsets, and at times watching the Swiss nailers making their nails. He keeps a little journal, in which he describes the process of nail-making, and incidentally throws a luminous beam upon himself.

"August 2nd, 1841. Clout nail-making goes on here rather considerably, and is a very neat and pretty operation to observe. I love a smith's shop and anything relating to smithery. *My father was a smith.*"

From Interlaken he went to the Falls of the Giessbach, on the pleasant lake of Brientz. And here we have him watching the shoot of the cataract down its series of precipices. It is shattered into foam at the base of each, and tossed by its own recoil as water-dust through the air. The sun is at his back, shining on the drifting spray, and he thus describes and muses on what he sees :—

"August 12th, 1841. To-day every fall was foaming from the abundance of water, and the current of wind brought down by it was in some places too strong to stand against. The sun shone brightly, and the rainbows seen from various points were very beautiful. One at the bottom of a fine but furious fall was very pleasant,—there it remained motionless whilst the gusts and clouds of spray swept furiously across its place and were dashed against the rock. It looked like a spirit strong in faith and steadfast in the midst of the storm of passions sweeping across it, and though it might fade and revive, still it held on to the rock as in hope and giving hope. And the very drops, which in the whirlwind of their fury seemed as if they would carry all away, were made to revive it and give it greater beauty."

Magnetization of Light.

But we must quit the man and go on to the discoverer: we shall return for a brief space to his company by and by. Carry your thoughts back to his last experiments, and see him endeavouring to prove that induction is due to the action of contiguous particles. He knew that polarized light was a most subtle and delicate investigator of molecular condition. He used it in 1834 in exploring his electrolytes, and he tried it in 1838 upon his dielectrics. At that time he coated two opposite faces of a glass cube with tinfoil, connected one coating with his powerful electric machine and the other with the earth, and examined by polarized light the condition of the glass when thus subjected to strong electric influence. He failed to obtain any effect, still he was persuaded an action existed, and required only suitable means to call it forth.

After his return from Switzerland he was beset by these thoughts: they were more inspired than logical; but he resorted to magnets and proved his inspiration true. His dislike of "doubtful knowledge" and his efforts to liberate his mind from the thraldom of hypotheses have been already referred to. Still this rebel against theory was incessantly theorizing himself. His principal researches are all connected by an undercurrent of speculation. Theoretic ideas were the very sap of his intellect—the source from which all his strength as an experimenter was derived. While once sauntering with him through the Crystal Palace, at Sydenham, I asked him what directed his attention to the magnetization of light. It was his theoretic notions. He had certain views regarding the unity and convertibility of natural forces; certain ideas regarding the vibrations of light and their relations to the lines of magnetic force; these views and ideas drove him to investigation. And so it must always be: the great experimentalist must ever be the habitual theorist, whether or not he gives to his theories formal enunciation.

Faraday, you must have been informed, endeavoured to improve the manufacture of glass for optical purposes. But though he produced a heavy glass of great refractive power, its value to optics did not repay him for the pains and labour bestowed on it. Now, however, we reach a result established by means of this same heavy glass, which made ample amends for all.

In November, 1845, he announced his discovery of the "Magnetization of Light, and the Illumination of the Lines of Magnetic Force." This title provoked comment at the time, and caused misapprehension. He therefore added an explanatory note; but the note left his meaning as entangled as before. In fact, Faraday had notions regarding the magnetization of light which were peculiar to himself, and untranslatable into the scientific language of the time. Probably no other philosopher of his day would have employed the phrases just quoted as appropriate to the discovery announced in 1845. But Faraday was more than a philosopher; he was a prophet, and often wrought by an inspiration to be understood by sympathy alone. The prophetic element in his character occasionally coloured and even injured the utterance of the man of science; but subtracting that element, though you might have conferred on him intellectual symmetry, you would have destroyed his motive force.

But let us pass from the label of this casket to the jewel it contains. "I have long," he says, "held an opinion almost amounting to conviction, in common I believe with many other lovers of natural knowledge, that the various forms under which the forces of matter are made manifest have one common origin; in other words, are so directly related and mutually dependent, that they are convertible, as it were, into one another, and possess equivalents of power in their action. . . . . This strong persuasion," he adds, "extended to the powers of light." And then he examines the action of magnets upon light. From conversation with him and Anderson, I should infer that the labour preceding this discovery was very great. The world knows little of the toil of the discoverer. It sees the climber jubilant on the mountain-top, but does not know the labour expended in reaching it. Probably hundreds of experiments had been made on transparent crystals before he thought of testing his heavy glass. Here is his own clear and simple description of the result of his first experiment with this substance:—"A piece of this glass, about two inches square, and 0·5 of an inch thick, having flat and polished edges, was placed as a *diamagnetic** between the poles (not as yet magnetized by the electric current), so that the polarized ray should pass through its length; the glass acted as air, water, or any other transparent substance would do; and if the eye-piece were previously turned into such a position that the polarized ray was extinguished, or rather the image produced by it rendered invisible, then the introduction of the glass made no alteration in this respect. In this state of circumstances, the force of the electro-magnet was developed by sending an electric current through its coils, and immediately the image of the lamp-flame became visible, and continued so long as the arrangement continued magnetic. On stopping the electric current, and so causing the magnetic force to cease, the light instantly disappeared. These phenomena could be renewed at pleasure, at any instant of time, and upon any occasion, showing a perfect dependence of cause and effect."

In a beam of ordinary light the particles of the luminiferous ether vibrate in all directions perpendicular to the line of progression; by the act of polarization, performed here by Faraday, all oscillations but those parallel to a certain plane are eliminated. When the plane of vibration of the polarizer coincides with that of the analyzer, a portion of the beam passes through both; but when these two planes are at right angles to each other, the beam is extinguished. If by any means, while the polarizer and analyzer remain thus crossed, the plane of vibration of the polarized beam between them could be changed, then the light would be, in part at least, transmitted. In Faraday's experiment this was accomplished. His magnet turned the plane of polarization of the beam through a certain angle, and thus enabled it to get through the analyzer; so that "the magnetization of light and the illumination of the magnetic lines of force" becomes when expressed in the language of modern theory, *the rotation of the plane of polarization.*

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* "By a *diamagnetic*," says Faraday, "I mean a body through which lines of magnetic force are passing, and which does not by their action assume the usual magnetic state of iron or loadstone." Faraday subsequently used this term in a different sense from that here given, as will immediately appear.

To him, as to all true philosophers, the main value of a fact was its position and suggestiveness in the general sequence of scientific truth. Hence, having established the existence of a phenomenon, his habit was to look at it from all possible points of view, and to develop its relationship to other phenomena. He proved that the direction of the rotation depends upon the polarity of his magnet; being reversed when the magnetic poles are reversed. He showed that when a polarized ray passed through his heavy glass in a direction parallel to the magnetic lines of force, the rotation is a maximum, and that when the direction of the ray is at right angles to the lines of force there is no rotation at all. He also proved that the amount of the rotation is proportional to the length of the diamagnetic through which the ray passes. He operated with liquids and solutions. Of aqueous solutions he tried 150 and more, and found the power in all of them. He then examined gases; but here all his efforts to produce any sensible action upon the polarized beam were ineffectual. He then passed from magnets to currents, enclosing bars of heavy glass, and tubes containing liquids and aqueous solutions within an electro-magnetic helix. A current sent through the helix caused the plane of polarization to rotate, and always *in the direction of the current.* The rotation was reversed when the current was reversed. In the case of magnets, he observed a gradual, though quick, ascent of the transmitted beam from a state of darkness to its maximum brilliancy when the magnet was excited. In the case of currents, the beam attained *at once* its maximum. This he showed to be due to *the time* required by the iron of the electro-magnet to assume its full magnetic power, which time vanishes when a current without iron is employed. "In this experiment," he says, "we may, I think, justly say that a ray of light is electrified, and the electric forces illuminated." In the helix, as with the magnets, he submitted *air* to magnetic influence "carefully and anxiously," but could not discover any trace of action on the polarized ray.

Many substances possess the power of turning the plane of polarization without the intervention of magnetism. Oil of turpentine and quartz are examples: but Faraday showed that, while in one direction, that is, across the lines of magnetic force, his rotation is zero, augmenting gradually from this until it attains its maximum, when the direction of the ray is parallel to the lines of force, in the oil of turpentine, the rotation is independent of the direction of the ray. But he showed that a still more profound distinction exists between the magnetic rotation and the natural one. I will try to explain how. Suppose a tube with glass ends containing oil of turpentine to be placed north and south. Fixing the eye at the south end of the tube, let a polarized beam be sent through it from the north. To the observer in this position the rotation of the plane of polarization, by the turpentine, is *right-handed.* Let the eye be placed at the north end of the tube and a beam be sent through it from the south: the rotation is still right-handed. Not so, however, when a bar of heavy glass is subjected to the action of an electric current. In this case if, in the first position of the eye, the rotation be right-handed, in the second position it is left-handed. These considerations make it manifest that if a polarized beam, after having passed through the oil of turpentine in its natural state, could, by any means, be reflected back through the liquid, the rotation impressed upon the direct beam would be exactly neutralized by that impressed upon the reflected one. Not so with the induced magnetic effect. Here it is manifest that the rotation would be doubled by the act of reflection. Hence Faraday concludes that the particles of the oil of turpentine which rotate by virtue of their natural force, and those which rotate in virtue of the induced force, cannot be in the same condition. The same remark applies to all bodies which possess a natural power of rotating the plane of polarization.

And then he proceeded with exquisite skill and insight to take advantage of this conclusion. He silvered the ends of his piece of heavy glass, leaving, however, a narrow portion parallel to two edges diagonally opposed to each other unsilvered. He then sent his beam through this uncovered portion, and by suitably inclining his glass caused the beam within it to reach his eye, first direct, and then after two, four, and six reflections. These corresponded to the passage of the ray once, three times, five times, and seven times through the glass. He thus established with numerical accuracy the exact proportionality of the rotation to the distance traversed by the polarized beam. Thus in one series of experiments where the rotation acquired by the direct beam was 12°, that acquired by three passages through the glass was 36°, while that acquired by five passages was 60°. But even when this method of magnifying was applied, he failed with various solid substances to obtain any effect; and in the case of air, though he employed to the utmost the power which these repeated reflections placed in his hands, he failed to produce the slightest sensible rotation.

These failures of Faraday to obtain the effect with gases, seem to indicate the true seat of the phenomenon. The luminiferous ether surrounds and is influenced by the ultimate particles of matter. The symmetry of the one involves that of the other. Thus, if the molecules of a crystal be perfectly symmetrical round any line through the crystal, we may safely conclude that a ray will pass along this line as through ordinary glass. It will not be doubly refracted. From the symmetry of the liquid figures, known to be produced in the planes of freezing, when radiant heat is sent through ice, we may safely infer symmetry of aggregation, and hence conclude that the line perpendicular to the planes of freezing is a line of no double refraction: that it is, in fact, the optic axis of the crystal. The same remark applies to the line joining the opposite blunt angles of a crystal of Iceland spar. The arrangement of the molecules round this line being symmetrical, the condition of the ether depending upon these molecules shares their symmetry; and there is, therefore, no reason why the wave-length should alter with the alteration of the azimuth round this line. Annealed glass has its molecules symmetrically arranged round every line that can be drawn through it; hence it is not doubly refractive. But let the substance be either squeezed or strained in one direction, the molecular symmetry, and with it the symmetry of the ether, is immediately destroyed and the glass becomes doubly refractive. Unequal heating produces the same effect. Thus mechanical strains reveal themselves by optical effects; and there is little doubt that in Faraday's experiment it is the *magnetic strain* that produces the rotation of the plane of polarization.*

Discovery of Diamagnetism—Researches on Magne-Crystallic Action.

Faraday's next great step in discovery was announced in a memoir on the "Magnetic Condition of all Matter," communicated to the Royal Society on the 18th of December, 1845. One great source of his success was the employment of extraordinary power. As already stated, he never accepted a negative answer to an experiment until he had brought to bear upon it all the force at his command. He had over and over again tried steel magnets and ordinary electro-magnets on various substances, but without detecting anything different from the ordinary attraction exhibited by a few of them. Stronger coercion, however, developed a new action. Before the pole of an electro-magnet, he suspended a fragment of his famous heavy glass; and observed that when the magnet was powerfully excited the glass fairly retreated from the pole. It was a clear case of magnetic *repulsion.* He then suspended a bar of the glass between two poles; the bar retreated when the poles were excited, and set its length *equatorially* or at right angles to the line joining them. When an ordinary magnetic body was similarly suspended, it always set *axially*, that is, from pole to pole.

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* The power of double refraction conferred on the centre of a glass rod, when it is caused to sound the fundamental note due to its longitudinal vibration, and the absence of the same power in the case of vibrating air (enclosed in a glass organ-pipe), seems to be analogous to the presence and absence of Faraday's effect in the same two substances.

Faraday never, to my knowledge, attempted to give, even in conversation, a picture of the molecular condition of his heavy glass when subjected to magnetic influence. In a mathematical investigation of the subject, published in the Proceedings of the Royal Society for 1856, Sir William Thomson arrives at the conclusion that the "diamagnetic" is in a state of molecular *rotation.*

Faraday called those bodies which were repelled by the poles of a magnet, *diamagnetic* bodies; using this term in a sense different from that in which he employed it in his memoir on the magnetization of light. The term *magnetic* he reserved for bodies which exhibited the ordinary attraction. He afterwards employed the term magnetic to cover the whole phenomena of attraction and repulsion, and used the word *paramagnetic* to designate such magnetic action as is exhibited by iron.

Isolated observations by Brugmanns, Becquerel, le Baillif, Saigy, and Seebeck had indicated the existence of a repulsive force exercised by the magnet on two or three substances; but these observations, which were unknown to Faraday, had been permitted to remain without extension or examination. Having laid hold of the fact of repulsion, Faraday immediately expanded and multiplied it. He subjected bodies of the most various qualities to the action of his magnet:—mineral salts, acids, alkalis, ethers, alcohols, aqueous solutions, glass, phosphorus, resins, oils, essences, vegetable and animal tissues, and found them all amenable to magnetic influence. No known solid or liquid proved insensible to the magnetic power when developed in sufficient strength. All the tissues of the human body, the blood—though it contains iron—included, were proved to be diamagnetic. So that if you could suspend a man between the poles of a magnet, his extremities would retreat from the poles until his length became equatorial.

Soon after he had commenced his researches on diamagnetism, Faraday noticed a remarkable phenomenon which first crossed my own path in the following way:—In the year 1849, while working in the cabinet of my friend, Professor Knoblauch, of Marburg, I suspended a small copper coin between the poles of an electro-magnet. On exciting the magnet, the coin moved towards the poles and then suddenly stopped, as if it had struck against a cushion. On breaking the circuit, the coin was repelled, the revulsion being so violent as to cause it to spin several times round its axis of suspension. A *Silbergroschen* similarly suspended exhibited the same deportment. For a moment I thought this a new discovery; but on looking over the literature of the subject, it appeared that Faraday had observed, multiplied, and explained the same effect during his researches on diamagnetism. His explanation was based upon his own great discovery of magneto-electric currents. The effect is a most singular one. A weight of several pounds of copper may be set spinning between the electro-magnetic poles; the excitement of the magnet instantly stops the rotation. Though nothing is apparent to the eye, the copper, if moved in the excited magnetic field, appears to move through a viscous fluid; while, when a flat piece of the metal is caused to pass to and fro like a saw between the poles, the sawing of the magnetic field resembles the cutting through of cheese or butter.* This virtual *friction* of the magnetic field is so strong that copper by its rapid rotation between the poles might probably be fused. We may easily dismiss this experiment by saying that the heat is due to the electric currents excited in the copper. But so long as we are unable to reply to the question, "What is an electric current?" the explanation is only provisional. For my own part, I look with profound interest and hope on the strange action here referred to.

Faraday's thoughts ran intuitively into experimental combinations, so that subjects whose capacity for experimental treatment would to ordinary minds seem to be exhausted in a moment, were shown by him to be all but inexhaustible. He has now an object in view, the first step towards which is the proof that the principle of Archimedes is true of magnetism. He forms magnetic solutions of various degrees of strength, places them between the poles of his magnet, and suspends in the solutions various magnetic bodies. He proves that when the solution is stronger than the body plunged in it, the body, though magnetic, is repelled; and when an elongated piece of it is surrounded by the solution it sets, like a diamagnetic body, equatorially between the excited poles. The same body when suspended in a solution of weaker magnetic power than itself is attracted as whole, while an elongated portion of it sets axially.

And now theoretic questions rush in upon him. Is this new force a true repulsion, or is it merely a differential attraction? Might not the apparent repulsion of diamagnetic bodies be really due to the greater attraction of the medium by which they are surrounded? He tries the rarefaction of air, but finds the effect insensible. He is averse to ascribing a capacity of attraction to space, or to any hypothetical medium supposed to fill space. He therefore inclines, but still with caution, to the opinion that the action of a magnet upon bismuth is a true and absolute repulsion, and not merely the result of differential attraction. And then he clearly states a theoretic view sufficient to account for the phenomena. "Theoretically," he says, "an explanation of the movements of the diamagnetic bodies, and all the dynamic phenomena consequent upon the action of magnets upon them, might be offered in the supposition that magnetic induction caused in them a contrary state to that which it produced in ordinary matter." That is to say, while in ordinary magnetic influence the exciting pole excites adjacent to itself the contrary magnetism, in diamagnetic bodies the adjacent magnetism is the same as that of the exciting pole. This theory of reversed polarity, however, does not appear to have ever laid deep hold of Faraday's mind; and his own experiments failed to give any evidence of its truth. He therefore subsequently abandoned it, and maintained the *non-polarity* of the diamagnetic force.

He then entered a new, though related field of inquiry. Having dealt with the metals and their compounds, and having classified all of them that came within the range of his observation under the two heads magnetic and diamagnetic, he began the investigation of the phenomena presented by crystals when subjected to magnetic power. The action of crystals had been in part theoretically predicted by Poisson,* and actually discovered by Plücker, whose beautiful results, at the period which we have now reached, profoundly interested all scientific men. Faraday had been frequently puzzled by the deportment of bismuth, a highly crystalline metal. Sometimes elongated masses of the substance refused to set equatorially, sometimes they set persistently oblique, and sometimes even, like a magnetic body, from pole to pole. "The effect," he says, "occurs at a single pole; and it is then striking to observe a long piece of a substance so diamagnetic as bismuth repelled, and yet at the same moment set round with force, axially, or end on, as a piece of magnetic substance would do." The effect perplexed him; and in his efforts to release himself from this perplexity, no feature of this new manifestation of force escaped his attention. His experiments are described in a memoir communicated to the Royal Society on the 7th of December, 1848.

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* See 'Heat as a Mode of Motion,' 3rd edition, § 36.

I have worked long myself at magne-crystallic action, amid all the light of Faraday's and Plücker's researches. The papers now before me were objects of daily and nightly study with me eighteen or nineteen years ago; but even now, though their perusal is but the last of a series of repetitions, they astonish me. Every circumstance connected with the subject; every shade of deportment; every variation in the energy of the action; almost every application which could possibly be made of magnetism to bring out in detail the character of this new force, is minutely described. The field is swept clean and hardly anything experimental is left for the gleaner. The phenomena he concludes are altogether different from those of magnetism or diamagnetism; they would appear in fact to present to us "a new force, or a new form of force, in the molecules of matter," which for convenience sake he designates by a new word, as "the *magne-crystallic* force."

He looks at the crystal acted upon by the magnet. From its mass he passes, in idea, to its atoms, and he asks himself whether the power which can thus seize upon the crystalline molecules, after they have been fixed in their proper positions by crystallizing force, may not, when they are free, be able to determine their arrangement? He therefore liberates the atoms by fusing the bismuth. He places the fused substance between the poles of an electro-magnet, powerfully excited; but he fails to detect any action. I think it cannot be doubted that an action is exerted here, that a true cause comes into play; but its magnitude is not such as sensibly to interfere with the force of crystallization, which, in comparison with the diamagnetic force, is enormous. "Perhaps," adds Faraday, "if a longer time were allowed and a permanent magnet used, a better result might be obtained. I had built many hopes upon the process." This expression, and his writings abound in such, illustrates what has been already said regarding his experiments being suggested and guided by his theoretic conceptions. His mind was full of hopes and hypotheses, but he always brought them to an experimental test. The record of his planned and executed experiments would, I doubt not, show a high ratio of hopes disappointed to hopes fulfilled; but every case of fulfilment abolished all memory of defeat; disappointment was swallowed up in victory.

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* See Sir William Thomson on Magne-crystallic Action, 'Phil. Mag.,' 1851.

After the description of the general character of this new force, Faraday states with the emphasis here reproduced its mode of action: "The *law* of action appears to be that *the line or axis of* MAGNE-CRYSTALLIC *force* (being the resultant of the action of all the molecules) *tends to place itself parallel, or as a tangent, to the magnetic curve, or line of magnetic force, passing through the place where the crystal is situated.*" The magne-crystallic force, moreover, appears to him "to be clearly distinguished from the magnetic or diamagnetic forces, in that it causes neither approach nor recession, consisting not in attraction or repulsion, but in giving a certain determinate position to the mass under its influence." And then he goes on "very carefully to examine and prove the conclusion that there was no connection of the force with attractive or repulsive influences." With the most refined ingenuity he shows that, under certain circumstances, the magne-crystallic force can cause the centre of gravity of a highly magnetic body to retreat from the poles, and the centre of gravity of a highly diamagnetic body to approach them. His experiments root his mind more and more firmly in the conclusion that it is "neither attraction nor repulsion causes the set, or governs the final position" of the crystal in the magnetic field. That the force which does so is therefore "distinct in its character and effects from the magnetic and diamagnetic forms of force. On the other hand," he continues, "it has a most manifest relation to the crystalline structure of bismuth and other bodies, and therefore to the power by which their molecules are able to build up the crystalline masses."

And here follows one of those expressions which characterize the conceptions of Faraday in regard to force generally:—"It appears to me impossible to conceive of the results in any other way than by a mutual reaction of the magnetic force, and the force of the particles of the crystal upon each other." He proves that the action of the force though thus molecular is an action at a distance; he shows that a bismuth crystal can cause a freely suspended magnetic needle to set parallel to its magne-crystallic axis. Few living men are aware of the difficulty of obtaining results like this, or of the delicacy necessary to their attainment. "But though it thus takes up the character of a force acting at a distance, still it is due to that power of the particles which makes them cohere in regular order and gives the mass its crystalline aggregation, which we call at other times the attraction of aggregation, and so often speak of as acting at *insensible* distances." Thus he broods over this new force, and looks at it from all possible points of inspection. Experiment follows experiment, as thought follows thought. He will not relinquish the subject as long as a hope exists of throwing more light upon it. He knows full well the anomalous nature of the conclusion to which his experiments lead him. But experiment to him is final, and he will not shrink from the conclusion. "This force," he says, "appears to me to be very strange and striking in its character. It is not polar, for there is no attraction or repulsion." And then, as if startled by his own utterance, he adds:—"What is the nature of the mechanical force which turns the crystal round, and makes it affect a magnet?" . . . "I do not remember," he continues, "heretofore such a case of force as the present one, where a body is brought into position only, without attraction or repulsion."

Plücker, the celebrated geometer already mentioned, who pursued experimental physics for many years of his life with singular devotion and success, visited Faraday in those days, and repeated before him his beautiful experiments on magneto-optic action. Faraday repeated and verified Plücker's observations, and concluded, what he at first seemed to doubt, that Plücker's results and magne-crystallic action have the same origin.

At the end of his papers, when he takes a last look along the line of research, and then turns his eyes to the future, utterances quite as much emotional as scientific escape from Faraday. "I cannot," he says, at the end of his first paper on magne-crystallic action, "conclude this series of researches without remarking how rapidly the knowledge of molecular forces grows upon us, and how strikingly every investigation tends to develop more and more their importance, and their extreme attraction as an object of study. A few years ago magnetism was to us an occult power, affecting only a few bodies, now it is found to influence all bodies, and to possess the most intimate relations with electricity, heat, chemical action, light, crystallization, and through it, with the forces concerned in cohesion; and we may, in the present state of things, well feel urged to continue in our labours, encouraged by the hope of bringing it into a bond of union with gravity itself."

Supplementary Remarks.

A brief space will, perhaps, be granted me here to state the further progress of an investigation which interested Faraday so much. Drawn by the fame of Bunsen as a teacher, in the year 1848 I became a student in the University of Marburg, in Hesse-Cassel. Bunsen behaved to me as a brother as well as a teacher, and it was also my happiness to make the acquaintance and gain the friendship of Professor Knoblauch, so highly distinguished by his researches on Radiant Heat. Plücker's and Faraday's investigations filled all minds at the time, and towards the end of 1849, Professor Knoblauch and myself commenced a joint investigation of the entire question. Long discipline was necessary to give us due mastery over it. Employing a method proposed by Dove, we examined the optical properties of our crystals ourselves; and these optical observations went hand in hand with our magnetic experiments. The number of these experiments was very great, but for a considerable time no fact of importance was added to those already published. At length, however, it was our fortune to meet with various crystals whose deportment could not be brought under the laws of magne-crystallic action enunciated by Plücker. We also discovered instances which led us to suppose that the magne-crystallic force was by no means independent, as alleged, of the magnetism or diamagnetism of the mass of the crystal. Indeed, the more we worked at the subject, the more clearly did it appear to us that the deportment of crystals in the magnetic field was due, not to a force previously unknown, but to the modification of the known forces of magnetism and diamagnetism by crystalline aggregation.

An eminent example of magne-crystallic action adduced by Plücker and experimented on by Faraday, was Iceland spar. It is what in optics is called a *negative* crystal, and according to the law of Plücker, the axis of such a crystal was always repelled by a magnet. But we showed that it was only necessary to substitute, in whole or in part, carbonate of iron for carbonate of lime, thus changing the magnetic but not the optical character of the crystal, to cause the axis to be attracted. That the deportment of magnetic crystals is exactly antithetical to that of diamagnetic crystals isomorphous with the magnetic ones, was proved to be a general law of action. In all cases, the line which in a diamagnetic crystal set equatorially, always set itself in an isomorphous magnetic crystal axially. By mechanical compression other bodies were also made to imitate the Iceland spar.

These and numerous other results bearing upon the question were published at the time in the 'Philosophical Magazine' and in 'Poggendorff's Annalen;' and the investigation of diamagnetism and magne-crystallic action was subsequently continued by me in the laboratory of Professor Magnus of Berlin. In December, 1851, after I had quitted Germany, Dr. Bence Jones went to the Prussian capital to see the celebrated experiments of Du Bois Reymond; and influenced, I suppose, by what he heard, he afterwards invited me to give a Friday evening discourse at the Royal Institution. I consented, not without fear and trembling. For the Royal Institution was to me a kind of dragon's den, where tact and strength would be necessary to save me from destruction. On February 11, 1853, the discourse was given, and it ended happily. I allude to these things that I may mention that though my aim and object in that lecture was to subvert the notions both of Faraday and Plücker, and to establish in opposition to their views what I regarded as the truth, it was very far from producing in Faraday either enmity or anger. At the conclusion of the lecture, he quitted his accustomed seat, crossed the theatre to the corner into which I had shrunk, shook me by the hand,

and brought me back to the table. Once more, subsequently, and in connection with a related question, I ventured to differ from him still more emphatically. It was done out of trust in the greatness of his character; nor was the trust misplaced. He felt my public dissent from him; and it pained me afterwards to the quick to think that I had given him even momentary annoyance. It was, however, only momentary. His soul was above all littleness and proof to all egotism. He was the same to me afterwards that he had been before; the very chance expression which led me to conclude that he felt my dissent being one of kindness and affection.

It required long subsequent effort to subdue the complications of magne-crystallic action, and to bring under the dominion of elementary principles the vast mass of facts which the experiments of Faraday and Plücker had brought to light. It was proved by Reich, Edmond Becquerel, and myself, that the condition of diamagnetic bodies in virtue of which they were repelled by the poles of a magnet, was excited in them by those poles; that the strength of this condition rose and fell with, and was proportional to, the strength of the acting magnet. It was not then any property possessed permanently by the bismuth, and which merely required the development of magnetism to act upon it, that caused the repulsion, for then the repulsion would have been simply proportional to the strength of the influencing magnet, whereas experiment proved it to augment as the square of the strength. The capacity to be repelled was therefore not inherent in the bismuth, but *induced*. So far an identity of action was established between magnetic and diamagnetic bodies. After this the deportment of magnetic bodies, "normal" and "abnormal," crystalline, amorphous, and compressed, was compared with that of crystalline, amorphous, and compressed diamagnetic bodies; and by a series of experiments, executed in the laboratory of this Institution, the most complete antithesis was established between magnetism and diamagnetism. This antithesis embraced the quality of polarity,—the theory of reversed polarity, first propounded by Faraday, being proved to be true. The discussion of the question was very brisk. On the continent Professor Wilhelm Weber was the ablest and most successful supporter of the doctrine of diamagnetic polarity; and it was with an apparatus, devised by him and constructed under his own superintendence, by Leyser of Leipzig, that the last demands of the opponents of diamagnetic polarity were satisfied. The establishment of this point was absolutely necessary to the explanation of magne-crystallic action.

With that admirable instinct which always guided him, Faraday had seen that it was possible, if not probable, that the diamagnetic force acts with different degrees of intensity in different directions, through the mass of a crystal. In his studies on electricity he had sought an experimental reply to the question whether crystalline bodies had not different specific inductive capacities in different directions, but he failed to establish any difference of the kind. His first attempt to establish differences of diamagnetic action in different directions through bismuth, was also a failure: but he must have felt this to be a point of cardinal importance, for he returned to the subject in 1850, and proved that bismuth was repelled with different degrees of force in different directions. It seemed as if the crystal were compounded of two diamagnetic bodies of different strengths, the substance being more strongly repelled across the magne-crystallic axis than along it. The same result was obtained independently, and extended to various other bodies, magnetic as well as diamagnetic, and also to compressed substances, a little subsequently by myself. The law of action in relation to this point is, that in diamagnetic crystals, the line along which the repulsion is a maximum, sets equatorially in the magnetic field; while in magnetic crystals the line along which the attraction is a maximum sets from pole to pole. Faraday had said that the magne-crystallic force was neither attraction nor repulsion. Thus far he was right. It was neither, taken singly, *but it was both.* By the combination of the doctrine of diamagnetic polarity with these differential attractions and repulsions, and by paying due regard to the character of the magnetic field, every fact brought to light in the domain of magne-crystallic action received complete explanation. The most perplexing of those facts were shown to result from the action of mechanical couples, which the proved polarity both of magnetism and diamagnetism brought into play. Indeed the thoroughness with which the experiments of Faraday were thus explained, is the most striking possible demonstration of the marvellous precision with which they were executed.

Magnetism of Flame and Gases: Atmospheric Magnetism.

When an experimental result was obtained by Faraday it was instantly enlarged by his imagination. I am acquainted with no mind whose power and suddenness of expansion at the touch of new physical truth could be ranked with his. Sometimes I have compared the action of his experiments on his mind to that of highly combustible matter thrown into a furnace: every fresh entry of fact was accompanied by the immediate development of light and heat. The light, which was intellectual, enabled him to see far beyond the boundaries of the fact itself, and the heat, which was emotional, urged him to the conquest of this newly-revealed domain. But though the force of his imagination was enormous, he bridled it like a mighty rider, and never permitted his intellect to be overthrown.

In virtue of the expansive power which his vivid imagination conferred upon him, he rose from the smallest beginnings to the grandest ends. Having heard from Zantedeschi that Bancalari had established the magnetism of flame, he repeated the experiments and augmented the results. He passed from flames to gases, examining and revealing their magnetic and diamagnetic powers; and then he suddenly rose from his bubbles of oxygen and nitrogen to the atmospheric envelope of the earth itself, and its relations to the great question of terrestrial magnetism. The rapidity with which these ever-augmenting thoughts assumed the form of experiments is unparalleled. His power in this respect is often best illustrated by his minor investigations, and, perhaps, by none more strikingly than by his paper "On the Diamagnetic Condition of Flame and Gases," published as a letter to Mr. Richard Taylor, in 'The Philosophical Magazine' for December, 1847. After verifying, varying, and expanding the results of Bancalari, he submitted to examination heated air-currents, produced by platinum spirals, placed in the magnetic field, and raised to incandescence by electricity. He then examined the magnetic deportment of gases generally. Almost all of these gases are invisible; but he must, nevertheless, track them in their unseen courses. He could not effect this by mingling smoke with his gases, for the action of his magnet upon the smoke would have troubled his conclusions. He, therefore, "caught" his gases in tubes, carried them out of the magnetic field, and made them reveal themselves at a distance from the magnet.

Immersing one gas in another, he determined their differential action; results of the utmost beauty being thus arrived at. Perhaps the most important are those obtained with atmospheric air and its two constituents. *Oxygen*, in various media, was strongly attracted by the magnet; in coal-gas, for example, it was powerfully magnetic, whereas *nitrogen* was diamagnetic. Some of the effects obtained with oxygen in coal-gas were strikingly beautiful. When the fumes of chloride of ammonia (a diamagnetic substance) were mingled with the oxygen, the cloud of chloride behaved in a most singular manner:—"The attraction of iron filings," says Faraday, "to a magnetic pole is not more striking than the appearance presented by the oxygen under these circumstances." On observing this deportment the question immediately occurs to him,—Can we not separate the oxygen of the atmosphere from its nitrogen by magnetic analysis? It is the perpetual occurrence of such questions that marks the great experimenter. The attempt to analyze atmospheric air by magnetic force proved a failure, like the previous attempt to influence crystallization by the magnet. The enormous comparative power of the force of crystallization was then assigned as a reason for the incompetence of the magnet to determine molecular arrangement: in the present instance the magnetic analysis is opposed by the force of diffusion, which is also very strong comparatively. The same remark applies to, and is illustrated by, another experiment subsequently executed by Faraday. Water is diamagnetic, sulphate of iron strongly magnetic. He enclosed "a dilute solution of sulphate of iron in a tube, and placed the lower end of the tube between the poles of a powerful horseshoe magnet for days together," but he could produce "no concentration of the solution in the part near the magnet." Here also the diffusibility of the salt was too powerful for the force brought against it.

The experiment last referred to is recorded in a paper presented to the Royal Society on the 2nd of August, 1850, in which he pursues the investigation of the magnetism of gases. Newton's observations on soap-bubbles were often referred to by Faraday. His delight in a soap-bubble was like that of a boy, and he often introduced them in his lectures, causing them, when filled with air, to float on invisible seas of carbonic acid, and otherwise employing them as means of illustration. He now finds them exceedingly useful in his experiments on the magnetic condition of gases. A bubble of air in a magnetic field occupied by air was unaffected, save through the feeble repulsion of its envelope. A bubble of nitrogen, on the contrary, was repelled from the magnetic axis with a force far surpassing that of a bubble of air. The deportment of oxygen in air "was very impressive, the bubble being pulled inward, or towards the axial line, sharply and suddenly, as if the oxygen were highly magnetic."

He next labours to establish the true magnetic zero, a problem not so easy as might at first sight be imagined. For the action of the magnet upon any gas, while surrounded by air, or any other gas, can only be differential; and if the experiment were made in vacuo, the action of the envelope, in this case necessarily of a certain thickness, would trouble the result. While dealing with this subject Faraday makes some note-worthy observations regarding *space*. In reference to the Torricellian vacuum, he says, "Perhaps it is hardly necessary for me to state that I find both iron and bismuth in such vacua, perfectly obedient to the magnet. From such experiments, and also from general observations and knowledge, it seems manifest that the lines of magnetic force can traverse pure space, just as gravitating force does, and as statical electrical forces do, and therefore space has a magnetic relation of its own, and one that we shall probably find hereafter to be of the utmost importance in natural phenomena. But this character of space is not of the same kind as that which, in relation to matter, we endeavour to express by the terms magnetic and diamagnetic. To confuse these together would be to confound space with matter, and to trouble all the conceptions by which we endeavour to understand and work out a progressively clearer view of the mode of action, and the laws of natural forces. It would be as if in gravitation or electric forces, one were to confound the particles acting on each other with the space across which they are acting, and would, I think, shut the door to advancement. Mere space cannot act as matter acts, even though the utmost latitude be allowed to the hypothesis of an ether; and admitting that hypothesis, it would be a large additional assumption to suppose that the lines of magnetic force are vibrations carried on by it, whilst as yet we have no proof that time is required for their propagation, or in what respect they may, in general character, assimilate to or differ from the respective lines of gravitating luminiferous or electric forces."

Pure space he assumes to be the true magnetic zero, but he pushes his inquiries to ascertain whether among material substances there may not be some which resemble space. If you follow his experiments you will soon emerge into the light of his results. A torsion beam was suspended by a skein of cocoon silk; at one end of the beam was fixed a cross-piece 1½ inches long. Tubes of exceedingly thin glass, filled with various gases, and hermetically sealed, were suspended in pairs from the two ends of the cross-piece. The position of the rotating torsion head was such that the two tubes were at opposite sides of, and equidistant from, the magnetic axis, that is to say from the line joining the two closely approximated polar points of an electro magnet. His object was to compare the magnetic action of the gases in the two tubes. When one tube was filled with oxygen, and the other with nitrogen, on the supervention of the magnetic force, the oxygen was pulled towards the axis, the nitrogen being pushed out. By turning the torsion head they could be restored to their primitive position of equidistance, where it is evident the action of the glass envelopes was annulled. The amount of torsion necessary to re-establish equidistance, expressed the *magnetic difference* of the substances compared.

And then he compared oxygen with oxygen at different pressures. One of his tubes contained the gas at the pressure of 30 inches of mercury, another at a pressure of 15 inches of mercury, a third at a pressure of 10 inches, while a fourth was exhausted as far as a good air-pump renders exhaustion possible. "When the first of these was compared with the other three, the effect was most striking." It was drawn towards the axis when the magnet was excited, the tube containing the rarer gas being apparently driven away, and the greater the difference between the densities of the two gases, the greater was the energy of this action.

And now observe his mode of reaching a *material* magnetic zero. When a bubble of nitrogen was exposed in air in the magnetic field, on the supervention of the power, the bubble retreated from the magnet. A less acute observer would have set nitrogen down as diamagnetic; but Faraday knew that retreat in a medium composed in part of oxygen might be due to the attraction of the latter gas, instead of to the repulsion of the gas immersed in it. But if nitrogen be really diamagnetic, then a bubble or bulb filled with the dense gas will overcome one filled with the rarer gas. From the cross-piece of his torsion-balance he suspended his bulbs of nitrogen, at equal distances from the magnetic axis, and found that the rarefaction, or the condensation of the gas in either of the bulbs had not the slightest influence. When the magnetic force was developed, the bulbs remained in their first position, even when one was *filled* with nitrogen, and the other as far as possible *exhausted*. Nitrogen, in fact, acted "like space itself;" it was neither magnetic nor diamagnetic.

He cannot conveniently compare the paramagnetic force of oxygen with iron, in consequence of the exceeding magnetic intensity of the latter substance; but he does compare it with the sulphate of iron, and finds that, bulk for bulk, oxygen is equally magnetic with a solution of this substance in water "containing seventeen times the weight of the oxygen in crystallized proto-sulphate of iron, or 3·4 times its weight of metallic iron in that state of combination." By its capability to deflect a fine glass fibre, he finds that the attraction of his bulb of oxygen, containing only 0·117 of a grain of the gas, at an average distance of more than an inch from the magnetic axis, is about equal to the gravitating force of the same amount of oxygen as expressed by its weight.

These facts could not rest for an instant in the mind of Faraday without receiving that expansion, to which I have already referred. "It is hardly necessary," he writes, "for me to say here that this oxygen cannot exist in the atmosphere exerting such a remarkable and high amount of magnetic force, without having a most important influence on the disposition of the magnetism of the earth, as a planet; especially, if it be remembered that its magnetic condition is greatly altered by variations of its density and by variations of its temperature. I think I see here the real cause of many of the variations of that force, which have been. and are now so carefully watched on different parts of the surface of the globe. The daily variation, and the annual variation, both seem likely to come under it; also very many of the irregular continual variations, which the photographic process of record renders so beautifully manifest. If such expectations be confirmed, and the influence of the atmosphere be found able to produce results like these, then we shall probably find a new relation between the aurora borealis and the magnetism of the earth, namely, a relation established, more or less, through the air itself in connection with the space above it; and even magnetic relations and variations, which are not as yet suspected, may be suggested and rendered manifest and measurable, in the further development of what I will venture to call *Atmospheric Magnetism.* I may be over-sanguine in these expectations, but as yet I am sustained in them by the apparent reality, simplicity, and sufficiency of the cause assumed, as it at present appears to my mind. As soon as I have submitted these views to a close consideration, and the test of accordance with observation, and, where applicable, with experiments also, I will do myself the honour to bring them before the Royal Society."

Two elaborate memoirs are then devoted to the subject of Atmospheric Magnetism; the first sent to the Royal Society on the 9th of October, and the second on the 19th of November, 1850. In these memoirs he discusses the effects of heat and cold upon the magnetism of the air, and the action on the magnetic needle, which must result from thermal changes. By the convergence and divergence of the lines of terrestrial magnetic force, he shows how the distribution of magnetism, in the earth's atmosphere, is affected. He applies his results to the explanation of the annual and of the diurnal variation: he also considers irregular variations, including the action of magnetic storms. He discusses, at length, the observations at St. Petersburg, Greenwich, Hobarton, St. Helena, Toronto, and the Cape of Good

Hope; believing that the facts, revealed by his experiments, furnish the key to the variations observed at all these places.

In the year 1851 I had the honour of an interview with Humboldt in Berlin, and his parting words to me then were, "Tell Faraday that I entirely agree with him, and that he has, in my opinion, completely explained the variation of the declination." Eminent men have since informed me that Humboldt was hasty in expressing this opinion. In fact, Faraday's memoirs on atmospheric magnetism lost much of their force—perhaps too much—through the important discovery of the relation of the variation of the declination to the number of the solar spots. But I agree with him and M. Edmond Becquerel, who worked independently at this subject, in thinking that a body so magnetic as oxygen, swathing the earth, and subject to variations of temperature, diurnal and annual, must affect the manifestations of terrestrial magnetism.* The air that stands upon a single square foot of the earth's surface is, according to Faraday, the equivalent in magnetic force to 8160lbs. of crystallized proto-sulphate of iron. Such a substance cannot be absolutely neutral as regards the deportment of the magnetic needle. But Faraday's writings on this subject are so voluminous, and the theoretic points are so novel and intricate, that I shall postpone the complete analysis of these researches to a time when I can lay hold of them more completely than my other duties allow me to do now.

Speculations: Nature of Matter: Lines of Force.

The scientific picture of Faraday would not be complete without a reference to his speculative writings. On Friday, January 19, 1844, he opened the weekly evening-meetings of the Royal Institution by a discourse entitled "A speculation touching Electric Conduction and the nature of Matter." In this discourse he not only attempts the overthrow of Dalton's Theory of Atoms, but also the subversion of all ordinary scientific ideas regarding the nature and relations of Matter and Force. He objected to the use of the term atom:—"I have not yet found a mind," he says, "that did habitually separate it from its accompanying temptations; and there can be no doubt that the words definite proportions, equivalent, primes, &c., which did and do fully express all the *facts* of what is usually called the atomic theory in chemistry, were dismissed because they were not expressive enough, and did not say all that was in the mind of him who used the word atom in their stead."

A moment will be granted me to indicate my own view of Faraday's position here. The word "atom" was not used in the stead of definite proportions, equivalents, or primes. These terms represented facts that followed from, but were not equivalent to the atomic theory. Facts cannot satisfy the mind: and the law of definite combining proportions being once established, the question "why should combination take place according to that law?" is inevitable. Dalton answered this question by the enunciation of the Atomic Theory, the fundamental idea of which is, in my opinion, perfectly secure. The objection of Faraday to Dalton might be urged with the same substantial force against Newton: it might be stated with regard to the planetary motions that the laws of Kepler revealed the *facts;* that the introduction of the principle of gravitation was an addition to the facts. But this is the essence of *all* theory. The theory is the backward guess from fact to principle; the conjecture, or divination regarding something, which lies behind the facts, and from which they flow in necessary sequence. If Dalton's theory then account for the definite proportions observed in the combinations of chemistry, its justification rests upon the same basis as that of the principle of gravitation. All that can in strictness be said in either case is that the facts occur *as if* the principle existed.

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* This persuasion has been greatly strengthened by the recent perusal of a paper by Mr. Baxendell.

The manner in which Faraday himself habitually deals with his hypotheses is revealed in this lecture. He incessantly employed them to gain experimental ends, but he incessantly took them down, as an architect removes the scaffolding when the edifice is complete. "I cannot but doubt," he says, "that he who as a mere philosopher has most power of penetrating the secrets of nature, *and guessing by hypothesis* at her mode of working, will also be most careful for his own safe progress, and that of others, to distinguish the knowledge which consists of assumption, by which I mean theory and hypothesis, from that which is the knowledge of facts and laws." Faraday himself, in fact, was always "guessing by hypothesis," and making theoretic divination the stepping-stone to his experimental results.

I have already more than once dwelt on the vividness with which he realized molecular conditions: we have a fine example of this strength and brightness of imagination in the present "speculation." He grapples with the notion that matter is made up of particles, not in absolute contact, but surrounded by interatomic space. "Space," he observes, "must be taken as the only *continuous part* of a body so constituted. Space will permeate all masses of matter in every direction like a net, except that in place of meshes it will form cells, isolating each atom from its neighbours, itself only being continuous."

Let us follow out this notion; consider, he argues, the case of a non-conductor of electricity, such for example as shell-lac, with its molecules, and their intermolecular spaces running through the mass. In its case space must be an insulator; for if it were a conductor it would resemble "*a fine metallic web*" penetrating the lac in every direction. But the fact is that it resembles the wax of black sealing-wax which surrounds and insulates the particles of conducting carbon, interspersed throughout its mass. In the case of shell-lac, therefore, *space is an insulator.*

But now take the case of a conducting metal. Here we have as before, the swathing of space round every atom. If space be an insulator there can be no transmission of electricity from atom to atom. But there *is* transmission; hence *space is a conductor*. Thus he endeavours to hamper the atomic theory. "The reasoning," he says, "ends in a subversion of that theory altogether; for if space be an insulator it cannot exist in conducting bodies, and if it be a conductor it cannot exist in insulating bodies. Any ground of reasoning," he adds, as if carried away by the ardour of argument, "which tends to such conclusions as these must in itself be false."

He then tosses the atomic theory from horn to horn of his dilemmas. What do we know, he asks, of the atom apart from its force? You imagine a nucleus which may be called *a*, and surround it by forces which may be called *m*; "to my mind the *a* or nucleus vanishes, and the substance consists in the powers of *m*. And indeed what notion can we form of the nucleus independent of its powers? What thought remains on which to hang the imagination of an *a* independent of the acknowledged forces." Like Boscovich he abolishes the atom and puts a "centre of force" in its place.

With his usual courage and sincerity he pushes his view to its utmost consequences. "This view of the constitution of matter," he continues, "would seem to involve necessarily the conclusion that matter fills all space, or at least all space to which gravitation extends; for gravitation is a property of matter dependent on a certain force, and it is this force which constitutes the matter. In that view matter is not merely mutually penetrable;* but each atom extends, so to say, throughout the whole of the solar system, yet always retaining its own centre of force."

It is the operation of a mind filled with thoughts of this profound, strange, and subtle character that we have to take into account in dealing with Faraday's later researches. A similar cast of thought pervades a letter addressed by Faraday to Mr. Richard Phillips, and published in the 'Philosophical Magazine' for May, 1846. It is entitled 'Thoughts on Ray-vibrations,' and it contains one of the most singular speculations that ever emanated from a scientific mind. It must be remembered here, that though Faraday lived amid such speculations he did not rate them highly, and that he was prepared at any moment to change them or let them go. They spurred him on, but they did not hamper him. His theoretic notions were *fluent;* and when minds less plastic than his own attempted to render those fluxional images rigid, he rebelled. He warns Phillips, moreover, that from first to last "he merely threw out as matter for speculation the vague impressions of his mind; for he gave nothing as the result of sufficient consideration, or as the settled conviction, or even probable conclusion at which he had arrived."

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* He compares the interpenetration of two atoms to the coalescence of two distinct waves, which though for a moment blended to a single mass, preserve their individuality, and afterwards separate.

The gist of this communication is that gravitating force acts in lines across space, and that the vibrations of light and radiant heat consist in the tremors of these lines of force. "This notion," he says, "as far as it is admitted, will dispense with the ether, which, in another view, is supposed to be the medium in which these vibrations take place." And he adds further on, that his view "endeavours to dismiss the ether but not the vibrations." The idea here set forth is the natural supplement of his previous notion that it is gravitating force which constitutes matter, each atom extending, so to say, throughout the whole of the solar system.

The letter to Mr. Phillips winds up with this beautiful conclusion:—

> "I think it likely that I have made many mistakes in the preceding pages, for even to myself my ideas on this point appear only as the shadow of a speculation, or as one of those impressions upon the mind which are allowable for a time as guides to thought and research. He who labours in experimental inquiries, knows how numerous these are, and how often their apparent fitness and beauty vanish before the progress and development of real natural truth."

Let it then be remembered that Faraday entertained notions regarding matter and force altogether distinct from the views generally held by scientific men. Force seemed to him an entity dwelling along the line in which it is exerted. The lines along which gravity acts between the sun and earth seem figured in his mind as so many elastic strings: indeed he accepts the assumed instantaneity of gravity as the expression of the enormous elasticity of the "lines of weight." Such views, fruitful in the case of magnetism, barren as yet in the case of gravity, explain his efforts to transform this latter force. When he goes into the open air and permits his helices to fall, to his mind's eye they are tearing through the lines of gravitating power, and hence his hope and conviction that an effect would and ought to be produced. It must ever be borne in mind that Faraday's difficulty in dealing with these conceptions was at bottom the same as that of Newton; that he is in fact trying to overleap this difficulty, and with it probably the limits prescribed to the intellect itself.

The idea of lines of magnetic force was suggested to Faraday by the linear arrangement of iron filings when scattered over a magnet. He speaks of and illustrates by sketches, the deflection, both convergent and divergent, of the lines of force, when they pass respectively through magnetic and diamagnetic bodies. These notions of concentration and divergence are also based on the direct observation of his filings. So long did he brood upon these lines; so habitually did he associate them with his experiments on induced currents, that the association became "indissoluble," and he could not think without them. "I have been so accustomed," he writes, "to employ them, and especially in my last researches, that I may have unwittingly become prejudiced in their favour, and ceased to be a clear-sighted judge. Still, I have always endeavoured to make experiment the test and controller of theory and opinion; but neither by that nor by close cross-examination in principle, have I been made aware of any error involved in their use."

In his later researches on magne-crystallic action, the idea of lines of force is extensively employed; it indeed led him to an experiment which lies at the root of the whole question. In his subsequent researches on Atmospheric Magnetism the idea receives still wider application, showing itself to be wonderfully flexible and convenient. Indeed without this conception the attempt to seize upon the magnetic actions, possible or actual, of the atmosphere would be difficult in the extreme; but the notion of lines of force, and of their divergence and convergence, guides Faraday without perplexity through all the intricacies of the question. After the completion of those researches, and in a paper forwarded to the Royal Society on the 22nd of October, 1851, he devotes himself to the formal development and illustration of his favourite idea. The paper bears the title "On lines of magnetic force, their definite character, and their distribution within a magnet and through space." A deep reflectiveness is the characteristic of this memoir. In his experiments, which are perfectly beautiful and profoundly suggestive, he takes but a secondary delight. His object is to illustrate the utility of his conception of lines of force. "The study of these lines," he says, "has at different times been greatly influential in leading me to various results which I think prove their utility as well as fertility."

Faraday for a long period used the lines of force merely as "a representative idea." He seemed for a time averse to going further in expression than the lines themselves, however much further he may have gone in idea. That he believed them to exist at all times round a magnet, and irrespective of the existence of magnetic matter, such as iron filings, external to the magnet, is certain. No doubt the space round every magnet presented itself to his imagination as traversed by loops of magnetic power, but he was chary in speaking of the physical substratum of those loops. Indeed it may be doubted whether the *physical theory* of lines of force presented itself with any distinctness to his own mind. The possible complicity of the luminiferous ether in magnetic phenomena was certainly in his thoughts. "How the magnetic force," he writes, "is transferred through bodies or through space we know not; whether the result is merely action at a distance, as in the case of gravity; or by some intermediate agency, as in the case of light, heat, the electric current, and (as I believe) static electric action. The idea of magnetic fluids, as applied by some, or of magnetic centres of action, does not include that of the latter kind of transmission, *but the idea of lines of force does.*" And he continues thus:—"I am more inclined to the notion that in the transmission of the [magnetic] force there is such an action [an intermediate agency] external to the magnet, than that the effects are merely attraction and repulsion at a distance. *Such an affection may be a function of the ether; for it is not at all unlikely that, if there be an* *ether, it should have other uses than simply the conveyance of radiations.*" When he speaks of the magnet in certain cases, "revolving amongst its own forces," he appears to have some conception of this kind in view.

A great part of the investigation completed in October, 1851, was taken up with the motions of wires round the poles of a magnet, and the converse. He carried an insulated wire along the axis of a bar magnet from its pole to its equator, where it issued from the magnet, and was bent up so as to connect its two ends. A complete circuit, no part of which was in contact with the magnet, was thus obtained. He found that when the magnet and the external wire were rotated together no current was produced; whereas, when *either* of them was rotated and the other left at rest currents were evolved. He then abandoned the axial wire, and allowed the magnet itself to take its place; the result was the same.* It was the *relative* motion of the magnet and the loop that was effectual in producing a current.

The lines of force have their roots in the magnet, and though they may expand into infinite space, they eventually return to the magnet. Now these lines may be intersected close to the magnet or at a distance from it. Faraday finds *distance* to be perfectly immaterial so long as the *number* of lines intersected is the same. For example, when the loop connecting the equator and the pole of his bar-magnet performs one complete revolution round the magnet, it is manifest that all the lines of force issuing from the magnet are *once* intersected. Now it matters not whether the loop be ten feet or ten inches in length, it matters not how it may be twisted and contorted, it matters not how near to the magnet or how distant from it the loop may be, one revolution always produces the same amount of current electricity, because in all these cases all the lines of force issuing from the magnet are *once* intersected and no more.

From the external portion of the circuit he passes in idea to the internal, and follows the lines of force into the body of the magnet itself. His conclusion is that there exists lines of force within the magnet of the same *nature* as those without. What is more, they are exactly equal in *amount* to those without. They have a relation in *direction* to those without; and in fact are continuations of them. . . . "Every line of force, therefore, at whatever distance it may be taken from the magnet, must be considered as a closed circuit, passing in some part of its course through the magnet, and having an equal amount of force in every part of its course."

All the results here described were obtained with *moving metals.* "But," he continues with profound sagacity, "mere motion would not generate a relation, which had not a foundation in the existence of some previous state; and therefore the *quiescent* metals must be in some relation to the active centre of force," that is to the magnet. He here touches the core of the whole question, and when we can state the condition into which the conducting wire is thrown *before* it is moved, we shall then be in a position to understand the physical constitution of the electric current generated by its motion.

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* In this form the experiment is identical with one made twenty years earlier. See page 210.

In this inquiry Faraday worked with steel magnets, the force of which varies with the distance from the magnet. He then sought a *uniform field* of magnetic force, and found it in space as affected by the magnetism of the earth. His next memoir, sent to the Royal Society on the 31st of December, 1851, is "On the employment of the Induced Magneto-electro Current as a test and measure of magnetic forces." He forms rectangles and rings, and by ingenious and simple devices collects the opposed currents which are developed in them by rotation across the terrestrial lines of magnetic force. He varies the shapes of his rectangles while preserving their areas constant, and finds that the constant area produces always the same amount of current per revolution. The current depends solely on the number of lines of force intersected, and when this number is kept constant the current remains constant too. Thus the lines of magnetic force are continually before his eyes, by their aid he colligates his facts, and through the inspirations derived from them he vastly expands the boundaries of our experimental knowledge. The beauty and exactitude of the results of this investigation are extraordinary. I cannot help thinking while I dwell upon them that this discovery of magneto-electricity is the greatest experimental result ever obtained by an investigator. It is the Mont Blanc of Faraday's own achievements. He always worked at great elevations, but a higher than this he never subsequently attained.

Unity and Convertibility of Natural Forces: Theory of the Electric Current.

The terms *unity* and *convertibility*, as applied to natural forces, are often employed in these investigations, many profound and beautiful thoughts respecting these subjects being expressed in Faraday's memoirs. Modern inquiry has however much augmented our knowledge of the relationship of natural forces, and it seems worth while to say a few words here, tending to clear up certain misconceptions which appear to exist among philosophic writers regarding this relationship.

The whole stock of *energy* or *working-power* in the world consists of *attractions*, *repulsions*, and *motions*. If the attractions and repulsions are so circumstanced as to be able to produce motion, they are sources of working-power, but not otherwise. Let us for the sake of simplicity confine our attention to the case of attraction. The attraction exerted between the earth and a body at a distance from the earth's surface is a source of working-power; because the body can be moved by the attraction, and in falling to the earth can perform work. When it rests upon the earth's surface it is *not* a source of power or energy, because it can fall no further. But though it has ceased to be a source of *energy*, the attraction of gravity still acts as a *force*, which holds the earth and weight together.

The same remarks apply to attracting atoms and molecules. As long as distance separates them, they can move across it in obedience to the attraction, and the motion thus produced may, by proper appliances, be caused to perform mechanical work. When, for example, two atoms of hydrogen unite with one of oxygen, to form water, the atoms are first drawn towards each other—they move, they clash, and then by virtue of their resiliency, they recoil and *quiver*. To this quivering motion we give the name of heat. Now this quivering motion is merely the redistribution of the motion produced by the chemical affinity; and this is the only sense in which chemical affinity can be said to be converted into heat. We must not imagine the chemical *attraction* destroyed, or converted into anything else. For the atoms when mutually clasped to form a molecule of water, are held together by the very attraction which first drew them towards each other. That which has really been expended is the *pull* exerted through the space by which the distance between the atoms has been diminished.

If this be understood it will be at once seen that *gravity* may in this sense be said to be convertible into heat; that it is in reality no more an outstanding and inconvertible agent, as it is sometimes stated to be, than chemical affinity. By the exertion of a certain pull through a certain space a body is caused to clash with a certain definite velocity against the earth. Heat is thereby developed, and this is the only sense in which gravity can be said to be converted into heat. In no case is the *force* which produces the motion annihilated or changed into anything else. The mutual *attraction* of the earth and weight exists when they are in contact as when they were separate; but the ability of that attraction to employ itself in the production of motion does *not* exist.

The transformation, in this case, is easily followed by the mind's eye. First, the weight as a whole is set in motion by the attraction of gravity. This motion of the mass is arrested by collision with the earth, being broken up into molecular tremors, to which we give the name of heat.

And when we reverse the process, and employ those tremors of heat to raise a weight, as is done through the intermediation of an elastic fluid in the steam-engine, a certain definite portion of the molecular motion is destroyed in raising the weight. In this sense, and this sense only, can the heat be said to be converted into gravity, or more correctly, into potential energy of gravity. It is not that the destruction of the heat has created any *new* attraction, but simply that the old attraction has now a power conferred upon it, of exerting a certain definite pull in the interval between the starting-point of the falling weight and its collision with the earth.

So also as regards magnetic attraction: when a sphere of iron placed at some distance from a magnet rushes towards the magnet, and has its motion stopped by collision, an effect mechanically the same as that produced by the attraction of gravity occurs. The magnetic attraction generates the motion of the mass, and the stoppage of that motion produces heat. In this sense, and in this sense only, is there a transformation of magnetic work into heat. And if by the mechanical action of heat brought to bear by means of a suitable machine, the sphere be torn from the magnet and again placed at a distance, a power of exerting a pull through that distance, and producing a new motion of the sphere, is thereby conferred upon the magnet; in this sense, and in this sense only, is the heat converted into magnetic potential energy.

When, therefore, writers on the conservation of energy speak of tensions being "consumed" and "generated," they do not mean thereby that old attractions have been annihilated and new ones brought into existence, but that, in the one case, the power of the attraction to produce motion has been diminished by the shortening of the distance between the attracting bodies, and that in the other case the power of producing motion has been augmented by the increase of the distance. These remarks apply to all bodies, whether they be sensible masses or molecules.

Of the inner quality that enables matter to attract matter we know nothing; and the law of conservation makes no statement regarding that quality. It takes the facts of attraction as they stand, and affirms only the constancy of *working-power*. That power may exist in the form of MOTION; or it may exist in the form of FORCE, *with distance to act through*. The former is dynamic energy, the latter is potential energy, the constancy of the sum of both being affirmed by the law of conservation. The *convertibility* of natural forces consists solely in transformations of dynamic into potential, and of potential into dynamic energy, which are incessantly going on. In no other sense has the convertibility of force, at present, any scientific meaning.

By the contraction of a muscle a man lifts a weight from the earth. But the muscle can contract only through the oxidation of its own tissue or of the blood passing through it. Molecular motion is thus converted into mechanical motion. Supposing the muscle to contract without raising the weight, oxidation would also occur, but the whole of the heat produced by this oxidation would be liberated *in the muscle itself*. Not so when it performs external work; to do that work a certain definite portion of the heat of oxidation must be expended. It is so expended in pulling the weight away from the earth. If the weight be permitted to fall, the heat generated by its collision with the earth would exactly make up for that lacking in the muscle during the lifting of the weight. In the case here supposed, we have a conversion of molecular muscular action into potential energy of gravity; and a conversion of that potential energy into heat; the heat, however, appearing at a distance from its real origin in the muscle. The whole process consists of a transference of molecular motion from the muscle to the weight, and gravitating force is the mere go-between, by means of which the transference is effected.

These considerations will help to clear our way to the conception of the transformations which occur when a wire is moved across the lines of force in a magnetic field. In this case it is commonly said we have a conversion of magnetism into electricity. But let us endeavour to understand what really occurs. For the sake of simplicity, and with a view to its translation into a different one subsequently, let us adopt for a moment the provisional conception of a mixed fluid in the wire, composed of positive and negative electricities in equal quantities and therefore perfectly neutralizing each other when the wire is still. By the motion of the wire, say with the hand, towards the magnet, what the Germans call a *Scheidungs-Kraft*—a separating force—is brought into play. This force tears the mixed fluids asunder, and drives them in two currents, the one positive and the other negative, in two opposite directions through the wire. The presence of these currents evokes a force of *repulsion* between the magnet and the wire; and to cause the one to approach the other, this repulsion must be overcome. The overcoming of this repulsion is, in fact, the work done in separating and impelling the two electricities. When the wire is moved away from the magnet, a *Scheidungs-Kraft*, or separating force, also comes into play; but now it is an *attraction* that has to be surmounted. In surmounting it, currents are developed in directions opposed to the former; positive takes the place of negative, and negative the place of positive; the overcoming of the attraction being the work done in separating and impelling the two electricities.

The mechanical action occurring here is different from that occurring where a sphere of soft iron is withdrawn from a magnet, and again attracted. In this case muscular force is expended during the act of separation; but the attraction of the magnet effects the reunion. In the case of the moving wire, also we overcame a resistance in separating it from the magnet, and thus far the action is mechanically the same as the separation of the sphere of iron. But after the wire has ceased moving, the attraction ceases; and so far from any action occurring similar to that, which draws the iron sphere back to the magnet, we have to overcome a repulsion to bring them together.

There is no potential energy conferred either by the removal or by the approach of the wire, and the only power really transformed or converted, in the experiment, is muscular power. Nothing that could in strictness be called a conversion of magnetism into electricity occurs. The muscular oxidation that moves the wire fails to produce *within the muscle* its due amount of heat, a portion of that heat equivalent to the resistance overcome, appearing in the moving wire instead.

Is this effect an attraction and a repulsion at a distance? If so, why should both cease when the wire ceases to move? In fact, the deportment of the wire resembles far more that of a body moving *in* *a resisting medium* than anything else; the resistance ceasing when the motion is suspended. Let us imagine the case of a liquid so mobile that the hand may be passed through it to and fro, without encountering any sensible resistance. It resembles the motion of a conductor in the unexcited field of an electro-magnet. Now let us suppose a body placed in the liquid, or acting on it, which confers upon it the property of *viscosity;* the hand would no longer move freely. During its motion, but then only, resistance would be encountered and overcome. Here we have rudely represented the case of the excited magnetic field, and the result in both cases would be substantially the same. In both cases heat would, in the end, be generated outside of the muscle, its amount being exactly equivalent to the resistance overcome.

Let us push the analogy a little further; suppose in the case of the fluid rendered viscous, as assumed a moment ago, the viscosity not to be so great as to prevent the formation of *ripples* when the hand is passed through the liquid. Then the motion of the hand, before its final conversion into heat, would exist for a time as wave-motion, which on subsiding would generate its due equivalent of heat. This intermediate stage, in the case of our moving wire, is represented by the period *during which the electric current is flowing through it;* but that current, like the ripples of our liquid, soon subsides, being, like them, converted into heat.

Do these words shadow forth anything like the reality? Such speculations cannot be injurious if they are enunciated without dogmatism. I do confess that ideas such as these here indicated exercise a strong fascination on my mind. Is then the magnetic field really viscous, and if so, what substance exists in it and the wire to produce the viscosity? Let us first look at the proved effects, and afterwards turn our thoughts back upon their cause. When the wire approaches the magnet, an action is evoked within it, which travels through it with a velocity comparable to that of light. One substance only in the universe has been hitherto proved competent to transmit power at this velocity; the luminiferous ether. Not only its rapidity of progression but its ability to produce the motion of light and heat, indicates that the electric current is also motion.* Further, there is a striking resemblance between the action of good and bad conductors as regards electricity, and the action of diathermanous and adiathermanous bodies as regards radiant heat. The good conductor is diathermanous to the electric current; it allows free transmission without the development of heat. The bad conductor is adiathermanous to the electric current, and hence the passage of the latter is accompanied by the development of heat. I am strongly inclined to hold the electric current, pure and simple, to be a motion of the ether alone; good conductors being so constituted that the motion may be propagated through their ether without sensible transfer to their atoms, while in the case of bad conductors this transfer is effected, the transferred motion appearing as heat.*

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* Mr. Clerk Maxwell has recently published an exceedingly important investigation connected with this question. Even in the non-mathematical portions of the memoirs of Mr. Maxwell, the admirable spirit of his philosophy is sufficiently revealed. As regards the employment of scientific imagery, I hardly know his equal in power of conception and clearness of definition.

I do not know whether Faraday would have subscribed to what is here written; probably his habitual caution would have prevented him from committing himself to anything so definite. But some such idea filled his mind and coloured his language through all the later years of his life. I dare not say that he has been always successful in the treatment of these theoretic notions. In his speculations he mixes together light and darkness in varying proportions, and carries us along with him through strong alternations of both. It is impossible to say how a certain amount of mathematical training would have affected his work. We cannot say what its influence would have been upon that force of inspiration that urged him on; whether it would have daunted him, and prevented him from driving his adits into places, where no theory pointed to a lode. If so, then we may rejoice that this strong delver at the mine of natural knowledge was left free to wield his mattock in his own way. It must be admitted, that Faraday's purely speculative writings often lack that precision which the mathematical habit of thought confers. Still across them flash frequent gleams of prescient wisdom which will excite admiration throughout all time, while the facts, relations, principles, and laws which his experiments have established are sure to form the body of grand theories yet to come.

SUMMARY.

When from an Alpine height the eye of the climber ranges over the mountains, he finds that for the most part they resolve themselves into distinct groups, each consisting of a dominant mass surrounded by peaks of lesser elevation. The power which lifted the mightier eminences, in nearly all cases lifted others to an almost equal height. And so it is with the discoveries of Faraday. As a general rule, the dominant result does not stand alone, but forms the culminating point of a vast and varied mass of inquiry. In this way, round about his great discovery of Magneto-electric Induction, other weighty labours group themselves. His investigations on the Extra Current; on the Polar and other Condition of Diamagnetic Bodies; on Lines of Magnetic Force, their definite character and distribution; on the employment of the Induced Magneto-electric Current as a measure and test of Magnetic Action; on the Revulsive Phenomena of the magnetic field, are all, notwithstanding the diversity of title, researches in the domain of magneto-electric induction.

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* One important difference, of course, exists between the effect of motion in the magnetic field, and motion in a resisting medium. In the former case the heat is generated *in the moving conductor*, in the latter it is in part generated *in the medium*.

Faraday's second group of researches and discoveries embrace the chemical phenomena of the current. The dominant result here is the great law of definite Electro-chemical Decomposition, around which are massed various researches on Electro-chemical Conduction, and on Electrolysis both with the Machine and with the Pile. To this group also belong his analysis of the Contact Theory, his inquiries as to the Source of Voltaic Electricity, and his final development of the Chemical Theory of the pile.

His third great discovery is the Magnetization of Light, which I should liken to the Weisshorn among mountains—high, beautiful, and alone.

The dominant result of his fourth group of researches is the discovery of Diamagnetism, announced in his memoir as the Magnetic Condition of all Matter, round which are grouped his inquiries on the Magnetism of Flame and Gases; on Magne-crystallic action, and on Atmospheric Magnetism, in its relations to the annual and diurnal variation of the needle, the full significance of which is still to be shown.

These are Faraday's most massive discoveries, and upon them his fame must mainly rest. But even without them, sufficient would remain to secure for him a high and lasting scientific reputation. We should still have his researches on the Liquefaction of Gases; on Frictional Electricity; on the Electricity of the Gymnotus; on the source of power in the Hydro-electric machine, the two last investigations being untouched in the foregoing memoir; on Electro-magnetic Rotations; on Regelation; all his more purely Chemical Researches, including his discovery of Benzol. Besides these he published a multitude of minor papers, most of which, in some way or other, illustrate his genius. I have made no allusion to his power and sweetness as a lecturer. Taking him for all in all, I think it will be conceded that Michael Faraday was the greatest experimental philosopher the world has ever seen; and I will add the opinion, that the progress of future research will tend not to dim or to diminish, but to enhance and glorify the labours of this mighty investigator.

ILLUSTRATIONS OF CHARACTER.

Thus far I have confined myself to topics mainly interesting to the man of science, endeavouring, however, to treat them in a manner unrepellent to the general reader who might wish to obtain a notion of Faraday as a worker. On others will fall the duty of presenting to the world a picture of the man. But I know you will permit me to add to the foregoing analysis a few personal reminiscences and remarks, tending to connect Faraday with a wider world than that of science—namely, with the general human heart.

One word in reference to his married life, in addition to what has been already said, may find a place here. As in the former case, Faraday shall be his own spokesman. The following paragraph, though written in the third person, is from his hand :—"On the 12th of June, 18~~4~~1, he married, an event which more than any other contributed to his earthly happiness and healthful state of mind. The union has continued for twenty-eight years and has in no wise changed, except in the depth and strength of its character." 2

Faraday's immediate forefathers lived in a little place called Clapham Wood Hall, in Yorkshire. Here dwelt Robert Faraday and Elizabeth his wife, who had ten children, one of them, James Faraday, born in 1761, being father to the philosopher. A family tradition exists that the Faradays came originally from Ireland. Faraday himself has more than once expressed to me his belief that his blood was in part Celtic, but how much of it was so, or when the infusion took place, he was unable to say. He could imitate the Irish brogue, and his wonderful vivacity may have been in part due to his extraction. But there were other qualities which we should hardly think of deriving from Ireland. The most prominent of these was his sense of order, which ran like a luminous beam through all the transactions of his life. The most entangled and complicated matters fell into harmony in his hands. His mode of keeping accounts excited the admiration of the managing board of this Institution. And his science was similarly ordered. In his Experimental Researches, he numbered every paragraph, and welded their various parts together by incessant reference. His private notes of the Experimental Researches, which are happily preserved, are similarly numbered: their last paragraph bears the figure 16,041. His working qualities, moreover, showed the tenacity of the Teuton. His nature was impulsive, but there was a force behind the impulse which did not permit it to retreat. If in his warm moments he formed a resolution, in his cool ones he made that resolution good. Thus his fire was that of a solid combustible, not that of a gas, which blazes suddenly, and dies as suddenly away.

And here I must claim your tolerance for the limits by which I am confined. No materials for a life of Faraday are in my hands, and what I have now to say, has arisen almost wholly out of our close personal relationship.

Letters of his, covering a period of sixteen years, are before me, each one of which contains some characteristic utterance ;—strong, yet delicate in counsel, joyful in encouragement, and warm in affection. References which would be pleasant to such of them as still live are made to Humboldt, Biot, Dumas, Chevreul, Magnus, and Arago. Accident brought these names prominently forward; but many others would be required to complete his list of continental friends. He prized the love and sympathy of men—prized it almost more than the renown which his science brought him. Nearly a dozen years ago it fell to my lot to write a review of his "Experimental Researches" for the 'Philosophical Magazine.' After he had read it, he took me by the hand, and said, "Tyndall, the sweetest reward of my work is the sympathy and goodwill which it has caused to flow in upon me from all quarters of the world." Among his letters I find little sparks of kindness, precious to no one but myself, but more precious to me than all. He would peep into the laboratory when he thought me weary, and take me upstairs with him to rest. And if I happened to be absent he would leave a little note for me, couched in this or some other similar form:—"Dear Tyndall,—I was looking for you, because we were at tea—we have not yet done—will you come up?" I frequently shared his early dinner; almost always, in fact, while my lectures were going on. There was no trace of asceticism in his nature. He preferred the meat and wine of life to its locusts and wild honey. Never once during an intimacy of fifteen years did he mention religion to me, save when I drew him on to the subject. He then spoke to me without hesitation or reluctance; not with any apparent desire to "improve the occasion," but to give me such information as I sought. He believed the human heart to be swayed by a power to which science or logic opened no approach, and right or wrong, this faith, held in perfect tolerance of the faiths of others, strengthened and beautified his life.

From the letters just referred to, I will select three for publication here. I choose the first, because it contains a passage revealing the feelings with which Faraday regarded his vocation, and also because it contains an allusion which will give pleasure to a friend.

"~~Royal Institution~~,

"Ventnor, Isle of Wight, 28*th June*, 1854.

"My dear Tyndall,

"You see by the top of this letter how much habit prevails over me; I have just read yours from thence, and yet I think myself there. However, I have left its Science in very good keeping, and I am glad to learn that you are at experiment once more. But how is the health? Not well, I fear. I wish you would get yourself strong first and work afterwards. As for the fruits, I am sure they will be good, for though I sometimes despond as regards myself, I do not as regards you. You are young, I am old. . . . . . . *But then our subjects are so glorious, that to work at them rejoices and encourages the feeblest; delights and enchants the strongest.*

"I have not yet seen anything from Magnus. Thoughts of him always delight me. We shall look at his black sulphur together. I heard from Schonbein the other day. He tells me that Liebig is full of ozone, *i.e.* of allotropic oxygen.

"Good-bye for the present.
"Ever, my dear Tyndall,
"Yours truly,
"M. Faraday."

The contemplation of Nature, and his own relation to her, produced in Faraday a kind of spiritual exaltation which makes itself manifest here. His religious feeling and his philosophy could not be kept apart; there was an habitual overflow of the one into the other.

Whether he or another was its exponent, he appeared to take equal delight in science. A good experiment would make him almost dance with delight. In November, 1850, he wrote to me thus:—"I hope some day to take up the point respecting the magnetism of associated particles. In the mean time I rejoice at every addition to the facts and reasoning connected with the subject. When science is a republic, then it gains; and though I am no republican in other matters, I am in that." All his letters illustrate this catholicity of feeling. Ten years ago, when going down to Brighton, he carried with him a little paper I had just completed, and afterwards wrote to me. His letter is a mere sample of the sympathy which he always showed to me and my work.

"BRIGHTON, *9th Dec.*, 1857.

"MY DEAR TYNDALL,

"I cannot resist the pleasure of saying how very much I have enjoyed your paper. Every part has given me delight. It goes on from point to point beautifully. You will find many pencil marks, for I made them as I read. I let them stand, for though many of them receive their answer as the story proceeds, yet they show how the wording impresses a mind fresh to the subject, and perhaps here and there you may like to alter it slightly, if you wish the full idea, *i.e.* not an inaccurate one, to be suggested at first; and yet, after all, I believe it is not your exposition, but the natural jumping to a conclusion that affects or has affected my pencil.

"We return on Friday, when I will return you the paper.

"Ever truly yours,

"M. FARADAY."

The third letter will come in its proper place towards the end.

While once conversing with Faraday on science in its relations to commerce and litigation, he said to me, that at a certain period of his career, he was forced definitely to ask himself, and finally to decide, whether he should make wealth or science the pursuit of his life. He could not serve both masters, and he was therefore compelled to choose between them. After the discovery of magneto-electricity his fame was so noised abroad, that the commercial world would hardly have considered any remuneration too high for the aid of abilities like his. Even before he became so famous, he had done a little "professional business." This was the phrase he applied to his purely commercial work. His friend, Richard Phillips, for example, had induced him to undertake a number of analyses, which produced, in the year 1830, an addition to his income of more than a thousand pounds; and in 1831, a still greater addition. He had only to will it to raise in 1832 his professional business income to £5000 a year. Indeed, this is a wholly insufficient estimate of what he might, with ease, have realized annually during the last thirty years of his life.

While re-studying the Experimental Researches with reference to the present memoir, the conversation with Faraday here alluded to, came to my recollection, and I sought to ascertain the period when the question, "wealth or science," had presented itself with such emphasis to his mind. I fixed upon the year 1831 or 1832, for it seemed beyond the range of human power to pursue science as he had done during the subsequent years, and to pursue commercial work at the same time. To test this conclusion I asked permission to see his accounts, and on my own responsibility, I will state the result. In 1832, his professional business-income, instead of rising to £5000, or more, fell from £1090 4*s.* to £155 9*s.* From this it fell with slight oscillations to £92 in 1837, and to zero in 1838. Between 1839 and 1845, it never, except in one instance, exceeded £22; being for the most part much under this. The exceptional year referred to was that in which he and Sir Charles Lyell were engaged by Government to write a report on the Haswell Colliery explosion, and then his business-income rose to £112. From the end of 1845 to the day of his death, Faraday's annual professional business-income was exactly zero. Taking the duration of his life into account, this son of a blacksmith, and apprentice to a bookbinder, had to decide between a fortune of £150,000 on the one side, and his undowered science on the other. He chose the latter, and died a poor man. But his was the glory of holding aloft among the nations the scientific fame of England for a period of forty years.

The outward and visible signs of fame were also of less account to him than to most men. He had been loaded with scientific honours from all parts of the world. Without, I imagine, a dissentient voice, he was regarded as the prince of the physical investigators of the present age. The highest scientific position in this country he had, however, never filled. When the late excellent and lamented Lord Wrottesley resigned the presidency of the Royal Society, a deputation from the council, consisting of his lordship, Mr. Grove, and Mr. Gassiot, waited upon Faraday, to urge him to accept the president's chair. All that argument or friendly persuasion could do was done to induce him to yield to the wishes of the council, which was also the unanimous wish of scientific men. A knowledge of the quickness of his own nature had induced in Faraday the habit of requiring an interval of reflection, before he decided upon any question of importance. In the present instance he followed his usual habit, and begged for a little time. On the following morning I went up to his room, and said on entering, that I had come to him with some anxiety of mind. He demanded its cause, and I responded, "Lest you should have decided against the wishes of the deputation that waited on you yesterday." "You would not urge me to undertake this responsibility," he said. "I not only urge you," was my reply, "but I consider it your bounden duty to accept it." He spoke of the labour that it would involve; urged that it was not in his nature to take things easy; and that if he became president, he would surely have to stir many new questions, and agitate for some changes. I said that in such cases he would find himself supported by the youth and strength of the Royal Society. This, however, did not seem to satisfy him. Mrs. Faraday came into the room, and he appealed to her. Her decision was adverse, and I deprecated her decision. "Tyndall," he said at length, "I must remain plain Michael Faraday to the last; and let me now tell you, that if I accepted the honour which the Royal Society desires to confer upon me, I would not answer for the integrity of my intellect for a single year." I urged him no more, and Lord Wrottesley had a most worthy successor in Sir Benjamin Brodie.

After the death of the Duke of Northumberland, our Board of Managers wished to see Mr. Faraday finish his career as President of the Institution, which he had entered on weekly wages more than half a century before. But he would have nothing to do with the presidency. He wished for rest, and the reverent affection of his friends was to him infinitely more precious than all the honours of official life.

The first requisite of the intellectual life of Faraday was the independence of his mind; and though prompt to urge obedience where obedience was due, with every right assertion of manhood he intensely sympathised. Even rashness on the side of honour found from him ready forgiveness, if not open applause. The wisdom of years, tempered by a character of this kind, rendered his counsel peculiarly precious to men sensitive like himself. I often sought that counsel, and, with your permission, will illustrate its character by one or two typical instances.

In 1855, I was appointed examiner under the Council for Military Education. At that time, as indeed now, I entertained strong convictions as to the enormous utility of physical science to officers of artillery and engineers, and whenever opportunity offered, I expressed this conviction without reserve. I did not think the recognition, though considerable, accorded to physical science in those examinations, at all proportionate to its importance; and this probably rendered me more jealous than I otherwise should have been of its claims.

In Trinity College, Dublin, a school had been organized with reference to the Woolwich examinations, and a large number of exceedingly well instructed young gentlemen were sent over from Dublin, to compete for appointments in the artillery and engineers. The result of one examination was particularly satisfactory to me; indeed the marks obtained appeared so eloquent, that I forbore saying a word about them. My colleagues, however, followed the usual custom of sending in brief reports with their returns of marks. After the results were published, a leading article appeared in 'The Times,' in which the reports were largely quoted; praise being bestowed on all the candidates, except the excellent young fellows who had passed through my hands.

A letter from Trinity College drew my attention to this article, bitterly complaining, that whereas the marks proved them to be the best of all, the science candidates were wholly ignored. I tried to set matters right by publishing, on my own responsibility, a letter in 'The Times.' The act I knew could not bear justification from the War Office point of view; and I expected and risked the displeasure of my superiors. The merited reprimand promptly came. "Highly as the Secretary of State for War might value the expression of Professor Tyndall's opinion, he begged to say that an examiner appointed by His Royal Highness the Commander-in-Chief had no right to appear in the public papers as Professor Tyndall has done, without the sanction of the War Office." Nothing could be more just than this reproof, but I did not like to rest under it. I wrote a reply, and previous to sending it, took it up to Faraday. We sat together before his fire, and he looked very earnest, as he rubbed his hands and pondered. The following conversation then passed between us:—

F. You certainly have received a reprimand, Tyndall; but the matter is over, and if you wish to accept the reproof, you will hear no more about it.

T. But I do not wish to accept it.

F. Then you know what the consequence of sending that letter will be?

T. I do.

F. They will dismiss you.

T. I know it.

F. Then send the letter!

The letter was firm, but respectful; it acknowledged the justice of the censure, but expressed neither repentance nor regret. Faraday, in his gracious way, slightly altered a sentence or two to make it more respectful still. It was duly sent, and on the following day I entered the Institution with the conviction that my dismissal was there before me. Weeks, however, passed. At length the well-known envelope appeared, and I broke the seal, not doubting the contents. They were very different from what I expected. "The Secretary of State for War has received Professor Tyndall's letter, and *deems the explanation therein given perfectly satisfactory.*" I have often wished for an opportunity of publicly acknowledging this liberal treatment, proving, as it did, that Lord Panmure could discern and make allowance for a good intention, though it involved an offence against routine. For many years subsequently it was my privilege to act under that excellent body, the Council for Military Education.

On another occasion of this kind, having encouraged me in a somewhat hardy resolution I had formed, Faraday backed his encouragement by an illustration drawn from his own life. The subject will interest you, and it is so sure to be talked about in the world, that no avoidable harm can arise from its introduction here.

In the year 1835, Sir Robert Peel wished to offer Faraday a pension, but that great statesman quitted office before he was able to realize his wish. The Minister who founded these pensions intended them, I believe, to be marks of honour, which even proud men might accept without compromise of independence. When, however, the intimation first reached Faraday, in an unofficial way, he wrote a letter announcing his determination to decline the pension; and stating that he was quite competent to earn his livelihood himself. That letter still exists, but it was never sent, Faraday's repugnance having been overruled by his friends. When Lord Melbourne came into office, he desired to see Faraday; and probably in utter ignorance of the man—for, unhappily for them and us, Ministers of State in England are only too often ignorant of great Englishmen—his lordship said something that must have deeply displeased his visitor. The whole circumstances were once communicated to me, but I have forgotten the details. The term "humbug," I think, was incautiously employed by his lordship, and other expressions were used of a similar kind. Faraday quitted the Minister with his own resolves, and that evening he left his card and a short and decisive note at the residence of Lord Melbourne, stating that he had manifestly mistaken his lordship's intention of honouring science in his person, and declining to have anything whatever to do with the proposed pension. The good-humoured nobleman at first considered the matter a capital joke; but he was afterwards led to look at it more seriously. An excellent lady, who was a friend both to Faraday and the Minister, tried to arrange matters between them; but she found Faraday very difficult to move from the position he had assumed. After many fruitless efforts, she at length begged of him to state what he would require of Lord Melbourne to induce him to change his mind. He replied, "I should require from his lordship what I have no right or reason to expect that he would grant:—a written apology for the words he permitted himself to use to me." The required apology came, frank and full, creditable, I thought, alike to the Prime Minister and the philosopher.

Considering the enormous strain imposed on Faraday's intellect, the boy-like buoyancy even of his later years was astonishing. He was often prostrate, but he had immense resiliency, which he brought into action by getting away from London whenever his health failed. I have already indicated the thoughts which filled his mind during the evening of his life. He brooded on magnetic media and lines of force; and the great object of the last investigation he ever undertook was the decision of the question whether magnetic force requires *time* for its propagation. How he proposed to attack this subject we may never know. But he has left some beautiful apparatus behind; delicate wheels and pinions, and associated mirrors, which were to have been employed in the investigation. The mere conception of such an inquiry is an illustration of his strength and hopefulness, and it is impossible to say to what results it might have led him. But the work was too heavy for his tired brain. It was long before he could bring himself to relinquish it, and during this struggle he often suffered from fatigue of mind. It was at this period, and before he resigned himself to the repose which marked the last two years of his life, that he wrote to me the following letter,—one of many priceless letters now before me,—which reveals more than anything another pen could express, the state of his mind at the time. I was sometimes censured in his presence for my doings in the Alps, but his constant reply was, "let him alone, he knows how to take care of himself." In this letter, anxiety on this score reveals itself, for the first time.

"HAMPTON COURT, 1*st* *Aug.*, 1864.

"MY DEAR TYNDALL,

"I do not know whether my letter will catch you, but I will risk it though feeling very unfit to communicate with a man whose life is as vivid and active as yours; but the receipt of your kind letter makes me to know that though I forget, I am not forgotten, and though I am not able to remember at the end of a line what was said at the beginning of it, the imperfect marks will convey to you some sense of what I long to say. We had heard of your illness through Miss Moore, and I was therefore very glad to learn that you are now quite well; do not run too many risks, or make your happiness depend too much upon dangers, or the hunting of them. Sometimes the very thinking of you and what you may be about wearies *me* with fears, and then the cogitations pause and change, but without giving me rest. I know that much of this depends upon my own worn-out nature, and I do not know why I write it, save that when I write to you I cannot help thinking it, and the thoughts stand in the way of other matter.

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"See what a strange desultory epistle I am writing to you, and yet I feel so weary that I long to leave my desk and go to the couch.

"My dear wife and Jane desire their kindest remembrances, I hear them in the next room; . . . . I forget—but not you, my dear Tyndall, for I am

"Ever yours,
"M. FARADAY."

This weariness subsided when he relinquished his work, and I have a cheerful letter from him, written in the autumn of 1865. But towards the close of that year he had an attack of illness, from which he never completely rallied. He continued to attend the Friday Evening Meetings, but the advance of infirmity was apparent to us all. Complete rest became finally essential to him, and he ceased to appear amongst us. There was no pain in his decline to trouble the memory of those who loved him. Slowly and peacefully he sank towards his final rest, and when it came, his death was a falling asleep. In the fulness of his honours and of his age he quitted us; the good fight fought, the work of duty—shall I not say of glory—done. The "Jane" referred to in the foregoing letter is Faraday's niece, Miss Jane Barnard, who with an affection raised almost to religious devotion, watched him and tended him to the end.

I saw Mr. Faraday for the first time on my return from Marburg in 1850. I came to the Royal Institution, and sent up my card with a copy of the paper which Knoblauch and myself had just completed. He came down and conversed with me for half-an-hour. I could not fail to remark the wonderful play of intellect and kindly feeling exhibited by his countenance. When he was in good health the question of his age would never occur to you. In the light and laughter of his eyes you never thought of his gray hairs. He was then on the point of publishing one of his papers on magne-crystallic action, and he had time to refer in a flattering note to the memoir I placed in his hands. I returned to Germany, worked there for nearly another year, and in June, 1851, came back finally from Berlin to England. Then, for the first time, and on my way to the meeting of the British Association, at Ipswich, I met a man who has since made his mark upon the intellect of his time, who has long been, and who by the strong law of natural affinity must continue to be a brother to me. We were both without definite outlook at the time, needing proper work, and only anxious to have it to perform. The chairs of Natural History and of Physics being advertised as vacant in the University of Toronto, we applied for them, he for the one, I for the other; but, possibly guided by a prophetic instinct, the University authorities declined having anything to do with either of us. If I remember aright, we were equally unlucky elsewhere.

One of Faraday's earliest letters to me had reference to this Toronto business, which he thought it unwise in me to neglect. But Toronto had its own notions, and in 1853, at the instance of Dr. Bence Jones, and on the recommendation of Faraday himself, a chair of physics at the Royal Institution was offered to me. I was tempted at the same time to go elsewhere, but a strong attraction drew me to his side. Let me say that it was mainly his and other friendships, precious to me beyond all expression, that caused me to value my position here more highly than any other that could be offered to me in this land. Nor is it for its honour, though surely that is great, but for the strong personal ties that bind me to it, that I now chiefly prize this place. You might not credit me were I to tell you how lightly I value the honour of being Faraday's successor compared with the honour of having been Faraday's friend. His friendship was an energy and inspiration; his "mantle" is a burden almost too heavy to be borne.

Sometimes during the last year of his life, by the permission or invitation of Mrs. Faraday, I went up to his rooms to see him. The deep radiance, which in his time of strength flashed with such extraordinary power from his countenance, had subsided to a calm and kindly light, by which my latest memory of him is warmed and illuminated. I knelt one day beside him on the carpet and placed my hand upon his knee, he stroked it affectionately, smiled, and murmured, in a low, soft voice, the last words that I remember as having been spoken to me by Michael Faraday.

It was my wish and aspiration to play the part of Schiller to this Goëthe; and he was at times so strong and joyful,—his body so active, and his intellect so clear,—as to suggest to me the thought that he, like Goëthe, would see the younger man laid low. Destiny ruled otherwise, and now he is but a memory to us all. Surely no memory could be more beautiful. He was equally rich in mind and heart. The fairest traits of a character sketched by Paul, found in him perfect illustration. For he was "blameless, vigilant, sober, of good behaviour, apt to teach, not given to filthy lucre." He had not a trace of worldly ambition, he declared his duty to his Sovereign by going to the levee once a year, but beyond this he never sought contact with the great. The life of his spirit and of his intellect were so full, that the things which men most strive after were absolutely indifferent to him. "Give me health and a day," says the brave Emerson, "and I will make the pomp of emperors ridiculous." In an eminent degree Faraday could say the same. What to him was the splendour of a palace compared with a thunderstorm upon Brighton Downs?—what among all the appliances of royalty to compare with the setting sun? I refer to a thunderstorm and a sunset, because these things excited a kind of ecstasy in his mind, and to a mind open to such ecstasy the pomps and pleasures of the world are usually of small account. Nature, not education, rendered Faraday strong and refined. A favourite experiment of his own was representative of himself. He loved to show that water in crystallizing excluded all foreign ingredients, however intimately they might be mixed with it. Out of acids, alkalis, or saline solutions, the crystal came sweet and pure. By some such natural process in the formation of this man, beauty and nobleness coalesced, to the exclusion of everything vulgar and low. He did not learn his gentleness in the world, for he withdrew himself from its culture; and still this land of England contained no truer gentleman than he. Not half his greatness was incorporated in his science, for science could not reveal the bravery and delicacy of his heart.

But it is time that I should end these weak words, and lay my poor garland on the grave of this

"Just and faithful knight of God."

[J. T.]

Royal Institution of Great Britain.

WEEKLY EVENING MEETING,

Friday, January 31, 1868.

John Peter Gassiot, Esq. F.R.S. Vice-President, in the Chair.

The Rev. F. W. Farrar, M.A. F.R.S.

On Public School Education.

So far from being half finished, the real battle for educational reform has hardly begun. Latin and Greek still continue to be the all but exclusive staple of our education, and though a classical training conducted on wise principles and with reasonable methods is of the highest value, yet the many and serious evils which our present system of it involves have been resolutely ignored. The yoke of the Greek and Latin languages has been made needlessly humiliating and needlessly heavy: taken alone, it is doubtful whether they furnish the best mental discipline for any, but certain that they do not furnish even a good discipline for all; and they remain to this day entrenched behind a mountain-heap of fallacies, of which no small number ought to have been banished ignominiously to the region of the most exploded errors.

But even if all the arguments in favour of a purely classical education were as tenable as half of them are fantastic, our present system of it is a complete and disastrous failure; and that it is so may be largely demonstrated alike by the criticism of its enemies, and the repeated confessions of its friends. And if this be so, it is our clear duty as Englishmen, as patriots, nay, even as mere honest men, to make that system more worthy of its immense importance and of our national prestige.

It would be easy to adduce the testimony of many eminent scholars to the humiliating ignorance on a multitude of subjects which has been the inevitable result of years exclusively devoted to two dead languages; but the case of the vast majority of boys who do not become scholars in any sense of the word is still more to be deplored. People read glowing estimates of Greek and Roman literature, and take them for a defence of classical education. There could not be a greater delusion. Hundreds of boys after years of expensive training know far less, and have far less real culture, than their sisters who have only had the modest aid of a single governess. They know nothing, except perhaps the merest and most useless smattering of modern languages, of history, of mathematics, or of science; and if they want to pass in a competitive examination they must be hastily sent to some professional tutor to have their minds crammed for the purpose like a hurriedly-packed portmanteau. The parent comforts himself that their education has been purely literary; but this purely literary education has somehow left them with a bad handwriting, with very vague notions of spelling, and with minds that can find satisfaction in nothing higher than sensational novels. The parents take refuge in the belief that at least their boys know Latin and Greek; but this is infinitely far from being the case; of the vocabulary they possibly know a little, but of the grammar less, and of the literature nothing at all. It is certain that they will never open a Greek or Latin book again; and for these paltry and miserable results they have all but sacrificed the happy seed-time during which so much might have been accomplished. The evidence of these facts, evidence given by most friendly witnesses, stands in the Commission Reports undisputed and indisputable. It shows that for many a boy the years of school-life are wasted. It is as though he stood in the midst of a boundless plain, waving on every side with golden corn, in the midst of which, trained to despise the sickle as vulgar, and the harvest as utilitarian, he had been taught for years to occupy his time in plucking a few petals of the scarlet poppies, which are crumpled as he gathers them, and which grow rank and flaccid even during the few moments that he holds them in his hand.

The question then is, not whether education is to be literary or scientific, but whether it is to be scientific or *nil;* the struggle is not between science and literature, but between something and nothing, between science and nescience, between intellectual culture and its almost total absence. It is a melancholy fact, but it is a fact, that at present we struggle almost in vain against the two potent elements of intellectual progress—extravagant athleticism on the one hand, and promiscuous sensation-reading on the other, of which the one poisons and effeminates the mind, the other often tasks and overstrains the body; the one absorbs the strenuous ambition which might have been devoted to nobler objects, the other wastes the inestimable leisure which might else have been rich in mental and moral benefits for our country and for mankind.

What, then, is to be done? Some would say, "substitute for your simulacrum of Greek and Latin, an education which, if less pretentious, shall at least be real and sound, in modern languages, in literature, and, above all, in science." But it would be a great disaster if there were supposed to be any antagonism between science and literature; both are indispensable; each of them is an absolutely essential factor in an education pretending to be liberal. Yet our present system is neither literary nor scientific, whereas it is perfectly certain that it might be both.

If, however, schoolmasters assert and assume that a boy cannot be taught both literature and science, and if by literature we are to understand Latin and Greek, then, while entirely disputing the assertion, our choice would be clear to abandon Latin and Greek altogether for English, for modern languages, and for science. But an education may be literary, in a very high sense of the word, without embracing the two dead languages; and many a boy would know far more of literature than he has now any opportunity of doing, if he confined himself to the magnificent poetry and unsurpassable eloquence which he may find in large abundance in the neglected treasure-house of our great English authors.

Such a training would be more useful than any which a boy now receives in Greek and Latin. There is an absurd prevalence of the "worship of inutility." If it be assumed—and it is a very questionable assumption—that it is mainly by words that we learn things, even then we should be inexcusable for wasting six or seven long precious years in not learning Greek and Latin. Many great linguists have added nothing to human knowledge, and the Greeks, our vaunted models of style and insight, knew no word of any language but their own, and were absolutely unacquainted with the terms and principles of their own grammar. Yet, were they illiterate? They knew little of *words*, but they made up for it by *thought*—by that power of deep reflection which makes facts luminous with meaning—by that earnest concentration of resolute attention on which have dawned some of the most splendid daybreaks of human discovery.

And, *if* it were necessary to make a choice between a classical and a scientific training, there are many obvious reasons for regarding the latter as the more natural, the better, and the happier of the two. The reason why people doubt it now, is because the majority are so profoundly ignorant of what science is, and what a training in science really implies. But no such exclusive choice is necessary. We ought to teach both literature and science; and to do so is perfectly possible, though not perhaps with our present programmes, our present methods, or possibly even with our present teachers. No system ought to be regarded as intellectually satisfactory which does not produce the following results:—That every boy of average ability leaving school at 18 or 19 should be able to read at sight any easy author in Greek and Latin; that he should be well grounded in arithmetic, algebra, and geometry; that he should understand French and German, and if possible speak one of the two; that he should be able to read his own language well, to write it intelligently, and to show some familiarity with its greatest literature; that he should have a sound knowledge of history and geography; and lastly, that he should be acquainted with the nature and greatest results of the sciences in general, and have a more minute, practical, and experimental acquaintance with one of them at least.

The remainder of the discourse was devoted to illustrating and proving the fact that such results, or results closely analogous to them, had constantly been attained in past times, are at the present moment frequently attained in other countries, and might at any moment be attained by right methods in our own; and the speaker concluded by pointing out the increasingly disastrous national consequences which arise from our continuance of an inadequate system, the utility of which has been immensely diminished by the growth of modern civilization, and which, while it ignores and neglects much that is now indispensable to all high culture, fails to accomplish even its own partial and insufficient aims.

[F. W. F.]

GENERAL MONTHLY MEETING,

Monday, February 3, 1868.

SIR HENRY HOLLAND, Bart. M.D. D.C.L. F.R.S. President, in the Chair.

Arthur Temple Felix Clay, Esq.
Rev. B. W. Gibsone, M.A.
John Kymer, Esq.
James Murray, Esq.
Mrs. S. Ralli,

were *elected* Members of the Royal Institution.

The MANAGERS reported, That at their Meeting, held on January 27th, 1868, the following Resolution was passed unanimously:—

> "The Managers desire to thank Mrs. Faraday for continuing the Superin-"tendence of the House to the end of last year; and at this their first meeting "of the present year, they wish to express their sense of the great debt which "they owe to Mr. Faraday, by resolving unanimously to propose to the "Members, that an Annuity of 150*l.* be offered to Mrs. Faraday from the 1st "of January, 1868."

Proposed by Dr. BENCE JONES, seconded by Mr. WILLIAM POLE, and Resolved unanimously:—

> "That an Annuity of 150*l.* be offered to Mrs. Faraday from the 1st of "January, 1868, and that she be requested to accept this Resolution as an "indication of the feelings of gratitude of the Members to one who so long "and so well did everything for the good of the Institution."

The special thanks of the Members were returned to Mrs. BARLOW for her Seventh Annual Donation of £5. 5*s*.

The special thanks of the Members were returned for the following additions to "the Donation Fund for the Promotion of Experimental Researches":—

Alfred Davis, Esq. (*2nd donation*)	£21	0	0
J. Carrick Moore, Esq. (*5th annual donation*)	10	0	0
John Peter Gassiot, Esq. (*5th annual donation*)	20	0	0
William Dell, Esq. (*2nd donation*)	5	5	0

The **Presents** received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

Secretary of State for India—Bombay Magnetical and Meteorological Observations, 1864. 4to. 1867.

British Museum Trustees—Catalogue of Heteropterous Hemiptera. Part II. 8vo. 1867.

Actuaries, Institute of—Journal, No. 70. 8vo. 1867.

Agricultural and Commercial Society, Royal—Catalogue of Contributions from British Guiana to the Paris Universal Exhibition. (K 95) 8vo. 1867.

Antiquaries, Society of—Archæologia, Vol. XLI. Part 1. 4to. 1867.
Proceedings, Vol. III. Nos. 3–6. 8vo. 1866–7.

Astronomical Society, Royal—Monthly Notices, Vol. XXVIII. Nos. 1, 2. 8vo. 1867–8.

Basel Natural History Society—Verhandlungen, Band IV. Heft 4. 8vo. 1867.
Festrede, u. s. w. 8vo. 1867.

Bremen Naturwissenschaftliche Vereins—Abhandlungen. Band I. Heft 2. 8vo. 1867.

British Architects, Royal Institute of—Sessional Papers, Nos. 1–5. 4to. 1867–8.

Chemical Society—Journal for Dec. 1867 and Jan. 1868. 8vo.

Civil Engineers' Institution—Minutes of Proceedings, Vol. XXVI. 8vo. 1867.

Davis, Alfred, Esq. M.R.I.—Reports of Artisans selected by a Committee of the Society of Arts to visit the Paris Universal Exhibition. 8vo. 1867.

Editors—Artizan for Dec. 1867 and Jan. 1868. 3to.
Athenæum for Dec. 1867 and Jan. 1868. 4to.
British Journal of Photography for Dec. 1867 and Jan. 1868. 4to.
Chemical News for Dec. 1867 and Jan. 1868. 4to.
Engineer for Dec. 1867 and Jan. 1868. fol.
Geological and Natural History Repository. Dec. 1867 and Jan. 1868. 8vo.
Horological Journal for Dec. 1867 and Jan. 1868. 8vo.
Journal of Gas-Lighting for Dec. 1867 and Jan. 1868. 4to.
Mechanics' Magazine for Dec. 1867 and Jan. 1868. 8vo.
Pharmaceutical Journal for Dec. 1867 and Jan. 1868.
Photographic News for Dec. 1867 and Jan. 1868. 4to.
Practical Mechanic's Journal for Dec. 1867 and Jan. 1868. 4to.
Revue des Cours Scientifiques et Littéraires. Dec. 1867 and Jan. 1868.

Essex Institute, U.S.—Proceedings, Vol. V. Nos. 3, 4. 8vo. 1867.

Francis, Dr.—Michael Faraday, his Life and Works. By Prof. De la Rive. (Phil. Mag. Dec. 1867.)

Franklin Institute—Journal, Nos. 503, 504. 8vo. 1867.

Geographical Society, Royal—Proceedings, Vol. XI. No. 6. 8vo. 1867.

Geological Society—Quarterly Journal, Nos. 92, 92*. 8vo. 1867.

Gover, Charles E. Esq. (the Author)—Uniform Metrology for India. 8vo. Madras, 1867.

Horticultural Society, Royal—Proceedings, No. 9. 8vo. 1868.

Linnean Society—Journal, No. 38. 8vo. 1867.

Lubbock, Sir John, Bart. F.R.S. M.R.I. (the Author)—Origin of Civilization and Primitive Condition of Man. 8vo. 1867.

Medical and Chirurgical Society, Royal—Medico-Chirurgical Transactions, Vol. L. 8vo. 1867.

Meteorological Society—Proceedings, No. 33. 8vo. 1867.

Photographic Society—Journal, Nos. 188, 189. 8vo. 1867–8.

Royal Society of Edinburgh—Transactions, Vol. XXIV. Part 3. 4to. 1867.
Proceedings, No. 71. 8vo. 1866–7.

Royal Society of London—Proceedings, Nos. 96, 97. 8vo. 1867.
Greenwich Observations, 1865. 4to. 1867.

Saxon Society of Sciences, Royal—Abhandlungen, Band XII. No. 3. 8vo. 1867.
Berichte, 1867. Hefte 3, 4. 8vo. 1867.

Scottish Society of Arts, Royal—Transactions, Vol. VII. Part 3. 8vo. 1867.
Society of Arts—Journal for Dec. 1867 and Jan. 1868. 8vo.
Statistical Society of London—Journal, Vol. XXX. Part 4. 8vo. 1867.
Surgeon-General United States Army—Circular, No. 7. 4to. 1867.
Sykes, Colonel, M.P. F.R.S. (the Author)—Storm Warnings. (K 95) 8vo. 1867.
Symons, G. J. Esq. (the Author)—Symons' Monthly Meteorological Magazine, Dec. 1867 and Jan. 1868. 8vo.
Tyndall, Professor, LL.D. F.R.S. M.R.I. (the Author)—Heat, a Mode of Motion. 3rd ed. 16to. 1868.
United Service Institution, Royal—Journal, No. 46. 8vo. 1867.
Vereins zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, März bis August, 1866. 4to.
Yates, James, Esq. F.R.S. M.R.I. (the Author)—Reasons why the Office of Warden of the Standards should include Standard Weights and Measures of the Metric System. (K 95) 8vo. 1867.

WEEKLY EVENING MEETING,

Friday, February 7, 1868.

SIR HENRY HOLLAND, Bart. M.D. D.C.L. F.R.S. President,
in the Chair.

PROFESSOR HUXLEY, LL.D. F.R.S.

On the Animals which are most nearly intermediate between Birds and Reptiles.

THOSE who hold the doctrine of Evolution (and I am one of them) conceive that there are grounds for believing that the world, with all that is in it and on it, did not come into existence in the condition in which we now see it, nor in anything approaching that condition.

On the contrary, they hold that the present conformation and composition of the earth's crust, the distribution of land and water, and the infinitely diversified forms of animals and plants which constitute its present population, are merely the final terms in an immense series of changes which have been brought about, in the course of immeasurable time, by the operation of causes more or less similar to those which are at work at the present day.

Perhaps this doctrine of Evolution is not maintained consciously and in its logical integrity by a very great number of persons.* But many hold particular applications of it without committing themselves to the whole; and many, on the other hand, favour the general doctrine without giving an absolute assent to its particular applications.

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* The only complete and systematic statement of the doctrine with which I am acquainted is that contained in Mr. Herbert Spencer's 'System of Philosophy;' a work which should be carefully studied by all who desire to know whither scientific thought is tending.

Thus, one who adopts the nebular hypothesis in Astronomy, or is a Uniformitarian in Geology, or a Darwinian in Biology, is, so far, an adherent of the doctrine of Evolution.

And, as I can testify from personal experience, it is possible to have a complete faith in the general doctrine of Evolution and yet to hesitate in accepting the Nebular, or the Uniformitarian, or the Darwinian hypotheses in all their integrity and fullness. For many of the objections which are brought against these various hypotheses affect them only, and even if they be valid, leave the general doctrine of Evolution untouched.

On the other hand, it must be admitted that some arguments which are adduced against particular forms of the doctrine of Evolution, would very seriously affect the whole doctrine if they were proof against refutation.

For example, there is an objection which I see constantly and confidently urged against Mr. Darwin's views, but which really strikes at the heart of the whole doctrine of Evolution, so far as it is applied to the organic world.

It is admitted on all sides that existing animals and plants are marked out by natural intervals into sundry very distinct groups:—Insects are widely different from Fish—Fish from Reptiles—Reptiles from Mammals—and so on. And out of this fact arises the very pertinent objection, How is it, if all animals have proceeded by gradual modification from a common stock, that these great gaps exist?

We, who believe in Evolution, reply, that these gaps were once non-existent; that the connecting forms existed in previous epochs of the world's history, but that they have died out.

Naturally enough then we are asked to produce these extinct forms of life. Among the innumerable fossils of all ages which exist, we are asked to point to those which constitute such connecting forms.

Our reply to this request is, in most cases, an admission that such forms are not forthcoming, and we account for this failure of the needful evidence by the known imperfection of the geological record. We say that the series of formations with which we are acquainted is but a small fraction of those which have existed, and that between those which we know there are great breaks and gaps.

I believe that these excuses have very great force; but I cannot smother the uncomfortable feeling that they are excuses.

If a landed proprietor is asked to produce the title-deeds of his estate, and is obliged to reply that some of them were destroyed in a fire a century ago, that some were carried off by a dishonest attorney, and that the rest are in a safe somewhere, but that he really cannot lay his hands upon them; he cannot, I think, feel pleasantly secure, though all his allegations may be correct and his ownership indisputable. But a doctrine is a scientific estate, and the holder must always be able to produce his title-deeds, in the way of direct evidence, or take the penalty of that peculiar discomfort to which I have referred.

You will not be surprised, therefore, if I take this opportunity of pointing out that the objection to the doctrine of Evolution, drawn from the supposed absence of intermediate forms in the fossil state, certainly does not hold good in all cases. In short, if I cannot produce the complete title-deeds of the doctrine of animal Evolution, I am able to show a considerable piece of parchment evidently belonging to them.

To superficial observation no two groups of beings can appear to be more entirely dissimilar than Reptiles and Birds. Placed side by side, a Humming-bird and a Tortoise, an Ostrich and a Crocodile, offer the strongest contrast, and a Stork seems to have little but animality in common with the Snake it swallows.

Careful investigation has shown, indeed, that these obvious differences are of a much more superficial character than might have been suspected, and that Reptiles and Birds do really agree much more closely than Birds with Mammals, or Reptiles with Amphibians. But still, "though not as wide as a church-door or as deep as a well," the gap between the two groups, in the present world, is considerable enough.

Without attempting to plunge you into the depths of anatomy, and confining myself to that osseous system to which those who desire to compare extinct with living animals are almost entirely restricted, I may mention the following as the most important differences between all the Birds and Reptiles which at present exist.

1. The pinion of a Bird, which answers to the hand of a man or to the forepaw of a Reptile, contains neither more nor fewer than three fingers. These answer to the thumb and the two succeeding fingers in man, and have their metacarpals connected together by firm bony union, or ankylosed. Claws are developed upon the ends of at most two of the three fingers (that answering to the thumb and the next), and are sometimes entirely absent.

No Reptile with well-developed forelimbs has so few as three fingers; nor are the metacarpal bones of these ever united together; nor do they present fewer than three claws at their terminations.

2. The breast-bone of a Bird becomes converted into a membrane-bone, and ossification commences in it from at least two centres.

The breast-bone of no Reptile becomes converted into membrane-bone, nor does it ever ossify from several distinct centres.

3. A considerable number of caudal and lumbar, or dorsal, vertebræ unite together with the proper sacral vertebræ of a Bird to form its "sacrum." In Reptiles the same region of the spine is constituted by the one or two sacral vertebræ.

4. In Birds the haunch-bone (ilium) extends far in front of, as well as behind, the acetabulum; the ischia and pubes are directed backwards, almost parallel with it and with one another; the ischia do not unite in the ventral middle line of the body.

In Reptiles, on the contrary, the haunch-bone is not produced in front of the acetabulum; and the axes of the ischia and pubes diverge and lie more or less at right angles to that of the ilium. The ischia always unite in the middle ventral line of the body.

5. In all Birds the axis of the thigh-bone lies nearly parallel with the median plane of the body (as in ordinary *Mammalia*) in the natural position of the leg. In Reptiles it stands out at a more or less open angle with the median plane.

6. In Birds one half of the tarsus is inseparably united with the tibia, the other half with the metatarsal bone of the foot. This is not the case in Reptiles.

7. Birds never have more than four toes, the fifth being always absent. The metatarsal of the hallux, or great toe, is always short and incomplete above. The other metatarsals are ankylosed together, and unite with one half of the tarsus, so as to form a single bone, which is called the *tarsometatarsus.*

Reptiles with completely developed hind-limbs have at fewest four toes, the metatarsals of which are all complete and distinct from one another.

Although all existing Birds differ thus definitely from existing Reptiles, one comparatively small section comes nearer Reptiles than the others. These are the *Ratitæ*, or struthious birds, comprising the Ostrich, Rhea, Emeu, Cassowary, *Apteryx*, and the but recently extinct (if they be really extinct) birds of New Zealand, *Dinornis*, &c., which attained gigantic dimensions. All these birds are remarkable for the small size of their wings, the absence of a crest or keel upon the breastbone, and of a complete furcula; in many cases, for the late union of the bones of the pinion, the foot, and the skull. In this last character, in the form of the sternum, of the shoulder-girdle, and in some peculiarities of the skull, these birds are more reptilian than the rest; but the total amount of approximation to the reptilian type is but small, and the gap between Reptiles and Birds is but very slightly narrowed by their existence.

How far can this gap be filled up by a reference to the records of the life of past ages?

This question resolves itself into two:—

1. Are any fossil Birds more reptilian than any of those now living?

2. Are any fossil Reptiles more bird-like than living reptiles? And I shall endeavour to show that both these questions must be answered in the affirmative.

It is very instructive to note by how mere a chance it is we happen to know that a fossil bird, more reptilian in some respects than any now living, once existed.

Bones of birds have been obtained from rocks of very various dates in the Tertiary series without revealing any forms but such as would range themselves among existing families.

A few years ago the great Mesozoic formations had yielded only the few fragmentary ornitholites which have been discovered in the Cambridge greensand, and which are insufficient for the complete determination of the affinities of the bird to which they belonged.

However, the very fine calcareous mud of the ancient oolitic sea-bottom which has now hardened into the famous lithographic slate of Solenhofen, and has preserved innumerable delicate organisms of the existence of which we should otherwise have been, in all probability, totally ignorant, in 1861 revealed the impression of a feather to the famous palæontologist, Herman von Meyer. Von Meyer named the unknown bird to which this feather belonged *Archæopteryx lithographica*, and in the same year, the independent discovery by Dr. Häberlein of the precious skeleton of the *Archæopteryx* itself, which now adorns the British Museum,* demonstrated the chief characters of this very early bird. But it must be remembered that this feather and this imperfect skeleton are the sole remains of birds which have yet been obtained in all that great series of formations known as Wealden and Oolite, which partly lie above and partly correspond with, the Solenhofen slates.

Though some palæontologists may be forced by a sense of consistency to declare that the class of birds was created in the sole person of *Archæopteryx* during the deposition of the Solenhofen slates, and disappeared during the Wealden, to be re-created in the Greensand, to vanish once more during the Cretaceous epoch and reappear in the Tertiaries, I incline to the hypothesis that many birds beside *Archæopteryx* existed throughout all this period of time, and that we know nothing about them, simply because we do not happen to have hit upon those deposits in which their remains are preserved.

Now, what is this *Archæopteryx* like? Unfortunately, the skull is lost, but the leg and foot, the pelvis, the shoulder-girdle, and the feathers, so far as their structure can be made out, are completely those of existing ordinary birds.

On the other hand, the tail is very long, and more like that of a reptile than that of a bird in this respect. Two digits of the manus have curved claws, much stronger than those of any existing bird; and, to all appearance, the metacarpal bones are quite free and disunited.

Thus it is a matter of fact that, in certain particulars, the oldest known bird does exhibit a closer approximation to reptilian structure than any modern bird.

Are any fossil reptiles more bird-like than those which now exist?

As in the case of birds, the Tertiary formations yield no trace of reptiles which depart from the type of the existing groups. But otherwise than is true of birds, the newest of the Mesozoic formations, the Chalk, makes us acquainted with reptiles which, at first sight, seem to approach birds in a very marked manner. These are those flying reptiles, the Pterodactyles, which resemble the great majority of birds in the presence of air-cavities in their bones, in the wonderfully bird-like aspect of their coracoid and scapula, and in their broad sternum with its median crest. Furthermore, in some of the Pterodactyles, the premaxillæ and the symphysial part of the mandibles were prolonged into beaks, which appear to have been sheathed in horn, while the rest of each jaw was armed with teeth.

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* The fossil has been described by Professor Owen in the 'Philosophical Transactions' for 1863.

But horn-sheathed beaks are found in reptiles as well as in birds; the structure of the scapulocoracoid arch and of the sternum, and the pneumaticity of the bones, vary greatly among birds themselves; and these characters of the Pterodactyles may be merely adaptive modifications.

On the other hand, the manus has four free digits, the three inner of which are strongly clawed, while the fourth is enormously prolonged, in total contrast to the abortion of the corresponding digit in birds. The pelvis is as wholly unlike that of birds as is the hind-limb and foot.

Thus it appears that Pterodactyles, among Reptiles, approach birds much as Bats, among Mammals, may be said to do so. They are a sort of reptilian Bats* rather than links between Reptiles and Birds, and it is precisely in those organs which, in birds, are the most characteristically ornithic, the manus and the pes, that they depart most widely from the ornithic type.

Clearly then the passage from Reptiles to Birds is not from the flying Reptile to the flying Bird. Let us try another line. I have already observed that, in the existing world, the nearest approximation to Reptiles is presented by certain land Birds, the Ostriches and their allies, all of which are devoid of the power of flight by reason of the small relative size of their fore-limbs and of the character of their feathers.

Can we find any extinct Reptiles which approached these flightless birds, not merely in the weakness of their fore-limbs, but in other and more important characters?

I imagine that we can, if we cast our eyes in what at first sight seems to be a most unlikely direction.

The *Dinosauria*, a group of extinct reptiles, containing the genera *Iguanodon*, *Hadrosaurus*, *Megalosaurus*, *Poikilopleuron*, *Scelidosaurus*, *Plateosaurus*, &c., which occur throughout the whole series of the Mesozoic rocks, and are, for the most part, of gigantic size, appear to me to furnish the required conditions.

In none of these animals are the skull, or the cervical region of the vertebral column, completely known, while the sternum and the manus have not yet been obtained in any of the genera. In none has any trace of a clavicle been observed.

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* It will be understood that I do not suggest any direct affinity between Pterodactyles and Bats.

With regard to the characters which have been positively determined, it has been ascertained, that:

1. From four to six vertebræ enter into the composition of the sacrum, and become connected with the ilia in a manner which is partly ornithic, partly reptilian.

2. The ilia are prolonged forwards in front of the acetabulum as well as behind it, and the resemblance to the bird's ilium thus produced is greatly increased by the widely arched form of the acetabular margin of the bone, and the extensive perforation of the floor of the acetabulum.

3. The other two components of the *os innominatum* have not been observed actually in place; indeed, only one of them is known at all, but that one is exceedingly remarkable from its strongly ornithic character. It is the bone which has been called "clavicle" in *Megalosaurus* and *Iguanodon* by Cuvier and his successors, though the sagacious Buckland had hinted its real nature.* But these bones are not in the least like the clavicles of any animal which possesses a clavicle, while they are extremely similar to the ischia of such a bird as an ostrich; and in the only instance in which they have been found in tolerably undisturbed relation with other parts of the skeleton, namely, in the Maidstone *Iguanodon*, they lie, one upon each side of the body, close to the ilia. I hold it to be certain that these bones belong to the pelvis, and not to the shoulder-girdle, and I think it probable that they are ischia; but I do not deny that they may be pubes.

4. The head of the femur is set-on at right angles to the shaft of the bone, so that the axis of the thigh-bone must have been parallel with the middle vertical plane of the body, as in birds.

5. The posterior surface of the external condyle of the femur presents a strong crest, which passes between the head of the fibula and the tibia as in birds. There is only a rudiment of this structure in other reptiles.

6. The tibia has a great anterior or "procnemial" crest, convex on the inner, and concave on the outer, side. Nothing comparable to this exists in other reptiles, but a correspondingly developed crest exists in the great majority of birds, especially such as have great walking or swimming powers.

7. The lower extremity of the fibula is much smaller than the other; it is, proportionally, a more slender bone than in other reptiles. In birds the distal end of the fibula thins away to a point, and it is a still more slender bone.

8. *Scelidosaurus* has four complete toes, but there is a rudiment of a fifth metatarsal. The third or middle toe is the largest, and the metatarsal of the hallux is much smaller at its proximal than at its distal end.

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* The so-called "coracoid" of *Megalosaurus* is the ilium. I am indebted to Professor Phillips, and to the splendid collection of Megalosaurian remains which he has formed at Oxford, for most important evidence touching this reptile.

Iguanodon has three large toes, of which the middle is the longest. The slender proximal end of a first metatarsal has been found adherent to the inner face of the second, so that if the hallux was completely developed it was probably very small. No rudiment of the outer toe has been observed.

It is clear, from the manner in which the three principal metatarsals articulate together, that they were very intimately and firmly united, and that a sufficient base for the support of the body was afforded by the spreading out of the phalangeal regions of the toes.

From the great difference in size between the fore and hind limbs, Mantell, and more recently Leidy, have concluded that the *Dinosauria* (at least, *Iguanodon* and *Hadrosaurus*) may have supported themselves, for a longer or shorter period, upon their hind legs. But the discovery made in the weald, by Mr. Beckles, of pairs of large three-toed foot-prints, of such a size and at such a distance apart that it is difficult to believe they can have been made by anything but an *Iguanodon*, lead to the supposition that this vast reptile, and perhaps others of its family, must have walked, temporarily or permanently, upon its hind legs.

However this may be, there can be no doubt that the hind quarters of the *Dinosauria* wonderfully approached those of birds in their general structure, and therefore that these extinct Reptiles were more closely allied to birds than any which now live.

But a single specimen, obtained from those Solenhofen slates, to the accident of whose existence and usefulness in the arts palæontology is so much indebted, affords a still nearer approximation to the "missing link" between reptiles and birds. This is the singular reptile which has been described and named *Compsognathus longipes* by the late Andreas Wagner, and some of the more recondite ornithic affinities of which have been since pointed out by Gegenbaur. Notwithstanding its small size (it was not much more than two feet in length), this reptile must, I think, be placed among, or close to, the *Dinosauria;* but it is still more bird-like than any of the animals which are ordinarily included in that group.

Compsognathus longipes has a light head, with toothed jaws, supported upon a very long and slender neck. The ilia are prolonged in front of and behind the acetabulum. The pubes seem to have been remarkably long and slender (a circumstance which rather favours the interpretation of the so-called "clavicles" of *Iguanodon* as pubes). The fore-limb is very small. The bones of the manus are unfortunately scattered, but only four claws are to be found, so that possibly each manus may have had but two clawed digits.

The hind limb is very large, and disposed as in birds. As in the latter class, the femur is shorter than the tibia, a circumstance in which *Compsognathus* is more ornithic than the ordinary *Dinosauria.*

The proximal division of the tarsus is ankylosed with the tibia, as in birds. In the foot the distal tarsals are not united with the three long and slender metatarsals, which answer to the second, third, and fourth toes. Of the fifth toe there is only a rudimentary metatarsal. The hallux is short, and its metatarsal appears to be deficient at its proximal end.

It is impossible to look at the conformation of this strange reptile and to doubt that it hopped or walked, in an erect or semi-erect position, after the manner of a bird, to which its long neck, slight head, and small anterior limbs must have given it an extraordinary resemblance.

I have now, I hope, redeemed my promise to show that, in past times, birds more like reptiles than any now living, and reptiles more like birds than any now living, did really exist.

But, on the mere doctrine of chances, it would be the height of improbability that the couple of skeletons, each unique of its kind, which have been preserved in those comparatively small beds of Solenhofen slate, which record the life of a fraction of Mesozoic time, should be the relics, the one of the most reptilian of birds, and the other of the most ornithic of reptiles.

And this conclusion acquires a far greater force when we reflect upon that wonderful evidence of the life of the Triassic age, which is afforded us by the sandstones of Connecticut. It is true that these have yielded neither feathers nor bones; but the creatures which traversed them when they were the sandy beaches of a quiet sea, have left innumerable tracks which are full of instructive suggestion. Many of these tracks are wholly undistinguishable from those of modern birds in form and size; others are gigantic three-toed impressions, like those of the Weald of our own country; others are more like the marks left by existing reptiles or *Amphibia*.

The important truth which these tracks reveal is, that, at the commencement of the Mesozoic epoch, bipedal animals existed which had the feet of birds, and walked in the same erect or semi-erect fashion. These bipeds were either birds or reptiles, or more probably both; and it can hardly be doubted that a lithographic slate of Triassic age would yield birds so much more reptilian than *Archæopteryx*, and reptiles so much more ornithic than *Compsognathus*, as to obliterate completely the gap which they still leave between reptiles and birds.

But if, on tracing the forms of animal life back in time, we meet, as a matter of fact, with reptiles which depart from the general type to become bird-like, until it is by no means difficult to imagine a creature completely intermediate between *Dromæus* and *Compsognathus*, surely there is nothing very wild or illegitimate in the hypothesis that the *phylum* of the class *Aves* has its root in the Dinosaurian reptiles; that these, passing through a series of such modifications as are exhibited in one of their phases by *Compsognathus*, have given rise to the *Ratitæ;* while the *Carinatæ* are still further modifications and differentiations of these last, attaining their highest specialization in the existing world in the Penguins, the Cormorants, the Birds of Prey, the Parrots, and the Song-birds.

However, as many completely differentiated birds in all probability existed even in the Triassic epoch, and as we possess hardly any knowledge of the terrestrial reptiles of that period, it may be regarded as certain that we have no knowledge of the animals which linked Reptiles and Birds together historically and genetically; and that the *Dinosauria*, with *Compsognathus*, *Archæopteryx*, and the struthious Birds, only help us to form a reasonable conception of what these intermediate forms may have been.

In conclusion, I think I have shown cause for the assertion that the facts of Palæontology, so far as Birds and Reptiles are concerned, are not opposed to the doctrine of Evolution, but, on the contrary, are quite such as that doctrine would lead us to expect; for they enable us to form a conception of the manner in which Birds may have been evolved from Reptiles, and thereby justify us in maintaining the superiority of the hypothesis, that Birds have been so originated, to all hypotheses which are devoid of an equivalent basis of fact.

[T. H. H.]

WEEKLY EVENING MEETING,

Friday, February 14, 1868.

SIR HENRY HOLLAND, Bart. M.D. D.C.L. F.R.S. President,
in the Chair.

HENRY E. ROSCOE, B.A. F.R.S.

On Vanadium, one of the Trivalent Group of Elements.

THE metal vanadium (so called from Vanadis, a cognomen of the Scandinavian goddess Freia) was discovered in 1830 by Sefström in the celebrated Swedish bar-iron made from the Taberg ore. From this source, even when using many pounds of the iron, Sefström obtained only minute quantities of the new substance, but he found it in somewhat larger amount in the slag or cinder produced in the reduction of the iron ore. Sefström ascertained some of the most peculiar characters of the substance, proved it to be a new element, and prepared some of its compounds in the pure state. The reactions by which vanadium can be separated and distinguished from all the other elements are: (1) The formation of a soluble sodium vanadate when the vanadium compounds are fused with sodium carbonate; (2) the formation of an insoluble ammonium vanadate when sal-ammoniac is added to the solution of a soluble vanadate; (3) the production of a splendid blue solution when this ammonium salt, dissolved in hydrochloric acid, is warmed with reducing agents such as oxalic acid.

Sefström not having leisure to prosecute the full examination of the properties of the new metal, handed over his preparations to Berzelius; and it is to the investigations of the great Swede (1831) that we owe almost all our acquaintance with the chemistry of vanadium.

Since Berzelius's time vanadium has been discovered in many minerals, of which a lead-ore containing lead vanadate and called by the mineralogists vanadinite, is the most important. It has also been found in many iron ores, in clay, bricks, and even in caustic soda. Still the quantity of the substance found in all these various sources has been extremely small; so much so, that the vanadium compounds must be reckoned amongst the greatest of chemical rarities, and we find them quoted in the price lists of dealers in chemicals at 1*s*. 6*d*. per grain, or 35*l*. per ounce! It is clear that our knowledge of the chemical properties of a substance so rare must necessarily be but incomplete, as the difficulties of obtaining exact or satisfactory results with small quantities of material are evident; and, in fact, the statements of the only persons who have worked upon the subject recently (Schafarik Czudnowicz), instead of giving us any more reliable information respecting the character of vanadium, have only served to throw doubt upon some of the conclusions of Berzelius, and thus to render our knowledge even less complete than it appeared to be.

Hence it was with much satisfaction that, in February, 1865, the speaker came into possession of a plentiful source of vanadium in a by-product obtained in the preparation of cobalt from the copper-bearing beds of the lower Keuper Sandstone of the Trias at Alderley Edge, in Cheshire. The manager of the works was puzzled to know why a blue solution, supposed by him to contain copper, did not deposit the red metal upon a strip of zinc; the speaker recognized this reaction as due to the presence of vanadium, and secured the whole of the by-product, which he found to contain about 2 per cent. of the rare metal. The exact position of the vanadium mineral in the sandstone beds cannot now be stated, as the mine (at Mottram St. Andrews) from which the cobalt ore was obtained is now closed, and cannot be entered. The general characters of the deposit are, however, well known, and exhibit points of great interest; they have been well described by Mr. Hull as follows:—

"The 'edge' or escarpment of Alderley rises from the eastern side of the plain of Cheshire gradually towards the east, but with a steep and abrupt ridge towards the north. This northern bank is richly wooded, and has a very beautiful aspect when viewed from a distance, as it contrasts strongly with the almost level plain which sweeps away to the northward and westward from its base. The ridge has here been upheaved along the line of a large fault, bearing east and west, throwing down at its base the Red Marl; and on the other side bringing up the soft sandstone of the Bunter, capped by a mural cliff of lower Keuper Conglomerate, which often breaks out in conspicuous masses through the foliage. The beds rise from the plain towards the east at an angle of about from 5° to 10°, and the escarpment is continued southward for some distance facing the east."

SUCCESSION OF BEDS IN DESCENDING ORDER—(*Hull*).

Red Marl		Red and grey laminated Marls.
Waterstones	Lower Keuper Sandstone, 500 feet.	Brownish flaggy Sandstones and Marls.
Freestone		White and brown Freestone.
Copper-bearing Sandstone ..		Soft white, yellow, and variegated Sandstone.
Conglomerate		Hard quartzose Conglomerate, underlain by bands of Marl, forming the base of the Keuper Sandstone.
Upper red and mottled Sandstone	Bunter.	Soft fine-grained yellow and red Sandstone, being the uppermost member of the Bunter Sandstone.

The beds in the above series which claim the greatest share of our attention are those at the base of the Keuper series, for in these occur the copper and other minerals. The copper, as both blue and green carbonate, occurs disseminated throughout the sand, the ore coating the outside of the grains of sand and the pebbles of quartz. In addition to copper, bands containing lead both as carbonate and sulphide (galena) occur, also bands and veins of cobalt ochre, oxide of manganese, and iron ochre in workable quantity. The copper is extracted from the ore by solution in hydrochloric acid and precipitation as metal by scrap iron. The ordinary copper liquor, as well as the oxide of iron precipitated by lime from the solution of the chloride, does not contain any trace of vanadium, nor was the speaker able to detect any of this metal in the ore as at present worked.

Following, in the main, the process of preparation adopted by Sefström, the speaker obtained from the above-mentioned lime precipitate several pounds of pure ammonium vanadate, from which all the other compounds of vanadium can be prepared.

What now were the conclusions to which Berzelius arrived from his experiments concerning the constitution of the vanadium compounds? He corroborated Sefström's statement, that the most characteristic feature of the substance is the existence of an acid-forming oxide, termed vanadic acid, produced whenever any of the oxides are heated in the air. Berzelius also discovered two other oxides of vanadium, of which he ascertained the composition; and likewise a volatile chloride. To the highest oxide he gave the formula $V\,O_3$, to the second $V\,O_2$, and to the lowest (or suboxide) $V\,O$; whilst the chloride was represented by $V\,Cl_3$. The atomic weight of the metal he ascertained to be $V = 68 \cdot 5$. Berzelius came to this conclusion from the following experimentally ascertained facts: (1) That on passing hydrogen over heated vanadic acid a constant loss of weight occurred, and the suboxide was formed; (2) that when dry chlorine is passed over the suboxide thus prepared, the volatile chloride was formed, and a residue of vanadic acid remained, which was exactly equal in weight to one-third of the acid originally taken for reduction. Hence assuming that the lowest oxide contains one atom of oxygen (an assumption borne out by the analysis of the chloride), the acid must contain three atoms of oxygen,* and the following formulæ represent the composition of these compounds according to Berzelius :—

$$VO \qquad VO_2 \qquad VO_3 \qquad VCl_3 \qquad (V = 68 \cdot 5)$$

The interest attaching to the conclusions which Berzelius fairly drew from his experiments was much heightened by an observation made by Rammelsberg in 1856, as to the exact crystalline form of the mineral vanadinite, a double salt of lead vanadate and lead chloride.

So long ago as 1780 Werner had observed the identity of crystalline form of two minerals, *viz.* apatite, a phosphato-fluoride of calcium, and pyromorphite, a phosphato-chloride of lead; to which may be added, mimetesite, an arsenato-chloride of lead. These minerals all have an analogous composition, being represented by the formulæ—

Apatite $3\,(Ca_3\,P_2\,O_8) + Ca\,Fl_2$
Pyromorphite $3\,(Pb_3\,P_2\,O_8) + Pb\,Cl_2$
†Mimetesite $3\,(Pb_3\,As_2\,O_8) + Pb\,Cl_2$.

They are truly isomorphous, crystallizing in hexagonal prisms, terminated with hexagonal pyramids, having the same angles and the same length of axes. Rammelsberg added to this list the mineral vanadinite, which he ascertained by measurement to be strictly isomorphous with the foregoing, and to be as follows. The angle P on P was in

1) Vanadinite .	142° 30′	3) Pyromorphite	142° 15′
2) Apatite . .	142° 20′	4) Mimetesite .	142° 7′

and the relation of the length of the axis:

1)	1 : 0·727	3)	1 : 0·736
2)	1 : 0·732	4)	1 : 0·739.

So far, indeed, has the identity of crystalline form been traced, that crystals have been found which at one end consisted of vanadinite, and at the other of pyromorphite (Heddle). Now judging from the

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* Berzelius concludes that the acid does not contain two atoms of metal, inasmuch as no alum could be formed with potassium sulphate corresponding to those formed by well-known sesquioxides.

† This group of minerals may be considered as Calcium Triphosphofluorhydine, &c., thus:—

$$\left.\begin{matrix} 3\,(PO''') \\ Ca_5 \\ \end{matrix}\right\}O_9 \qquad\qquad \left.\begin{matrix} 3\,PO''' \\ Ca_6 \\ \end{matrix}\right\}O_9$$
$$\begin{matrix} Fl \end{matrix} \qquad\qquad\qquad\qquad \begin{matrix} Cl \end{matrix}$$

Apatite. Pyromorphite. (*Würtz.*)

crystallographic analogies alone, we should conclude that the formula of vanadinite is

$$3\,(Pb_3\,V_2\,O_8) + Pb\,Cl_2{}^*$$

the oxide of vanadium contained in the mineral having a formula V_2O_5, agreeing with the corresponding oxides of phosphorus and arsenic, P_2O_5 and As_2O_5. In making this assumption, we are, however, at once confronted with the unyielding chemical facts of Berzelius, according to which the oxide in question must be represented by the formula $V\,O_3$, and contains three, and not five, atoms of oxygen.

It is, then, evident that we have here either to do with an exception to the law of Isomorphism, or else Berzelius's views are erroneous. Until this latter has been proved to be the case, chemists have, however, only been justified in assuming the former alternative to be the correct explanation.

The speaker stated that in order to endeavour to clear up this question, he had most carefully repeated Berzelius's experiments, and that he had confirmed them in every particular; but having pursued the subject further than Berzelius, he had at last come to conclusions concerning the constitution of the vanadium compounds totally different from those drawn by the Swedish chemist, and had succeeded in obtaining the key to the enigma presented by the above anomalous crystallographic relations.

The speaker has proved that the substance supposed by Berzelius to be Vanadium, $V=68{\cdot}5$, is not the metal, but an oxide, and that the true atomic weight of the metal is $68{\cdot}5-16=52{\cdot}5$ (or rather, according to the speaker's exact determinations of the atomic weight, $67{\cdot}3-16=51{\cdot}3$).† The highest oxide, the vanadic acid, $V\,O_3$, of Berzelius, hence becomes a pentoxide, V_2O_5, corresponding to P_2O_5 and As_2O_5, and the isomorphism of vanadinite with the pyromorphite group of minerals is fully explained. The suboxide of Berzelius is a tri-oxide, V_2O_3, whilst the terchloride ($V\,Cl_3$) of Berzelius is an oxychloride, having the formula $V\,O\,Cl_3$, and corresponding to oxychloride of phosphorus, $P\,O\,Cl_3$. The oxide supposed by Berzelius to be the metal contains 51·3 parts by weight of vanadium to 16 parts by weight of oxygen, and the vanadic oxide of Berzelius also exists,

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* Or Lead Trivanadochlorhydrine

$$\left.\begin{array}{c} 3\,V\,O \\ Pb_5 \\ Cl \end{array}\right\} O$$

† In his paper on Vanadium, read before the Royal Society (Dec. 19, 1867), the author ventured to predict that the difference between the number he obtained (67·3) and that found by Berzelius (68·5) was probably owing to the fact that the vanadium compounds employed by Berzelius contained traces of phosphorus, which render the perfect reduction of the vanadic acid in hydrogen impossible. Most fortunately this supposition has been singularly verified, inasmuch as Dr. Frankland has kindly placed in the speaker's hands a small specimen of vanadiate of ammonia found in Faraday's collection, and labelled, "Sent to me by Berzelius, 1831." On examination, this sample was found to contain considerable quantities of phosphorus, thus confirming the speaker's previously expressed opinion.

containing 51·3 parts of the metal to 32 parts of oxygen; to these oxides the empirical formulæ V_2O_3 and V_2O_4 may be given. Thus we have the following as representing the true composition of these vanadium compounds:—

	Dioxide.	Trioxide.	Tetroxide.	Pentoxide.	Oxytrichloride.
$V = 51 \cdot 3$	V_2O_2	V_2O_3	V_2O_4	V_2O_5	$VOCl_3$

Each of the four oxides can be obtained in the anhydrous state; the dioxide is prepared as a grey metallic powder, by passing the vapour of the oxytrichloride mixed with hydrogen over red-hot carbon. The trioxide is obtained by the reduction of vanadic acid in a current of hydrogen, and the tetroxide is formed by the slow oxidation of the trioxide.

The lowest or dioxide of vanadium (V_2O_2) is obtained in solution by the reducing action of nascent hydrogen evolved from zinc, cadmium, or sodium amalgam upon the sulphuric acid solution of vanadic acid, which, passing through all stages of blue and green colour, ultimately assumes a permanent lavender tint. This solution of V_2O_2 in sulphuric acid acts as a most powerful reducing agent, bleaching indigo solution and other vegetable colouring matters as rapidly as chlorine; it also absorbs oxygen with avidity from the air, forming a deep brown solution. The other oxides of vanadium may be obtained in solution by the action of various reducing agents on the sulphuric solution of vanadic acid. Thus, by the action of nascent hydrogen evolved from magnesium a permanent *green* tint is obtained, and the vanadium is contained in solution as the trioxide, V_2O_3; whilst if moderate reducing agents, such as sulphurous acid, sulphuretted hydrogen, or oxalic acid are employed, the colour of the liquid does not pass beyond the *blue* stage, and the vanadium is contained in solution as tetroxide, V_2O_4.* The different colours of solutions containing these oxides was exhibited by means of the magnesium light.

The fact that the lemon-coloured chloride (the terchloride of Berzelius) contains oxygen was clearly demonstrated during the discourse by passing the vapour from a few grammes of the substance, together with perfectly pure hydrogen gas, over red-hot carbon. A portion of the oxygen of the oxychloride unites with the carbon to form carbonic acid, and the presence of this gas was shown by the precipitation of barium carbonate in clear baryta water contained in two test-tubes placed one before the other. At the commencement of the experiment, the carbonic acid was entirely absorbed by the small quantity of baryta water contained in the first test-tube; but after some time the hydrochloric acid gas simultaneously produced by the decomposition of the chloride saturated this liquid, expelling the carbonic acid gas, which being carried forward into the second test-tube, threw down a bulky precipitate of barium carbonate, thus showing that the turbidity cannot possibly be due to the presence of any vanadium compound. It was found quite unnecessary to place a tube containing heated copper oxide after the red-hot carbon, for the purpose of oxidizing any carbonic oxide gas which might be formed, inasmuch as carbonic acid was always left in sufficient quantity to give a considerable precipitate. No method has been found for separating the whole of the oxygen from the oxychloride, and hence it has been impossible to make the above experiment quantitatively. Solid oxychlorides are obtained by the action of hydrogen upon the oxytrichloride, one of which resembles mosaic gold, possessing a bright metallic bronze-like lustre, and having been taken for the metal by Schafarik.

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* In his communication to the Royal Society (Bakerian Lecture, Proc. Royal Soc., XVI., 220), the author gave the empirical formula VO and VO_2 to the 1st and 3rd oxides of vanadium, as the molecular weights of these oxides have not been determined, and it is uncertain whether they obey the law of even atomicities, or, like the only corresponding compounds, the nitrogen oxides, are exceptions to this law.

On consideration, the author has, however, thought it best to adopt the doubled formula as urged by Sir Benjamin Brodie on the occasion above referred to.

The atomic weight of vanadium was determined (1) by reducing the pentoxide to trioxide in a current of hydrogen. (2) By the analysis of the oxytrichloride. The atomic weight obtained as the mean of a large number of well-agreeing experiments is 51·3.

The metal itself has not yet been obtained, but a compound of vanadium and nitrogen has been prepared, shown by direct analysis to contain 14 parts by weight of nitrogen to 51·3 parts by weight of vanadium, corresponding to the formula $V N$. The existence of this compound is proof positive of the true atomic weight of the metal, and the nitride serves as the point of departure from which to seek for the metal and the true chlorides of vanadium, one of which, $V Cl_3$, has already been prepared by the action of chlorine upon the nitride. It is a dark brown liquid, which decomposes when thrown into water, forming a green solution containing $V_2 O_3$. The speaker demonstrated the fact that the oxychloride, $V O Cl_3$, when thrown into water decomposes with formation of a *yellow* solution of vanadium pentoxide, $V_2 O_5$, whilst the trichloride, $V Cl_3$, on being similarly treated yields a *green* solution containing the metal in solution as trioxide, $V_2 O_3$. He then compared these reactions with the decomposition of the corresponding phosphorus compounds, $P O Cl_3$ and $P Cl_3$, forming $P_2 O_5$ and $P_2 O_3$, and rendered these reactions visible by obtaining a precipitate of yellow silver phosphate in the first case, and of black metallic silver in the second.

The characters of the vanadates themselves bear out the analogy of the highest oxide with the corresponding oxides of phosphorus and arsenic. In the first place, all the naturally occurring vanadates are tribasic; secondly, the true character of vanadic acid is shown to be tribasic, by the fact that, when the pentoxide is fused with sodium carbonate, three atoms of $C O_2$ are liberated, and the normal or orthovanadate, $Na_3V_2O_8$ (corresponding to $Na_3P_2O_8$), is formed; thirdly, the so-called mono-vanadates are monobasic salts, corresponding to the monobasic phosphates, and may be termed meta-vanadates, thus, $NaVO_3$ and $Ba\,2\,VO_3$, whilst the so-called bi-vanadates are anhydro-salts.

All the reactions by which Berzelius explained the facts he discovered, can equally well be represented according to the new atomic weight and constitution; thus:—

BERZELIUS' FORMULÆ.

$$V=68{\cdot}5 \quad O=8$$

$$1)\ VO_3+H_2=VO+H_2O_2$$

$$2)\ 3\,(VO)+Cl_6=VO_3+2\,(VCl_3)$$

NEW FORMULÆ.

$$V=51{\cdot}3 \quad O=16$$

$$V_2O_5+2\,H_2=V_2O_3+2\,(H_2O)$$

$$3\,(V_2O_3)+6\,Cl_2=V_2O_5+4\,(VOCl_3)$$

The speaker stated that the foregoing facts clearly pointed out that vanadium, hitherto standing in no definite relation to other elements, must be regarded as a member of the well-known Trivalent or Triad class of elementary substances, comprising nitrogen, phosphorus, boron, arsenic, antimony, and bismuth.

It is true that we are still but imperfectly acquainted with many of the characters of vanadium, but the more its nature is studied, the more points of family resemblance will be discovered, and the more close will the ties be found, which bind it to the great Triad family.

The following tabular statement of the compounds of the most important members of this group clearly shows their common relations:—

TRIVALENT GROUP OF ELEMENTS.

	Nitrogen. $N = 14$	Phosphorus. $P = 31$	Vanadium. $V = 51{\cdot}3$	Arsenic. $As = 75$	Antimony. $Sb = 122$
Trihydrides	NH_3	PH_3	—	AsH_3	SbH_3
Trichlorides	NCl_3 (?)	PCl_3	VCl_3	$AsCl_3$	$SbCl_3$
Pentachlorides ..	—	PCl_5	—	—	$SbCl_5$
Oxychlorides ..	—	$POCl_3$	$VOCl_3$	—	—
Monoxides	N_2O	—	—	—	—
Dioxides	N_2O_2	—	V_2O_2	—	—
Trioxides	N_2O_3	P_2O_3	V_2O_3	As_2O_3	Sb_2O_3
Tetroxides	N_2O_4	—	V_2O_4	—	Sb_2O_4
Pentoxides	N_2O_5	P_2O_5	V_2O_5	As_2O_5	Sb_2O_5

In conclusion, the speaker remarked that vanadium was the fourth substance, supposed by its discoverer to be a metal, which had in recent years been shown to be a compound body.

Titanium.	*Uranium.*	*Niobium.*	*Vanadium.*
Wollaston, 1823. Wöhler, 1849.	Klaproth, 1789. Peligot, 1849.	Hatchett, 1801. Rose, 1842–64. Marignac, 1865.	Sefström and Berzelius, 1831.

[H. E. R.]

WEEKLY EVENING MEETING,

Friday, February 21, 1868.

Sir Henry Holland, Bart. M.D. D.C.L. F.R.S. President,
in the Chair.

Rev. M. W. Mayow, M.A.

On Hamlet.

The speaker, after a few words as to certain modes of estimating greatness, came more directly to consideration of the transcendent merits of 'Hamlet.' "For," said the speaker, "here first I would say, however much the other plays may exceed any other man's execution, however much many of them may exceed any other poet's conception, yet pre-eminently does 'Hamlet' stand out beyond all other works in this its conception. And mark the difference as to the *kind* of conception which is exhibited in 'Hamlet.' In the 'Tempest,' in the 'Midsummer Night's Dream,' in 'Macbeth,' there is the most wonderful conception of the feelings and nature of spiritual beings—supernatural beings, transferred to this nether world, and acting a part in it. This element is not wanting in Hamlet. The Ghost comes and speaks as a disembodied spirit might well be supposed to speak having *his* cause to prompt him; but, after all, the wonder of wonders in the conception in this play is not in its supernatural, but in the natural elements. Hamlet himself is the marvellous creation. Some may think it more wonderful to seem to know and depict to the life spiritual natures; but to me it does not seem so, at least not when such a character as that of Hamlet is placed in the opposite scale. We have, too, better means of judging of men than of spirits how true the depiction is to nature. In the present instance, we admit this truth to nature in Hamlet; and yet we see how impossible it is that such a nature should ever have been conceived at all if Shakespeare had not portrayed and expressed it. We see, believe, know, that Hamlet is all nature—nature of the highest order, moral, intellectual, I believe too, physical.—We see all these excellences, in the most wonderful combination, under the hardest trial, working out their results, till we are assured it would be a philosophical heresy of the deepest dye to question a single point as to truth to nature in the character; and yet the character is so hard to make out that no two men seem to be agreed as to Hamlet's feelings, motives, thoughts, actions, or the true key to them. See then here the pre-eminence of the creation and portrayal of such a character over others. Grant that Othello, Caliban, Prospero, Lear, Macbeth, or their equivalents, might be reproduced in a thousand, or ten thousand years, you feel the world must run out a million before you would have another Hamlet: that is, that practically neither he (nor his equivalent) will ever be reproduced at all. You see there might be—there is—there has been *one such man* (he is a true man, no monster even in excellences) since the world began. Now that he stands before you, you recognize and confess this; but you see also that if Shakespeare had not, Prometheus-like, put life in him, he would never have been at all. And this, above and independently of all other characters in the play—this, apart from all other considerations: all beauty of dialogue, all skill in execution, melody in versification, general truth of sentiment, wisdom, interest, morality; beyond all these things—this wonder of conception in this particular 'quintessence of dust' places 'Hamlet,' in my judgment, first and foremost above all the other plays in order of merit."

The speaker then proceeded to say that amid the raging contests as to the true key to Hamlet's character and actions (which time did not permit him to go through), he felt bound to say a few words as to his own conception of Hamlet, lest he should seem to hold him up to admiration without even a theory of the excellences or consistency of his character. "The following," he said, "is in my conceit, *something* of what Shakespeare intended: A man, as evidently of the highest rank, so likewise of the highest intellect; of the most perfect form, the most delicate organization, the utmost natural piety, the most exquisite human affections, the most cultivated understanding, proved in all ways of reflection, argument, repartee (Hamlet is absolutely the quickest-witted man ever drawn),—this man meeting with the hardest trials—1st, his father's death; 2nd, his mother's incestuous marriage with his uncle; and 3rd, and chief of all, the revelation of his father's murder, and of the murderer,—

> 'The serpent that did sting thy father's life,
> Now wears his crown;'

and the mission of vengeance entrusted to him by the Ghost, apparently permitted to revisit the earth solely for the purpose of confiding to him this knowledge, and enjoining upon him this duty. All this utterly harassing him—if you will, dismaying him; but not deranging his intellect (I cannot admit that); till he is in the most dire perplexity how to act. All which difficulties, too, the very subtilty of his faculties, and the perfection of his organization, intellectual and moral, only increase and enhance, because on every side he sees objections, and arguments, and possible dangers, and even sins, in whatever he may do, which a blunter intellect, or a duller heart, or less quickened feelings, or a less active conscience would never have seen or felt at all. Thus the very balance of his excellences, and the perfection of his faculties, made him to be poised in inaction, and (as he says himself)—

> 'Thus conscience does make cowards of us all;
> And thus the native hue of resolution
> Is sicklied o'er with the pale cast of thought;
> And enterprises of great pith and moment
> With this regard, their currents turn awry,
> And lose the name of action.'

Add to all this his passionate, tender, true love for the gentle and beautiful Ophelia—and what a new cause of distress is there! Oh, how it cracked his heart-strings to reckon this among the 'trivial fond records' which were now no more to have any part in his life or thoughts! What wonder, then, that these things strove hard to upset his reason (though I must maintain they did not)? or what wonder that to an undiscerning dull world he appears to be full of nothing but an incomprehensible irresolution?"

Mr. Mayow then went through, in some detail, the evidence of the restraining thoughts which addressed themselves to such a mind and heart as Hamlet's; and dwelt upon his manifest unwillingness to kill his uncle, especially until he had tested the Ghost's truth by the device of the play. He admitted—nay, claimed—that even after being satisfied by that proof of the king's guilt, he had yet the utmost abhorrence of his task, and pointed to this trait in his character as the sufficient explanation of the fearful scene, where, finding the king at his prayers, he refuses to kill him, on the professed plea that to do so would be "hire and salary, not revenge." Citing Dr. Johnson's criticism—"This speech in which Hamlet, represented as a virtuous character, is not content with taking blood for blood, but contrives damnation for the man that he would punish, is too horrible to be read or to be uttered,"—Mr. Mayow remarked that the criticism would be just if Dr. Johnson's understanding of the act and its motive were correct. But he thought it well admitted of another and a different reading, wherein we might find one of the most marvellous instances of Shakespeare's surpassing genius, by which he gives us insight into the recesses of Hamlet's mind, and shows us the unlimited resources of his intellect. Here he came upon the king, alone and utterly defenceless before him, when there was no possible difficulty in killing him. As he said,—"Now might I do it pat;" but he had *no mind to do it.* When it came to the point he abhorred the deed. Even though by the test of the play he was wholly satisfied of the king's guilt, yet his old dislike and detestation of the act remained. And yet what plea could he find for not now executing his mission? Here the quickness of his intellect hit upon the only possible hindrance or objection,—that thus to take him at his prayers would be to send him to heaven, and so no vengeance. The thought served its turn. He had found a cause for delay. He let the king escape, and all that follows in the speech, drawing out with horrible distinctness this assumed motive for his sparing him, is but the evidence, *not* of the malignity of his heart and nature, but of the inexhaustible fertility of his imagination and his argumentative powers, by which he could in a moment dress up all which should look like a reason in the perfection of words, as well as in the most absolute completeness and plausibility of invention. After noticing some minor evidences, confirmatory of this view of Hamlet's character, the speaker pointed to the final catastrophe of the play as entirely consistent with it; inasmuch as at the last, even after he knew the fresh and fresh instances of the king's villany, it is more upon excitement and impulse than upon reason or deliberation that he acts, and this, too, upon the new and deep provocation of his mother's death, and when, if he any longer delayed, his mission of retribution could not be executed at all.

The speaker then proceeded at considerable length to the question of Hamlet's madness. Was it real or feigned? Or feigned at first, and in his own intention, but passing on afterwards beyond his own control into a real derangement of mind, even without his knowing it himself, as many great critics—Dr. Conolly for instance, and, in some measure, even Coleridge—seem to think? To this, however, Mr. Mayow could not for a moment assent. He did not believe Hamlet was mad, not in any degree, nor upon any point. He was over-wrought, worn, sick at heart, perplexed, oppressed by all which was laid upon him to suffer and to do; but there was, he thought, no evidence to show, either as to Shakespeare's intention, or as to his own conduct, any perversion of his reason, any cloud upon his intellect, any delusion upon his mind, as to any single matter in which he was concerned. If any maintained the contrary theory, it could not be too much to ask, *what* and *where* was the *delusion*. Of course it was not to be taken as a delusion that he saw the ghost, or received his mission from him. This was a *postulate* of the whole play, and was to be taken for granted by all, as indeed others of whose sanity there was no question, saw and heard him as well as Hamlet. Then by a somewhat lengthened examination of the prince's perception in their true character of all the persons, things, and circumstances around him, the speaker arrived at the conclusion that no single matter could be pointed out in relation to which he was under any delusion whatever.

But beyond such negative proof, there existed many positive proofs of his sanity. Over and above other things, such as his dialogues with Horatio, Rosencrantz, and Guildenstern, with the players and the grave-diggers, the speaker directed attention especially to his soliloquies, and the immediate transition from his feigned disjointedness of mind, to the most sound appreciation of everything, upon his being left alone. In a single moment, said the speaker, he is himself, and shows himself competent to deal with every thought and every circumstance. And this was not the mere coming to a lucid interval, but the having the entire command of himself so as at his own mere will, at all times, and in a moment, to come to this lucid interval, as the advocates of his madness must call it. Among many other instances of this sort and kind, Mr. Mayow noticed, especially, the few words uttered by him, immediately upon the players leaving him subsequently to his first idea of the play. On their going out he says,—"Now I am alone." Those four words seemed to be an absolute proof of sanity, "Now I am alone." The restraint is off me. I know all I have been doing, all I have been acting, and now I can unbend and commune with my soul, and ask it, Why I am so tardy in what I have to do? or, Whether there be not some need for caution and further testing the truth of the ghost's story before committing myself irrevocably to carry out his behest?

The speaker arrived at the same conclusion from an examination of the closet scene with his mother, and particularly the test of sanity which he offers when he says—

> "Ecstacy!
> My pulse, as yours, doth temperately keep time,
> And makes as healthful music: * * *
> * * * * bring me to the test,
> And I the matter will re-word; which madness
> Would gambol from."

Of which, in spite of all cavillers, Mr. Mayow affirmed that he could not doubt Shakespeare himself intended it to be a true test of a sound mind; "and if so," he said, "I will back him to have drawn it rightly, against the world and all other authorities in it, and I hope and think," he added, "that I shall go near to have your suffrages with me in the wager."

The speaker then examined what he believed by the advocates of the insane theory was considered their stronghold,—Hamlet's cruel behaviour, as they affirm, to Ophelia, of which they conceive the only explanation must be that he was mad; in short, that the only way in which to save his character as a gentleman is to give him up as a lunatic. But the speaker thought all this admitted of another and a better explanation. It must not be forgotten, first, that he had made it a special object to get it reported and believed that he was mad. It was part of his scheme, and by his conversation with Ophelia he certainly took means to obtain currency for such report and rumour; whilst, secondly, it must be borne in mind that he felt bound to break off wholly his thoughts of love (what this cost him, the scene afterwards at her funeral showed), and yet he would feel it was even more cruel simply to withdraw himself from her without a reason, than to act as he did, whilst at the same time to explain was impossible. To let his feigned madness then take such a turn of severity and sarcasm as might revolt her mind from him, and almost disgust her with his presence, might well be in his motives for his conduct towards her. At the same time, thirdly, a large part of his dialogue with her, and the extreme severity of his reflections upon women ("to a nunnery go," &c.), may most naturally be traced and set down to the heart-breaking bitterness of his thoughts in connection, not with Ophelia, but with his mother, and to the shame which he felt she had cast on the whole race of women. "All this," said the speaker, after giving various instances of the sarcasms, "seems to me but the natural *outcome* of his most acute and metaphysical mind when once he let it run free into the appearance of being unhinged under the motives which I have mentioned, and when he did not try to repress the disgust with which his mother's conduct had filled him. And oh! it would make me *sick at heart* to believe that all that subtle imagination,—that prompt wit,—that unrivalled perception,—that intense aptitude,—that deep penetration,—that profound philosophy,—that refined taste,—that telling sarcasm,—that sharp repartee,—that unequalled diction,—that awful power,—that quick conscience,—that tender heart,—that heroic temper,—were all but the developments, or at least the accompaniments, of a disturbed fancy and a deranged mind, and not the representation to us by him who alone was able to conceive or draw the picture, of the most perfect intellect which the world has ever witnessed. *But* for Shakespeare himself we well might say that Hamlet is the greatest genius the world has ever known."

He ought to add a word perhaps to show Hamlet's claim to this praise,—to show his wonderful quick-wittedness, versatility, and intellectual power. These were the qualities which in part made it so difficult to understand him, and therefore also to represent him. Let it be observed, he is up to everything, sees everything, knows everything, understands everything, weighs and appreciates everything (aye at a single glance), and is able to excel in everything. He is so ready and perfect in everything, and by instinct so throws himself into every pursuit, that even under the pressure of his great trial he cannot shake off this his nature. However repressed in his communings with himself, it bursts out afresh continually in his intercourse with others. See it in his converse with Horatio, Polonius, Rosencrantz, and Guildenstern; in his directions to the players; in his composition of the scene inserted in the play; in his talk with the grave-diggers; in all he does and says everywhere; not least in the triumphant and though bombastic, yet most true and pathetic outbreak of doggerel upon the termination of the play, and the king's abrupt departure "frighted with false fire."

> "Why, let the strucken deer go weep,
> The hart ungalled play;
> For some must watch, while some must sleep:
> Thus runs the world away."

Now these characteristics were amongst the things which made the great difficulty, almost impossibility, of *acting* Hamlet satisfactorily. The player is disposed to put too much *way*, if it may be so said, on the boat in order to steer it. He cannot combine the interest which Hamlet, from the activity of his mind, still takes in other things with his great concern in his main pursuit and mission. Whereas Hamlet's fancy is so various, his wit so ready, his versatility so great, his interest in all he touches so vivid, that this one strong absorbing current, which the actor gives him as to his mission of vengeance, is to a certain extent contrary to his bent, and to a sort of waywardness which is a part of him, and which is very hard to render. Thus (to take an example) it has been said no player has ever been able to give rightly the words, "Well said, old mole, can'st work so fast i' the earth," so as to combine with the recklessness and levity of the phrase, and the turn of thought shown in it, the due and deep reverence for his father which was an absolute and integral part of Hamlet's nature.

There was one further proof in illustration of all this power and versatile intellect in Hamlet which the speaker was unwilling wholly to pass over, because it was furnished by a test which he did not remember to have seen elsewhere applied. He would assert that Hamlet was so "myriad-minded," so had the intellect, feeling, perceptions of every one else, and that we are so unconsciously conscious of this (if the expression might be used), that we should recognize a very large number of other speeches as natural in his person. He would say, try other people's sayings throughout the other plays in the mouth of Hamlet, and it will be found he might have said them—he would not say *all*, but to an enormous extent—with perfect justness and consistency, though of so very various and different kinds.

And this would be found to be all the more remarkable because, for the most part, Shakespeare's speeches in the mouths of their different speakers are entirely *characteristic*, suitable only to those who utter them. They are not at all, as a rule, interchangeable, and no one but the man who *does* say them *should* say them. So that it has been affirmed, perhaps with a little exaggeration of phrase but yet certainly with an underlying truth, "Were all Shakespeare's plays to be acted at once, they would require upwards of 700 performers, besides crowds and suites, yet of all these personages not one in the least resembles another; there is no repetition; this variety, coupled with the perfect truth to nature in each case, is marvellous, one is almost tempted to say, miraculous."* Now this would be found true to the extent that we ought to be able, when a few lines of any distinctive bearing are recited, even if we do not remember the passage, to give at least a good guess at the speaker. For instance, take the lines:—

. . . "O! that a man might know
The end of this day's business e'er it come;
But it sufficeth that the day will end,
And then the end is known."

Now whose speech is this? Any man perhaps in an anxious panting expectation of some great event might say the first two lines—

. . . "O! that a man might know
The end of this day's business e'er it come."

But the turn of the latter part is peculiar: the almost stoical philosophy; the calm resignation to what shall be; the very turn of the phrase—*not* the end *will be* known, but the end *is* known—points to one man as the speaker—Brutus, before the battle of Philippi. And yet *Hamlet* might have said the words, and given expression to their sentiment in every part. Take other different speakers, and yet the rule would hold good. *Who* but himself would seem able to say the majestic lines of Prospero, describing the lapse and fall of the earth and all that therein is?—

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* 'Shakespeare Memorial.' S. O. Beeton, 1864. P. 47.

> "Our revels now are ended; these our actors,
> As I foretold you, were all spirits, and
> Are melted into air, into thin air;
> And, like the baseless fabric of this vision,
> The cloud-capped towers, the gorgeous palaces,
> The solemn temples, the great globe itself,
> Yea, all which it inherit, shall dissolve,
> And, like this insubstantial pageant faded,
> Leave not a rack behind. We are such stuff
> As dreams are made on, and our little life
> Is rounded with a sleep."

And yet Hamlet might have said this very speech throughout with perfect propriety. Equally the exclamation of Trinculo, creeping under the gabardine of Caliban to avoid the storm—

> "Misery acquaints a man with strange bed-fellows."

Or, again, Falstaff's quaint interpolation—

> "Rebellion lay in his way and he found it."

Or, almost all the wit and imagination of Mercutio, from—

> "O! then I see Queen Mab has been with you."

to—

> "But I'll be hanged, sir, if he wear your livery."

Or the philosophic musings of the melancholy Jacques; or the love-making of Orlando; or the sighing pang of utterance in Rosalind—

> "O! how bitter it is to look into happiness through another man's eyes."

Or the moralizing reflection of that lord in 'All's Well that Ends Well'—

> "The web of our life is a mingled yarn; good and ill together: our virtues would be proud if our faults whipped them not, and our crimes would despair if they were not cherished by our virtues."

Or the whole of Henry V.'s wonderful conversation and argument with the soldiers the night before the battle of Agincourt; or Lear's word—

> "A man more sinned against than sinning."

Or Viola's—

> "She never told her love."

Or even Juliet's (supposing him a few years younger, and brought into the corresponding circumstance), even Juliet's first sweet, musing, despairing confession—

> "My only love sprung from my only hate,
> Too early seen unknown, and known too late."

Or that great line of the wise Ulysses—

> "One touch of nature makes the whole world kin."

Or the sarcastic remark of Thersites dictated by so wide a knowledge of the world—

> "A plague of opinion: a man may wear it on both sides, like a leathern jerkin."

Or the great question and answer of Antony and Cleopatra. Her question—

> "If it be love indeed, tell me how much?"

And his reply—

> "There's beggary in the love that can be reckoned."

Or a thousand other pieces of wit, philosophy, sarcasm, narrative, description, sentiment, which might flow from Hamlet, whilst the converse will hardly hold good with *one*, there being no character in all the plays who could for a moment sustain Hamlet's part, or with any plausibility give forth his utterances.

"Here then," said the speaker, summing up his remarks, "here I believe we have the masterpiece of all human composition. Take Poetry, as the highest kind of all art; take Dramatic poetry as the highest kind of Poetry; take Tragedy as the highest sort of Drama; take Shakespeare as the highest writer of Tragedy; take 'Hamlet' as the very top and crown of his tragedies and of all his writings, and you come to this result. So full and great a result that our ordinary feeble intellects toil after him, in great measure in vain; so rich a mine as never to be exhausted; so true and divine a moral as never to be out of date. Yes, a moral, though not a strained or forced one—teaching just as God teaches in the world around us. I say teaching that this world is not the place for exact compensation or retribution, where things crooked are made straight, or things rough made smooth; but that these imperfections, inequalities, and dislocations are but the trials of our faith in a world that is a place of trial. As one of our greatest poets has said—

> 'There's something in this world amiss,
> Shall be unriddled by-and-bye.'

Trials of our faith, purifiers of our fallen nature, and the argument and assurance to us of a better world, where all which is, or seems to us to be, unequal or unintelligible shall be both set right and explained: all ordered and weighed in the unerring balances of God. As it is said, in the fullest conviction that all is ordered by Him:—

> 'We defy augury! There's a special providence
> In the fall of a sparrow . . . . . The readiness is all.'

Or again, that it is not only ordered, but ordered right:—

> 'There's a divinity doth shape our ends,
> Rough-hew them how we will.'"

[M. W. M.]

WEEKLY EVENING MEETING,

Friday, February 28, 1868.

Sir Henry Holland, Bart. M.D. D.C.L. F.R.S. President,
in the Chair.

A. Vernon Harcourt, Esq. M.A.
SECRETARY OF THE CHEMICAL SOCIETY.

On the Rate at which Chemical Actions take place.

The science of Chemistry may be defined as the science which investigates the relations of the different kinds of matter one to another. The conception of different kinds of matter,—each of which has its particular character, its own colour and crystalline form, its own hardness and brittleness or the reverse, its own conducting powers, its own specific heat and specific gravity, and many other peculiarities of its own, and each of which is homogeneous, the smallest particle having all these properties equally with the largest mass,—is the fundamental conception of Chemistry.

And the whole world to a chemist is only a mixture of such different kinds of matter, whose mode of aggregation has been and is being determined by physical and vital forces which are foreign to his science, but whose resemblances and differences, and whose changes under changed conditions or by contact one with another, form the subject of his study.

In the study of any chemical change there are two things to be discovered: first, the *result* of the change,—what kinds of matter have ceased to exist and what have come into existence; and secondly, the *course* of the change; as to which such inquiries as the following present themselves,—at what rate does the change occur, and under what conditions? Is it simple, or does it consist of several changes? Are these dependent or independent, successive or simultaneous?—with many others of a more hypothetical kind as to the molecular nature of the change. A familiar example of this twofold nature of chemical inquiry may be drawn from the case of a fire, a chemical change which has been more watched than any other. We know all that is to be known as to the result of the change, when we have discovered that the coals are a mixture of various hydrocarbons with a small quantity of metallic salts, that the air is a mixture of oxygen and nitrogen, and that when the fire has burnt out, there exists, instead of so much coal and so much air, a quantity of carbonic acid and water, the salts, which form the ash, and the nitrogen remaining mainly as they were. But there is still much besides this to be found out as to the burning of the fire. How, for example, is the rate at which it burns affected by the draught, or by the density of the air, or by the breaking up of the fuel, or by access of the sun's rays? What are the substances, formed from the heated coal, which actually burn? Does the reduction of the products of combustion by carbon play an important part in the phenomenon? Such questions as these relate to the course of the chemical change.

The two lines of inquiry thus indicated have been pursued with very unequal vigour. The study of the results of chemical action has engrossed the attention of chemists almost to the exclusion of the study of their course. And, indeed, so great is the number of different kinds of matter, all capable of undergoing a multitude of changes by the action of heat or electricity or by contact with others, giving rise thus to new kinds of matter capable of similar changes, that this part of the science appears absolutely boundless. The direction which chemistry has taken in consequence of this superabundance of materials may, perhaps, be contrasted with that taken by physical science. If the number of distinct physical forces met with in nature, such as gravity, magnetism, electricity, heat, light, &c., instead of being quite a small number, had been a large number, and these forces had proved to be convertible not only one into another but into an infinite variety of other distinct forces, physical experimentalists might have occupied themselves wholly with establishing the transmutations of one kind of force into another and creating new modes of force, instead of studying minutely, as they have done, the conditions under which the existing forces are produced, and the laws which govern their distribution and transformation.

It is, however, not only the vastness of the chemical field, and the particular satisfaction which so solid a result as the creation of a new kind of matter brings to the mind of the investigator, which has led to the neglect of the study of the course of chemical changes. This study is beset with peculiar difficulties, and indeed, out of the vast number of chemical changes whose results are known, there are but very few whose course can readily be observed. The principal reason of this is the velocity with which such changes take place; and this velocity is apt to be the greatest in the case of the simple chemical actions which are most suitable for investigation. Either, then, we must contrive some mode of estimating a very great velocity, as has been done for the measurement of the rate at which light and electricity travel, or we must select a change—and this the variety of chemistry makes possible—which proceeds at a rate convenient for observation.

Examples of the different velocity of chemical changes are furnished by the precipitation of a barium and of a calcium salt from their solutions upon the addition of a sulphate. With the former, the change is apparently instantaneous. The result is known, but the course cannot be observed. With the latter, the change is gradual, and it would be possible to determine its rate at different temperatures and with different quantities of the two salts in solution.

The decomposition of a hyposulphite in an acid solution is another example of a gradual, observable change.

We may compare, also, the reduction of a chromate by a sulphite and by an oxalate. The former occupies no appreciable time; the actual time is, doubtless, greater in a more dilute solution and at a lower temperature, but we cannot discern any difference. But with an oxalate for reducing agent, though the final result of the change is the same, the action takes a long time to accomplish itself, and it would be quite practicable to observe in what way different circumstances affect its rate.

But in order to discover the laws which govern the rate of any chemical change, some exact mode of measuring the rate is necessary. It remains to show how this may be accomplished in certain cases.

A solution of ammonium nitrite, heated to a temperature of about 80° C. in a flask provided with a gas delivery tube, gives off a quantity of nitrogen, which may be collected over the pneumatic trough. By keeping the temperature constant, and collecting the gas evolved during successive equal intervals of time in similar cylinders, it is possible at once to show the regular diminution in the volume of gas which is caused by the constant diminution of the quantity of salt in solution. And by making the experiment and measuring the quantities of gas with accuracy, it would be possible to discover the relation between the amount of change going on at any moment and the amount of salt in solution, and also, by making the experiment at different temperatures, to discover how the temperature of the solution affects the rate at which the action takes place.

The reduction of a permanganate by an oxalate in an acid solution furnishes another case of a gradual measurable change, and has been more fully studied. Here it is possible to start the change at any moment by adding the measured quantity of permanganate to the other ingredients and mixing rapidly. It is also possible to stop it at any moment by adding a solution of iodide to the mixture; and the iodine which is set free by the action of the residual permanganate corresponds to it in quantity and can readily be estimated. By making a number of such experiments, differing from one another only in the time during which the gradual change is allowed to proceed, its course may be traced throughout with any required degree of minuteness. The results obtained in many series of such experiments are given in the 'Philosophical Transactions for 1866,' p. 206. The general conclusion to which they lead is that the total amount of change occurring at any moment is directly proportional, all other conditions being alike, to the amount of permanganate in the solution.

The last chemical change which has been investigated from this point of view, is that which takes place when dilute acid solutions of an iodide and a dioxide, such as barium or sodium dioxide, are mixed together. By arranging suitably the dilution, acidity, and temperature of the solution, the change may be made to proceed at any rate that is most convenient for measurement. One of the products of the change is iodine, a substance for which we have, in its action on starch, a most delicate test. By bringing a small known quantity of hyposulphite into the liquid, all the iodine that is formed by the gradual reaction of peroxide and iodide is reconverted into iodide, and this continues till iodine enough has been formed to remove all the hyposulphite. As soon as the last particle of hyposulphite has been removed (converted into tetrathionate), free iodine appears in the solution, and the moment of its appearance may be noted by carefully watching the colour of the liquid. By adding successive quantities of hyposulphite, and observing the interval which elapses between successive reappearances of the blue colour of the iodide of starch, it is possible accurately to determine the rate at which the change is proceeding. An account of a number of experiments made in this way, and of their results, is to be found in the 'Philosophical Transactions for 1867,' p. 117. Each set of observations determines at what rate the dioxide is reduced under certain definite conditions; and by making different series of experiments, in which the several conditions affecting the rate of change are systematically varied, it is possible to discover the laws of connection between each of the conditions and the amount of change. Having discovered these laws, our knowledge of the change is so far complete, and we can predict with certainty the time that would be required for any given amount of change under any given circumstances.

The following propositions embody the principal conclusions to which the examination of these cases of gradual chemical change has led:—

1. The rate at which a chemical change proceeds is constant under constant conditions, and is independent of the time that has elapsed since the change commenced.

2. When any substance is undergoing a chemical change, of which no condition varies, excepting the diminution of the changing substance, the amount of change occurring at any moment is directly proportional to the quantity of the substance.

3. When two or more substances act one upon another, the amount of action at any moment is directly proportional to the quantity of each of the substances.

4. When the rate of any chemical change is affected by the

presence of a substance, which itself takes no part in the change, the acceleration or retardation produced is directly proportional to the quantity of the substance.

5. The relation between the rate of a chemical change occurring in a solution, and the temperature of the solution, is such, that for every additional degree the number expressing the rate is to be multiplied by a constant quantity.

[A. V. H.]

GENERAL MONTHLY MEETING,

Monday, March 2, 1868.

William Spottiswoode, Esq., M.A. F.R.S. Treasurer and Vice-President, in the Chair.

Frederick Dale Banister, Esq.
Peter Henry Berthon, Esq.
Miss Martha Conway
Mrs. Ernest Hankey
Charles Hart, Esq.
Mrs. Henry Huth
Benjamin Isaac, Esq.
Morton Latham, Esq.
Mrs. Le Breton
Robert Longsdon, Esq.
Alexander Macmillan, Esq.
William Ingram Marter, Esq.
Thomas Parry, Esq. M.P.
Mrs. Frederick Pollock
Robert Prance, Esq.
Miss Edith Prance
Edward Smirke, Esq.
John Palmer Stocker, Esq.
Charles H. Lardner Woodd, Esq.

were *elected* Members of the Royal Institution.

The following Repeals and Enactments, in alteration of the Bye-Laws of the Royal Institution, were passed :—

In Chapter II., Art. 6, for the word "*five*," in the third line, substitute the word "*ten*."

In the fifth line of the same Article insert the word "*subsequent*" between the words "*each*" and "*year*."

In the same Chapter repeal Article 8.

In the same Chapter repeal Article 23.

In Chapter XI., Art. 2, for the words, "*The Composition of Members (except the* "*sum of Ten Guineas out of every Composition paid by any Member not having been* "*previously a Life Subscriber to be carried over to the Laboratory Fund) shall upon* "*payment thereof be invested*," substitute the following words :—"*Out of the Com-* "*position of each Member the sum of Forty Guineas shall on payment thereof be* "*invested*."

In the same Chapter, in the part distinguished as "*thirdly*," in Art. 3, for the words, "*and in aid of the Library and Mineralogical Fund*," substitute "*and generally in furtherance of the objects and welfare of the Institution.*"

In Chapter XIII., Art. 4, for the words, "*pay the sum of One Guinea into the Library and Mineralogical Fund, Chap. XX., Art. 4*," substitute "*pay the further sum of One Guinea in furtherance of the objects and welfare of the Institution.*"

In Chapter XX. repeal the following Articles, viz.:—

Article 4.
Article 5.

In the same Chapter, in Art. 6, for the words, "*The Patrons shall cause fair and just Accounts and Registers, in writing, to be kept of all receipts, payments, and transactions by them, and take care that they be annually made up*," substitute the following words:—"*The Managers shall cause fair and just Accounts, in writing, to be kept of all receipts and payments of and relating to the contributions of One Hundred Pounds above in this Chapter mentioned, and shall take care that they, separate and distinct from the other Accounts of the Institution, be annually made up.*"

In Chapter XXI. repeal the following Articles, viz:—

Article 3.
Article 4.
Article 5.
Article 6.
Article 7.
Article 8.
Article 9.

In Article 10 of this Chapter, repeal the words, "*to the support of the Laboratory*," in the 3rd and 4th lines thereof, and the words "*Laboratory Fund*" in the 5th line thereof; and substitute the words, "*to the support of the Laboratory*," for the said words, "*Laboratory Fund*," in such 5th line.

In Article 11 of this Chapter, for the words, "*Laboratory Fund*," substitute "*to the support of the Laboratory.*"

N.B.—The above references to the Bye-laws are to the edition of 1835, except as relates to Chapter XX., as to which reference is made to the Minutes of the General Monthly Meeting of the Members of the 7th February, 1848.

The special thanks of the Members were returned for the following addition to "the Donation Fund for the Promotion of Experimental Researches":—

Arthur Giles Puller, Esq. (*3rd donation*) . . . £21 0 0

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same:—

FROM

Her Majesty's Government (through the Director-General of the Geological Survey)—
J. E. Portlock, Report on the Geology of Londonderry. 8vo. 1843.
Mineral Statistics of the United Kingdom, 1865 and 1866. 8vo. 1866–7.
British Organic Remains. Decade XII. 8vo. 1866.

Asiatic Society of Bengal—Proceedings, 1867, Nos. 8–10. 8vo.
Journal, No. 140. 8vo. 1867.

Astronomical Society, Royal—Monthly Notices, Vol. XXVIII. No. 3. 8vo. 1867–8.

Bavarian Academy of Science, Royal—Sitzungsberichte, 1867. Band II. Heft 2, 3. 8vo.
Annalen der Sternwarte, München. Band XV., XVI. 8vo. 1867.

British Architects, Royal Institute of—Sessional Papers, Nos. 6, 7. 4to. 1867–8.

Chemical Society—Journal for Feb. 1868. 8vo.

Editors—American Journal of Science, Jan. 1868. 8vo.
Artizan for Feb. 1868. 3to.
Athenæum for Feb. 1868. 4to.
British Journal of Photography for Feb. 1868. 4to.
Chemical News for Feb. 1868. 4to.
Engineer for Feb. 1868. fol.
Geological and Natural History Repository. Feb. 1868. 8vo.
Horological Journal for Feb. 1868. 8vo.
Journal of Gas-Lighting for Feb. 1868. 4to.
Mechanics' Magazine for Feb. 1868. 8vo.
Pharmaceutical Journal for 1868.
Photographic News for Feb. 1868. 4to.
Practical Mechanics' Journal for Feb. 1868. 4to.
Revue des Cours Scientifiques et Littéraires. Feb. 1868. 4to.

Enderby, Charles, Esq. F.R.S. M.R.I. (*the Author*)—A Cure for Ireland's Wrongs. (K 95) 8vo. 1868.

Franklin Institute—Journal, No. 505. 8vo. 1868.

Geographical Society, Royal—Proceedings, Vol. XII. No. 1. 8vo. 1868.

Geological Society—Quarterly Journal, No. 93. 8vo. 1868.

Kelly, F. W. Esq. (*the Author*)—Poems. 2 vols. 12mo. 1842–61.

Linnean Society—Journal, Nos. 39, 42. 8vo. 1868.

Mechanical Engineers' Institution—Proceedings, June, 1867. Part I. 8vo.

Meteorological Society—Proceedings, No. 34. 8vo. 1868.

Photographic Society—Journal, No. 190. 8vo. 1868.

Royal Society of London—Proceedings, No. 98. 8vo. 1867.

Saxon Society of Sciences, Royal—Abhandlungen, Band XIII. Nos. 4, 5. 8vo. 1867.
Berichte, 1866, Nos. 4, 5. 1867, Nos. 1, 2. 8vo.

Society of Arts—Journal for Feb. 1868. 8vo.

Surgeon-General United States Army—Annual Report. 8vo. 1867.

Symons, G. J. (*the Author*)—Symons' Monthly Meteorological Magazine, Feb. 1868. 8vo.

WEEKLY EVENING MEETING,

Friday, March 6, 1868.

Sir Henry Holland, Bart. M.D. D.C.L. F.R.S. President,
in the Chair.

W. Kingdon Clifford, Esq. B.A. Cantab.

On some of the Conditions of Mental Developement.

If you will carefully consider what it is that you have done most often during this day, I think you can hardly avoid being drawn to this conclusion: that you have really done nothing else from morning to night but *change your mind.* You began by waking up. Now that act of waking is itself a passage of the mind from an unconscious to a conscious state, which is about the greatest change that the mind can undergo. Your first idea upon waking was probably that you were going to rest for some time longer; but this rapidly passed away, and was changed into a desire for action, which again transformed itself into volition, and produced the physical act of getting up. From this arose a series of new sensations; that is to say, a change of mind from the state of not perceiving or feeling these things to the state of feeling them. And so afterwards. Did you perform any deliberate action? There was the change of mind from indecision to decision, from decided desire to volition, from volition to act. Did you perform an impulsive action? Here there is the more sudden and conspicuous change marked by the word *impulsive;* as if your mind were a shuttlecock, which has its entire state of motion suddenly changed by the *impulse* of the battledore: conceive the shuttlecock descending quite regularly with a gentle corkscrew motion—the battledore intervenes—instantaneously the shuttlecock flies off in a totally unexpected direction, having apparently no relation to its previous motion; and you will see how very apt and expressive a simile you use when you speak of certain people as having an *impulsive temperament.* Have you felt happy or miserable? It was a change in your way of looking at things in general; a transition, as Spinoza says, from a lower to a higher state of perfection, or *vice versâ.* In a word, whatever you have done, or felt, or thought, you will find upon reflection that you could not possibly be conscious of anything else than a change of mind.

But then, you will be inclined to say, this change is only a small thing after all. It does not penetrate beyond the surface of the mind, so to speak. Your character, the general attitude which you take up with regard to circumstances outside, remains the same throughout the day: even for great numbers of days. You can distinguish between individual people to such an extent that you have a general idea of how a given person will act when placed in given circumstances. Now for this to be the case, it is clear that each person must have retained his individual character for a considerable period, so that you were able to take note of his behaviour in different cases, to frame some sort of general rules about it, and from them to calculate what he would do in any supposed given case. But is it true that this character or mark by which you know one person from another is absolutely fixed and unvarying? Do you not speak of the character of a child growing into that of a man; of a man in new circumstances being quite a different person from what he was before? Is it not regarded as the greatest stroke of art in a novelist that he should be able not merely to draw a character at any given time, but also to sketch the growth of it through the changing circumstances of life? In fact, if you consider a little further, you will see that it is not even true that a character remains the same for a single day: every circumstance, however trivial, that in any way affects the mind, leaves its mark, infinitely small it may be, imperceptible in itself, but yet more indelible than the stone-carved hieroglyphics of Egypt. And the sum of all these marks is precisely what we call the character, which is thus itself a history of the entire previous life of the individual; which is therefore continually being added to, continually growing, continually in a state of change.

Let me illustrate this relation by the example of the motion of a planet. People knew, ages and ages ago, that a planet was a thing constantly moving about from one place to another; and they made continual attempts to discover the *character* of its motion, so that by observing the general way in which it went on, they might be able to tell where it would be at any particular time. And they invented most ingenious and complicated ways of expressing this character:

> "Cycle on epicycle, orb on orb,"

till a certain very profane king of Portugal, who was learning astronomy, said that if *he* had been present at the making of the Solar system, he would have tendered some good advice. But the fact was that they were all wrong, and the real case was by no means so complicated as they supposed it to be. Kepler was the first to discover what was the real character of a planetary orbit; and he did this in the case of the planet Mars. He found that this planet moved in an ellipse or oval curve round the sun which was situated rather askew near the middle. But upon further observation, this was found to be not quite exact; the orbit itself is revolving slowly round the sun, it is getting elongated and then flattened in turns, and even the plane in which the motion takes place sways slowly from side to side of its mean position. Thus you see that although the elliptic character of the motion does represent it with considerable exactness for a long time together, yet this character itself must be regarded as incessantly in a state of gradual change. But the great point of the comparison—to aid in the conception of which, in fact, I have used the comparison at all—is this: that for no two seconds together does any possible ellipse *accurately* represent the orbit. It is impossible for the planet to move a single inch on its way, without the oval having slightly turned round, become slightly elongated or shortened, and swayed slightly out of its plane; so that the oval which accurately represented the motion at one end of the inch would not accurately represent the motion at the other end. The application is obvious. In like manner it is true that the character which will *roughly* represent the law of a man's actions for some considerable time, will not *accurately* represent that law for two seconds together. No action can take place in accordance with the character without modifying the character itself; just as no motion of a planet could take place along its orbit without a simultaneous change in the orbit itself.

But I will go even further. Historians are accustomed to say that at any given point of a nation's history there is a certain general type which prevails among the various changes of character which different men undergo. There is some kind of law, they say, which regulates the slow growth of each character from childhood to age; so that if you compared together all the biographies you would find a sort of family likeness, suggesting that some common force had acted upon them all to make these changes. This force they call the Spirit of the Age. The spirit, then, which determines all the changes of character that take place, which is, therefore, more persistent than character itself, is this, at last, a thing absolutely fixed, permanent, free from fluctuations? No: for the entire history of humanity is an account of its continual changes. It tells how there were great waves of change which spread from country to country, and swept over whole continents, and passed away; to be succeeded by similar waves. No history can be philosophical which does not trace the origin and course of these: things far more important than all the kings and rulers and battles and dates which some people imagine to be history.

To recapitulate. The mind is changing so constantly that we only know it by its changes. The law of these changes, which we call character, is also a thing which is continually changing though more slowly. And that law or force which governs all the changes of character in a given people at a given time, which we call the spirit of the age, this also changes, though more slowly still.

Now it is a belief which, whether true or not, we are all of us constantly acting upon, that these changes have some kind of fixed relation to the surrounding circumstances. In every part of our conduct towards other people we proceed constantly upon the assumption that what they will do is to a certain extent, and in some way or other, dependent upon what we do. If I want a man to treat me with kindness and respect, I have to behave in a certain way towards him. If

I want to produce a more special and defined effect, I have recourse to threats or promises. And even if I want to produce a certain change of mind in myself, I proceed upon the same assumption that in some way or other, and to a certain extent, I am dependent on the surrounding circumstances. People tie knots in their handkerchiefs to make themselves remember things; they also read definite books with a view of putting themselves into definite mental states or moods; and attempts are constantly made to produce even a further and more permanent effect, to effect an alteration in character. What else is the meaning of schools, prisons, reformatories, and the like? Some have actually gone further than this: there have not been wanting enterprising and far-seeing statesmen who have attempted to control and direct the Spirit of the Age. Now in all these cases in which we use means to an end, we are clearly proceeding on the assumption that there is some fixed relation of cause and effect, in virtue of which the means we adopt may be antecedently expected to bring about the end we are in pursuit of. We are all along assuming, in fact, that changes of mind are connected by some fixed laws or relations with surrounding circumstances. Now this being so, since every mind is thus continually changing its character for better or worse, and since the character of a race or nation is subject to the same constant change; since also these changes are connected in some definite manner with surrounding circumstances; the question naturally presents itself, What is that attitude of mind which is likely to change for the better? All the individuals of a race are changing in character, all changing in different directions, with every possible degree of divergence; also the average character itself, the Spirit of the Age, is either changing in some one definite direction, or tending to split into two different characters: an individual, therefore, may be going with the race or dropping out of it; a portion of the race may be going right or wrong. Let us suppose that some portion of the race is going right and improving: the question is, in what way are we to distinguish that individual who is improving with the race, from the others who are either dropping out of the march altogether or going wrong?

Now what I have proposed to myself to do to-night is this, merely to suggest a method by which this question may ultimately be answered. I shall also endeavour afterwards to point out what I conceive to be one or two results of this method: but this part will be of minor importance; the results depend upon my application of the method, can be only partially true, and may be wholly false; the method itself I believe to be altogether a true one, and one which must ultimately lead to the correct results.

It consists in observing and making use of a certain analogy, namely, the analogy between the mind and the visible forms of organic life. You know that every animal and every plant is constantly going through a series of changes. The flower closes at night and opens in the morning; trees are bare in winter and covered with leaves in summer; while the growth of every organism from birth to maturity cannot fail to strike you as a forcible illustration of the gradual change of character in the human mind. In fact, it is the peculiarity of living things not merely that they change under the influence of surrounding circumstances, but that any change which takes place in them is not lost but retained, and, as it were, built into the organism to serve as the foundation for future actions. If you cause any distortion in the growth of a tree and make it crooked, whatever you may do afterwards to make the tree straight, the mark of your distortion is there; it is absolutely indelible; it has become part of the tree's nature, and will even be transmitted in some small degree to the seeds. If, however, you take a piece of inanimate matter—a lump of gold, say, which is yellow and quite hard—you melt it, and it becomes liquid and green—here an enormous change has been produced; but let it cool; it returns to the solid and yellow condition, and looks precisely as before—there is no trace whatever of the actions that have been going on. No one can tell by examining a piece of gold how often it has been melted and cooled in geologic ages by changes of the earth's crust, or even in the last year by the hand of man. Anyone who cuts down an oak can tell by the rings in its trunk how many times winter has frozen it into widowhood and summer has warmed it into life. A living being must always contain within itself the history not merely of its own existence but of all its ancestors. Seeing then that in its continual changes and in the preservation of the records of those changes every organism resembles the mind, so that to this extent they belong to the same order of phenomena, may we not reasonably suppose that the laws of change are alike, if not identical, in the two cases? This is of course a mere supposition, not deducible from anything which we have yet observed, which requires therefore to be tested by facts. I shall endeavour to show that the supposition is well founded; that such laws of change as have been observed in animals and plants do equally hold good in the case of the mind. I shall then endeavour to find out what we mean by higher and lower in the two cases, and to show, in fact, that we mean much the same thing. Supposing all this to have been done, the question will have been stated in a form which it is possible to answer. I shall then make an attempt to give part of the answer to it.

In investigating the laws of change of organic beings I shall make use of what is called the evolution-hypothesis, which, as applied to this subject, is much the same thing as the Darwinian theory, though it is not by any means tied down to the special views of Mr. Darwin. But I shall use this merely as an hypothesis; and the validity of the method of investigation which I have suggested is entirely independent of the truth of that hypothesis. If you will pardon me for a short time, I should like to illustrate somewhat further what I mean by this.

When Kepler found out what was the form of the orbit described by the planet Mars, he thought that the planet was driven by some force which acted in the direction in which the planet was going. I have known people who learned a certain amount of astronomy for nautical purposes, whose ideas were very similar to those of Kepler. They thought that the sun's rotation was what caused the planets to revolve about him, just as if you spin a teaspoon in the middle of a cup of tea, it makes the bubbles go round and round. But Newton discovered that the real state of the case was far different. If you fasten a ball on to the end of an elastic string, and then swing it round and round, you can make the ball describe an orbit very similar to that of the planet, so that your hand is not quite in the centre of it. Now here the pulling force does not act in the direction in which the ball is going, but always in the direction of your hand, and yet the ball revolves about your hand and never actually comes to it. Newton supposed that the case of the planet was similar to that of the ball; that it was always pulled in the direction of the sun, and that this attraction or pulling of the sun produced the revolution of the planet in the same way that the traction or pulling of the elastic string produces the revolution of the ball. *What* there is between the sun and the planet that makes each of them pull the other, Newton did not know; nobody knows to this day; and all we are now able to assert positively is that the known motion of the planet is precisely what would be produced if it were fastened to the sun by an elastic string, having a certain law of elasticity. Now observe the nature of this discovery, the greatest in its consequences that has ever yet been made in physical science:—

I. It begins with a hypothesis, by supposing that there is an analogy between the motion of a planet and the motion of a ball at the end of a string.

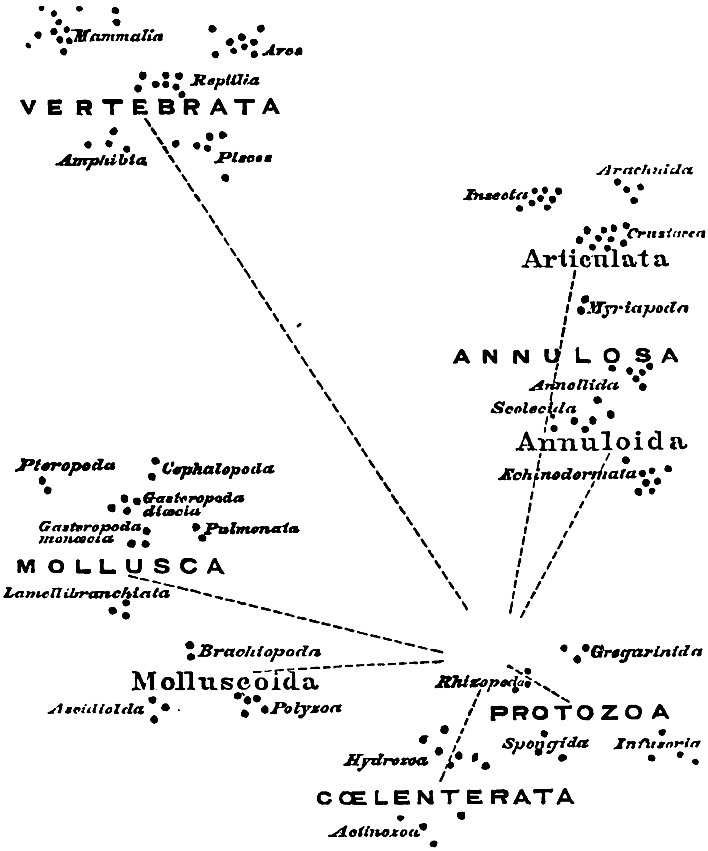
II. Science becomes independent of the hypothesis, for we merely use it to investigate the properties of the motion, and do not trouble ourselves further about the cause of it.

I will take another example. It has been supposed for a long time that light consists of waves transmitted through an extremely thin ethereal jelly that pervades all space; it is easy to see the very rapid tremor which spreads through a jelly when you strike it at one point. From this hypothesis we can deduce laws of the propagation of light, and of the way in which different rays interfere with one another, and the laws so deduced are abundantly confirmed by experiment. But here also science kicks down the ladder by which she has risen. In order to explain the phenomena of light it is not necessary to assume anything more than a periodical oscillation between two states at any given point of space. . *What* the two states are nobody knows; and the only thing we can assert with any degree of probability is that they are *not* states of merely mechanical displacement like the tremor of a jelly; for the phenomena of fluorescence appear to negative this supposition. Here again, then, the same two remarks may be made. The scientific discovery appears first as the hypothesis of an analogy; and science tends to become independent of the hypothesis.

The theory of heat is another example. If you hold one end of a poker in the fire, the other end becomes hot, even though it is not exposed to the rays of the fire. Fourier, in trying to find the laws of this spread of heat from one part of a body to another part, made the hypothesis that heat was a fluid which flowed from the hot end into the cold as water flows through a pipe. From this hypothesis the laws of conduction were deduced; but in the process it was found that the very same laws would flow from other hypotheses. In fact, whatever can be explained by the motion of a fluid can be equally well explained either by the attraction of particles or by the strains of a solid substance; the very same mathematical calculations result from the three distinct hypotheses; and science, though completely independent of all three, may yet choose one of them as serving to link together different trains of physical inquiry.

Now the same two remarks which may be made in all these cases apply equally to the evolution hypothesis. It is grounded on a supposed analogy between the growth of a species and the growth of an individual. It supposes, for instance, that the race of crabs has gone through much the same sort of changes as every crab goes through now, in the course of its formation in the egg, its pristine shape utterly unlike what it afterwards attains, its gradual metamorphosis and formation of shell and claws. By this analogy the laws of change are suggested, and these are afterwards checked and corrected by the facts. But as before, science tends to become independent of hypothesis. The laws of change are established for present and finitely distant times; but they give us no positive information about the origin of things. So, therefore, if I make use of this hypothesis to represent to you the laws of change that are deduced from it; you will see that the truth of those laws and the conclusions which may be drawn from them are in no way dependent on the truth of the hypothesis.

There are certain errors current about the nature of the evolution-theory which I wish particularly to guard against. In the first place it is very commonly supposed that all existing animals can be arranged in one continuous chain, from the highest to the lowest; that the transition is gradual all through, and that nature makes no jumps. This idea was worked out into a system of classification by Linnæus, and survived among naturalists until the time of Cuvier. "They were bent," says Agassiz, "upon establishing one continual uniform series to embrace all animals, between the links of which it was supposed there were no unequal intervals." . . . "They called their system *la chaine des êtres.*" The holders of the Darwinian theory are then supposed to believe that all these forms grew out of one another, beginning with the lowest and ending with the highest; so that any one animal of the series has in the course of its evolution passed through all the lower forms. And as the species is thus supposed to have grown up through the chain, and the lower species to be continually growing into the higher, so it is imagined that every individual creature, in the course of its production, passes through the lower adult forms; that a chicken, for instance, while it is being formed in the egg, becomes in succession a snail, an insect, a fish, and a reptile, before it becomes a bird. Now that all these ideas are entirely wrong, I need hardly remind you; and I have mentioned them in order that there may be no mistake about the theory which I am using as an analogy. So far is it from being possible to arrange existing organisms in a single line or chain, that they cannot be adequately represented even in the manner which is attempted in the following diagram taken from Spencer's 'Principles of Biology,' vol. i., p. 303.


Mammalia
Aves
Reptilia
VERTEBRATA
Amphibia
Pisces
Arachnida
Insecta
Crustacea
Articulata
Myriapoda
ANNULOSA
Annelida
Scolecida
Annuloida
Echinodermata
Pteropoda
Cephalopoda
Gasteropoda dioecia
Gasteropoda monoecia
Pulmonata
MOLLUSCA
Lamellibranchiata
Brachiopoda
Gregarinida
Molluscoida
Rhizopoda
Ascidioida
Polyzoa
PROTOZOA
Spongida
Infusoria
Hydrozoa
CŒLENTERATA
Actinozoa

In the next place, no existing organism could possibly grow into any other. What is really supposed is this:—that if you went back a million years or so, and made a picture like this one, representing the forms that existed then, no single spot which is covered in one figure would be covered in the other; but the general arrangement would be very similar, except that all the groups would be nearer to the centre or radiant point, and therefore nearer to each other. And if you made a third picture, representing the state of things another million years or so further back, then they would be still nearer together; and at a distance of time too vast to be represented, they would all converge into this radiant point. So the theory is that at that stupendous distance of time all species were alike, mere specks of jelly: that they gradually diverged from each other and got more and more different, till at last they attained the almost infinite variety that we now find. If you will imagine a tree with spreading branches, like an oak; then the outside leaves at any time may be taken to represent all the existing species at a given time. It is quite impossible to arrange them in any serial order. As the tree grows, the outer leaves diverge, and get further from the trunk and from each other; and two extremities that have once diverged never converge and grow together again. But even this simile is insufficient; for species may diverge in a far greater variety of directions than the branches of a tree. Space has not dimensions enough to represent the true state of the case.

Von Baer's doctrine of developement is illustrated by the same figure. If you took embryos of polypes, and snails, and cuttle-fish, and insects, and crabs, and fish and frogs, and if you could watch their gradual growth into these several animals: at first they would be all absolutely alike and indistinguishable. Then, after a little while, you would find that they might be sorted off into these four great classes. Afterwards these groups might be divided into smaller groups, representing orders; then these into families and genera; last of all would appear those differences which would separate them into species.

The evolution-hypothesis, then, represents a *race* of animals or plants as a thing slowly changing: and it also represents these changes as connected by fixed laws with the action of the surrounding circumstances, or, as it is customary to say, the environment. Now the action of the environment on a race is of two kinds, direct and indirect. That part which is called direct action is very easily understood. There is no difficulty in seeing how changes of climate might produce changes in the colour of the skin, or how new conditions which necessitated the greater use of any organ would lead to the increase of that organ, as we know that muscles may be made to swell with exercise; and changes thus made habitual would in time be inherited. But the indirect action of the environment, which is called natural selection, is still more important. The mode of its operation may be seen from an example. There are two butterflies in South America, nearly resembling one another in form, but one of which has a very sweet taste and is liked by the birds, while the other is bitter and distasteful to them. Now suppose that, for some reason or other, sweet butterflies were occasionally produced with markings similar to the bitter ones, these, being mistaken by the birds for bitter ones, would run less chance of being eaten, and therefore more chance of surviving and leaving offspring. If this peculiarity of marking is at all inheritable, then the number of sweet butterflies with bitter marks will in the next generation be greater in proportion to the whole number than before; and, as this process goes on, the sweet butterflies which retain their distinguishing marks will be all weeded out by the birds, and the entire species will have copied the markings of the bitter species. This has actually taken place: the one species has mimicked the markings of the other. Here we see the working of Natural Selection. Any variation in an individual which gives him an advantage in the struggle for life is more likely to be transmitted to offspring than any other variation, because the individual is more likely to survive; so that nature gradually weeds out all those forms which are not suited to the environment, and thus tends to produce equilibrium between the species and its surrounding circumstances. Changes, then, are produced in a species by the selection of advantageous changes which happen to be made in individuals. Now there are three kinds of change that are produced in individuals: change of size, or growth; change of structure, that is to say, change in the shape and arrangement of the parts, as when the cartilaginous skeleton of an infant becomes hardened into bone; and change of function, that is to say, change in the use which is made of any part of the organism. I have one or two remarks to make about the first of these, namely, growth, or change of size. Every organism is continually taking in matter through the external surface to feed the inside. A certain quantity of this is needed to make up for the waste that is continually going on. But let us suppose, to begin with, that an organism has more surface than it absolutely wants to make up for waste, then a certain portion of the assimilated matter, or food, will remain over, and the organism will increase in size. But, you say, if this is all that is meant by growth, why does it not go on for ever? The explanation is very simple. I take this cube, which has six sides, each a square inch; let us suppose it to represent an animal, and imagine, to begin with, that two of the sides by themselves are capable of feeding the whole mass, then the nutrition taken in by the other four sides is left over, and the mass must increase in size. Imagine it now grown to twice the linear dimensions, that is to say, to a cube every side of which is two inches. The mass to be fed is now eight times what it was, while the surface is only four times as great; of the twenty-four square inches of surface sixteen are taken up with feeding the mass, while only eight, or one-third, are left to supply the materials for growth. Still there is an overplus, and the organism will grow. Let it now acquire three times its original height and breadth and thickness, the mass is twenty-seven times as great, and the surface only nine times: that is to say, while there are twenty-seven cubic inches to be fed, there are just fifty-four square inches to feed them. There is no longer any overplus; the organism will stop growing. And it is a general rule that, in any case, when a thing grows its mass increases much faster than its surface. However much, therefore, the feeding power of the surface may be in excess to begin with, the mass must inevitably catch it up, and the growth will stop.

Now the changes of an individual mind may be reduced to the same three types:—

Growth.
Change of structure.
Change of function.

First then, what is the growth of the mind? It is the acquisition of new knowledge; not merely of that which is required to make up for our wonderful power of forgetting, for oblivion is really a far more marvellous thing than memory; but of a certain overplus which goes to increase the entire mass of our mental experiences. Now I do not know whether there is any race between surface and mass here as in the case of an organism; but it is certainly true that whereas in childhood the amount we forget is very little, and our powers of acquisition preponderate immensely over our powers of oblivion; as we grow up, the powers of oblivion gain rapidly upon the acquisitive ones, and finally catch them up; the growth ceases as soon as this balance is attained. So that in this first law, you see, there is an entire analogy between the two cases.

In the next place, the mind experiences changes of structure; that is to say, changes in the shape and arrangement of its parts. Ideas which were only feebly connected become aggregated into a close and compact whole. The ideas of several different qualities, for instance, which we never thought of as connected with each other, are brought together by the qualities being found to exist in the same object. In this way we form conceptions of things, which gradually get so compact that we cannot even in thought separate them into their component parts. Portions of our knowledge which we held as distinct are connected together by scientific theories; images which were scattered all about are bound up into living bundles by the artist, and so we find them re-arranged.

Lastly, changes of function take place. Everybody knows how the mental faculties open out and become visible as a child grows up. Men acquire faculties by practice. And without any conscious seeking, you must know how often we wake up as it were and find ourselves gifted with new powers. We have found evidence then of the existence of our three types of change,—growth, structure, and function.

The actions therefore which go on between the environment and the individual may be reduced to the same three types in the case of the mind as in the case of any visible organism. Being somewhat encouraged by this result, let us go back to our original question, What is that attitude of mind which is likely to change for the better? What is the meaning of *better?*

Although it is quite impossible to arrange all existing organisms in a serial chain, yet we certainly have a general notion of higher and lower. A bird we regard as higher than a fish, and a dog is higher than a snake. And if we return to our illustration of the tree, we shall see that at every point, at any given time, there is a definite direction of developement. So that though we might not be able to say which of two co-existing organisms was the higher, yet by comparing a species with itself at a time shortly after, we might say whether it had degenerated or improved. Now by examining various cases, we shall find that there are six marks of improvement:—

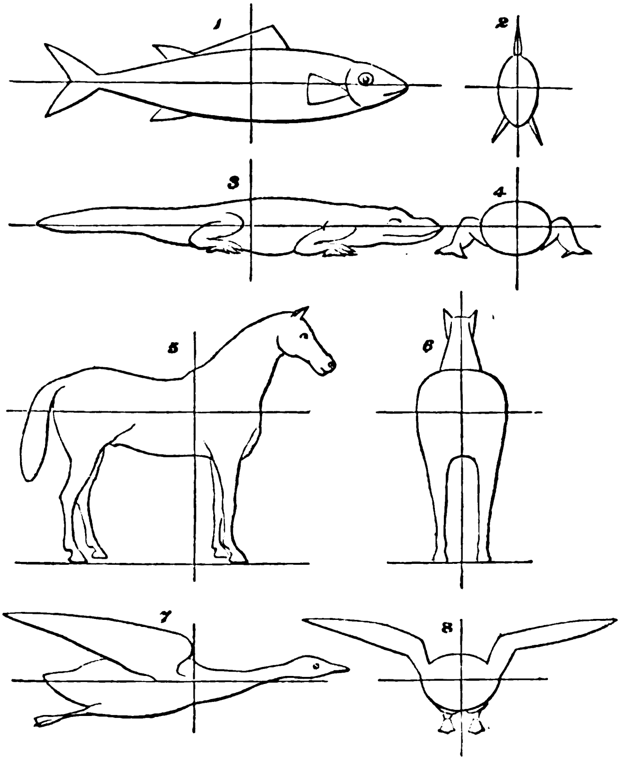
The parts of the organism get more different.
" " " " connected.
The organism gets more different from the environment.
" " connected with the environment.
The organism gets more different from other individuals.
" " connected with other individuals.

The processes in fact which result in developement are made up of *differentiation* and *integration:* differentiation means the making things to be different, integration means the binding them together into a whole; these are applied to the parts of the organism, the organism and surrounding nature, the organism and other organisms. Differentiation of parts is illustrated by the figure on the opposite page. [Spencer's 'Principles of Biology,' vol. ii., p. 187.]

Integration of parts means the connected play of them; so that one being touched the rest are affected. Differentiation from the environment takes place in weight, composition, and temperature. A polype is little else than sea-water, which it inhabits; a fish is several degrees of temperature above it, and made of quite different materials; till at last a mammal is 70° or 80° above the surrounding matter, and made of still more different materials. Integration with the environment means close correspondence with it; actions of the environment are followed by corresponding actions of the animal. Differentiation from other organisms means individuality; integration with them sociality.

In a similar way we have a sort of general notion of higher and lower stages of mental developement. I will endeavour to show that this general notion resolves itself into a measure of the extent to which the same six processes have gone on, namely:—

Separation of parts,
Connection of parts,
Separation from the environment,
Closer correspondence with the environment,
Separation from other individuals.
Sociality.



The only conception we can form of a purely unconscious state is one in which all is exactly alike, or rather, in which there is no difference.

> There is not one thing with another
> But Evil saith to Good: My brother,
>   My brother, I am one with thee:
> They shall not strive nor cry for ever:
> No man shall choose between them: never
>   Shall this thing end and that thing be.

The first indication of consciousness is a perception of difference. The child's eyes follow the light. Immediately this colourless, homogeneous universe splits up into two parts, the light part and the dark part. A line is drawn across it, it is made heterogeneous, and the first thing that exists is a distinction. Then other lines are drawn; appearance is separated into white, black, blue, red, and so on. This is the first process, the differentiation of the parts of consciousness. But by-

and-by a number of these lines of distinction are found to enclose a definite space; they assume relations to one another; the lines white, round, light, capable of being thrown at people, include the conception of a ball; this gains coherence, becomes one, a thing, holding itself together, not only separated from the rest of unconsciousness, but connected in itself into a distinct whole, integrated. Here we have the second process. And throughout our lives the same two processes go hand in hand; whatever we perceive is a line of demarcation between two different things; we can be conscious of nothing but a separation, a change in passing from one thing to another. And these different lines of demarcation are constantly connecting themselves together, marking out portions of our consciousness as complete wholes, and making them cohere. Just as a sculptor clears away from a block of marble now this piece and now that, making every time a separation between what is to be kept and what is to be chipped off, till at last all these chippings manifest the connection that ran through them, and the finished statue stands out as a complete whole, a positive thing made up of contradictory negations: so is a conception formed in the mind.

And this conception, when it is thus made into a whole, integrated, by an act of the mind, what does it immediately appear to be? Why, something outside of ourselves, a real thing, different from us. This is the third process, the process of *differentiation* from the environment. This is beautifully described by Cuvier, who pictures the first man wandering about in ecstacies at the discovery of so many new parts of himself; till gradually he learns that they are not himself, but things outside. This notion, then, of a thing being real, existing external to ourselves, is due to the active power of the mind which regards it as one, which binds together all its boundaries. And this goes on as long as we live. Constantly we frame to ourselves more complicated combinations of ideas, and by giving them unity make them real. And, at the same time, the converse process is equally active. While more and more of our ideas are put outside of us and made real, our minds are continually growing more and more into accordance with the nature of external things; our ideas become truer, more conformable to the facts; and at the same time they answer more surely and completely to changes in the environment; a new experience is more rapidly and more completely connected with the sum of previous experiences. But there is more than this. The action of these two laws taken together does in fact amount to the creation of new senses. Men of science, for example, have to deal with extremely abstract and general conceptions. By constant use and familiarity, these, and the relations between them, become just as real and external as the ordinary objects of experience; and the perception of new relations among them is so rapid, the correspondence of the mind to external circumstances so great, that a real scientific sense is developed, by which things are perceived as immediately and truly as I see you now. Poets and painters and musicians also are so accustomed to put outside of them the idea of beauty, that it becomes a real external existence, a thing which they see with spiritual eyes, and then describe to you, but by no means create, any more than we seem to create these ideas of table and forms and light, which we put together long ago. There is no scientific discoverer, no poet, no painter, no musician, who will not tell you that he found ready-made his discovery or poem or picture—that it came to him from outside, and that he did not consciously create it from within. And there is reason to think that these senses or insights are things which actually increase among mankind. It is certain at least, that the scientific sense is immensely more developed now than it was three hundred years ago; and though it may be impossible to find any absolute standard of Art, yet it is acknowledged that a number of minds which are subject to artistic training will tend to arrange themselves under certain great groups, and that the members of each group will give an independent yet consentient testimony about artistic questions. And this arrangement into schools, and the definiteness of the conclusions reached in each, are on the increase. So that here, it would seem, are actually two new senses, the scientific and the artistic, which the mind is now in the process of forming for itself. There are two remaining marks of developement: differentiation from surrounding minds, which is the growth of individuality; and closer correspondence with them, wider sympathies, more perfect understanding of others. These, you will instantly admit, are precisely the twin characteristics of a man of genius. He is clearly distinct from the people that surround him, that is how you recognize him; but then this very distinction must be such as to bind him still closer to them, extend and intensify his sympathies, make him want their wants, rejoice over their joys, be cast down by their sorrows. Just as the throat is a complicated thing, quite different from the rest of the body, but yet is always ready to cry when any other part is hurt.

We have thus got a tolerably definite notion of what mental developement means. It is a process of simultaneous differentiation and integration which goes on in the parts of consciousness, between the mind and external things, between the mind and other minds. And the question I want answered is, what attitude of mind tends to further these processes?

I have now done all that it was my business to do, namely, I have stated the question in a form in which it is possible to answer it. There is no doubt that by a careful study of the operations of nature we shall be able to find out what actions of an organism are favourable to its higher developement. Having formulated these into a law, we shall be able to interpret this law with reference to the mind.

But now I am going to venture on a partial answer to this question. What I am going to say is mere speculation, and requires to be verified by facts.

The changes which take place in an organism are of two kinds. Some are produced by the direct action of things outside, and these are to a great extent similar to the changes which we observe in inanimate things. When a tree is bent over by the wind, and gets ultimately fixed in this position, the change is in no way different from that which takes place when we bend a wire and it does not entirely return to its former straightness. Other changes are produced by the spontaneous action of that store of force which by the process of growth is necessarily accumulated within the organism. Such are all those apparently disconnected motions which make up the great distinction between living things and dead. Now my speculation is, that advantageous permanent changes are always produced by the spontaneous action of the organism, and not by the direct action of the environment. This, I think, is most clear when we take an extreme case. Let us suppose a race of animals that never had any changes produced by their spontaneous activity. The race must at a certain time have a definite amount of plasticity, that is, a definite power of adapting itself to altered circumstances by changing in accordance with them. Every permanent effect of the environment upon them is a crystallization of some part which before was plastic; for the part must have been plastic for the effect to be produced at all; and as the effect is permanent, the part has to that extent lost in plasticity. As this goes on, the race of animals will bind up in itself more and more of its history, but will in that process lose the capability of change which it once had; at last it will be quite fixed, crystallized, incapable of change. Then it must inevitably die out in time; for the environment must change sooner or later, and then the race, incapable of changing in accordance with it, must be killed off. On the other hand, any addition to the organism which is made by its spontaneous activity is an addition of something which has not yet been acted upon by the environment, which is therefore plastic, capable of indefinite modification, in fact, an increase of power. The bending of a tree by the wind is a positive disadvantage to it if the wind should ever happen to blow from the other side. But when a plant, for no apparent reason, grows long hairs to its seed—the material for which may have been accidentally supplied by the environment, while its use in this way is a spontaneous action of the plant—this is a definite increase of power; for the new organ may be modified in any conceivable way to suit the exigencies of the environment, may cling to the sides of beasts, and so help the distribution of the seed, or effect the same object by being caught by the wind. Activity, in fact, is the first condition of developement. A very good example of this occurs in Professor Huxley's lizards, of which you heard two or three weeks ago. About the time marked by the primary strata it appears that there was a race of lizards, thirty feet high, that walked on their hind legs, balancing themselves by their long tails, and having three toes like birds. This race diverged in three directions. Some of them yielded to the immediate promptings of the environment, found it convenient to go on all fours and eat fish; they became crocodiles. Others took to exercising their fore-legs violently, developed three long fingers, and became birds. The rest were for a long while undecided whether they would use their arms or their legs most; at length they diverged, and some became pterodactyles and others kangaroos. For Mr. Seeley, of Cambridge, has discovered marsupial bones in pterodactyles, that is to say, bones like those which were supposed peculiar to the order of mammals to which the kangaroo belongs.

Assuming now that this law is true, and that the developement of an organism proceeds from its activities rather than its passivities, let us apply it to the mind. What, in fact, are the conditions which must be satisfied by a mind in process of upward developement, so far as this law gives them?

They are two; one positive, the other negative. The positive condition is that the mind should act rather than assimilate, that its attitude should be one of creation rather than of acquisition. If scientific, it must not rest in the contemplation of existing theories, or the learning of facts by rote; it must act, create, make fresh powers, discover new facts and laws. And if the analogy is true, it must create things not immediately useful. I am here putting in a word for those abstruse mathematical researches which are so often abused for having no obvious physical application. The fact is that the most useful parts of science have been investigated for the sake of truth, and not for their usefulness. A new branch of mathematics, which has sprung up in the last twenty years, was denounced by the Astronomer Royal before the university of Cambridge as doomed to be forgotten, on account of its uselessness. Now it turns out that the reason why we cannot go further in our investigations of molecular action is that we do not know enough of this branch of mathematics. If the mind is artistic, it must not sit down in hopeless awe before the monuments of the great masters, as if heights so lofty could have no heaven beyond them. Still less must it tremble before the conventionalism of one age, when its mission may be to form the whole life of the age succeeding. No amount of erudition or technical skill or critical power can absolve the mind from the necessity of creating, if it would grow. And the power of creation is not a matter of static ability, so that one man absolutely can do these things and another man absolutely cannot; it is a matter of habits and desires. The results of things follow not from their state but from their tendency. The first condition then of mental developement is that the attitude of the mind should be creative rather than acquisitive: or, as it has been well said, that intellectual food should go to form mental muscle and not mental fat.

The negative condition is plasticity: the avoidance of all such crystallization as is immediately suggested by the environment. A mind that would grow must let no ideas become permanent except such as lead to action. Towards all others it must maintain an attitude of absolute receptivity; admitting all, being modified by all, but permanently biassed by none. To become crystallized, fixed in opinion and mode of thought, is to lose the great characteristic of life, by which it is distinguished from inanimate nature: the power of adapting itself to circumstances.

This is true even more of the race. There are nations in the East so enslaved by custom that they seem to have lost all power of change except the capability of being destroyed. Propriety, in fact, is the crystallization of a race. And if we consider that a race, in proportion as it is plastic and capable of change, may be regarded as young and vigorous, while a race which is fixed, persistent in form, unable to change, is as surely effete, worn out, in peril of extinction; we shall see, I think, the immense importance to a nation of checking the growth of conventionalities. It is quite possible for conventional rules of action and conventional habits of thought to get such power that progress is impossible, and the nation only fit to be improved away. In the face of such a danger *it is not right to be proper.*

[W. K. C.]

WEEKLY EVENING MEETING,

Friday, March 13, 1868.

Sir Henry Holland, Bart. M.D. D.C.L. F.R.S. President, in the Chair.

W. Stanley Jevons, M.A.

PROFESSOR AND COBDEN LECTURER ON POLITICAL ECONOMY IN OWENS COLLEGE, MANCHESTER.

On the Probable Exhaustion of our Coal Mines.

I. The coal raised from the coal mines of the United Kingdom in the year 1866 amounted to more than *one hundred million tons* (more exactly 101,630,544 tons), according to the excellent returns published by Mr. Robert Hunt, of the Mining Record Office. Reflecting upon the full significance of this fact it may be asserted :—

1. That the coal trade of this kingdom is the greatest trade, in regard to the bulk and weight of the commodity, ever carried on.

2. That every pound of that vast quantity of coal may be regarded as a pound of the most intrinsically useful and valuable substance ever discovered.

3. That the power and usefulness of coal is felt in every branch of industry, and in almost every operation which we carry on.

4. That Britain possesses the aid of this most invaluable substance in an altogether peculiar degree; and—

5. That we cannot hope to stand very long in this most happy position.

II. So vast a quantity as 100,000,000 tons cannot be represented to the eye or mind. Its bulk is 30 times as great as that of the greatest single work of human hands, the Pyramid of Cheops. Greater quantities of commodities are brought into British ports at present, than are recorded in the history of any nation, and yet it would take more than seven times as many vessels as those which enter our ports in a year to carry the quantity of coal we use.

More than half of the whole carrying power of the railways of the United Kingdom, devoted to goods traffic, is occupied in the conveyance of coal. So far as we can judge from returns, which do not always distinguish the kinds of goods carried, the goods traffic of the railways of the United Kingdom in 1865 was as follows:—

	Tons.
General Merchandise*	36,800,000
Minerals	18,300,000
Total	55,100,000
Coal and Coke	59,500,000
Total	114,600,000

III. This vast trade in coal can only be accounted for by considering the wonderful qualities with which coal is endowed. It is the MAINSPRING OF OUR MATERIAL INDUSTRY. It may be called the real Philosopher's Stone, which supplies us cheaply and plentifully with everything that can conduce to the service of man. This extreme usefulness of coal is due—

1. To the enormous power which is latent in it, and is brought forth when we burn it;
2. To the fact, now so clearly revealed by science, that *force is the key to all the changes of matter.*

By aid of the mechanical equivalent of heat, we can ascertain that good coal contains latent force sufficient to raise its own weight 11,422,000 feet, or about 2100 miles against the force of gravity. The coal raised in 1866 may further be calculated to contain force equal to that which would be exerted by 530,000,000 horses, or 2,650,000,000 men, working eight hours a day for 300 working days in the year.

IV. This vast power is turned to use in an indefinite multitude of ways, which may thus be rudely classified.

CLASSIFICATION OF THE USES OF COAL.

(I.) As Source of Heat.

1. *For Household Use.*—Warming and ventilating houses, churches, public buildings, &c.
2. *For the Alteration of Cohesive Condition of Substances.*
   Melting and casting of metals; softening and forging of metals—the blacksmith's fire.

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* Not including live stock, of which the weight is not ascertained.

Manufacture of glass, bricks, earthenware, &c.
Boiling salt, soap, &c.; brewing; distilling; drying substances.
Chemical manufactures.

3. *For the Production of Power by the Steam, Gas, or Hot-air Engine.* Pumping water; draining mines; supply of water; removal of sewage.

Steam navigation.
Railways, and road locomotives.
Hammering, rolling, and working metals.
Mill and factory labour.
Hydraulic and pneumatic machines.
Small machines moved by gas engine.
Machine agriculture; steam ploughing, &c.
Manufacture of ice.

(II.) As REDUCING AGENT; SOURCE OF HEAT, WITH CHEMICAL AFFINITY.

Smelting of the metals—iron, copper, lead, zinc, &c.
Chemical manufactures.

(III.) AS INDIRECT SOURCE OF ELECTRICITY BY MAGNETO-ELECTRIC MACHINES.

Electro-telegraphy.
Electro-metallurgy.

(IV.) AS SOURCE OF LIGHT.

Gas manufacture; petroleum; paraffin candles.
Electric light-house illumination.
Photography by artificial light.

(V.) AS SOURCE OF MATERIAL.

Tar, pitch, naphtha, lubricating oils.
Ammoniacal manures; carbolic acid; aniline dyes; ethereal odours and flavours, &c.

It is only by thus collecting together the multitudinous uses of coal that we can gain an adequate idea of its importance to us and the certainty that its use will extend.

V. Comparing, now, the present yield of coal (100,000,000 tons annually) with the quantity which Mr. Hull believes to lie in these islands, within 4000 feet of the surface and in workable condition (83,544,000,000 tons), we find that we might continue to consume coal at our present annual rate for 835 years at least; but when we remember that our consumption has increased by 36 millions in the last twelve years (from about 65 millions in 1854 to 101,000,000 in 1866), and that the causes of increase still continue in existence, we cannot attribute any importance to the above calculation. There is no appearance that steam navigation or railways have at all approached their full developement in this country; while in the steam-plough, in schemes of steam-drainage or water-supply, the employment of steam-produced hydraulic pressure, in the use of small gas-engines in workshops, and in a multitude of other ways, we have some indication of the increased future demand for coal.

VI. Economy, it may be pointed out, does not tend to reduce the industrial consumption of coal, but acts in the opposite direction: by increasing the profitableness of coal-labour, it extends its use. Almost every improvement in the engine for the last century and a half has been directed to economizing the consumption of coal; and yet the use of the engine and the quantities of coal consumed advanced *pari passu* with its economical performance.

It is altogether irrational to argue that progressive economy, which has coexisted with and been the partial cause of advancing consumption in the past, will have the opposite effect in the future.

VII. As regards the law of increase of coal consumption, both experience and theory lead us to believe that the increase takes place in a geometrical series, by multiplication rather than by mere addition. The following numbers will illustrate the difference in question:—

Arithmetical Series, increasing by addition ..	1	2	3	4	5	6	7	8
Geometrical Series, increasing by multiplication	1	2	4	8	16	32	64	128

The following table will show that when we can get accurate statistics of the consumption of coal we find the increments indefinitely increasing, in the manner rather of a geometrical than of an arithmetical series.

Year.	Total quantity of coal imported into London.	Increase in fifty years.
	Tons.	Tons.
1650	216,000	—
1700	428,100	212,100
1750	688.700	260,600
1800	1,099,000	410,300
1850	3,638,883	2,539,883
1863	5,119.887	5,696,170[*]

The above and other statistics quoted in the 'Coal Question,'[†] Chapters IX. X. and XI., show that our industry grows by multiplication, and by multiplication at a rising rather than at a falling rate. The temporary depressions of trade which occur at intervals may sometimes seem to check the rapidity of this increase; but we have only to wait a year or two to see our industry advancing again with growing strides.

No statements of the total amount of coal produced in this kingdom are the least to be relied on, except those collected by Mr. Robert Hunt, the Keeper of Mining Records, and the following is a statement of the general progress of the coal trade of the United Kingdom as ascertained by him:*—

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[*] Increase as for fifty years, if continued at same rate as during the thirteen years experienced.

[†] 'The Coal Question: an Inquiry concerning the Progress of the Nation, and the Probable Exhaustion of our Coal Mines.' By W. S. Jevons, M.A. 2nd ed. revised. London, 1866. (Macmillan.)

Year.	Coal raised. Tons.	Coal exported. Tons.	Year.	Coal raised. Tons.	Coal exported. Tons.
1854	64,661,000	4,309,000	1861	85,635,000	7,222,000
1855	61,453,000	4,976,000	1862	83,638,000	7,694,000
1856	66,645,000	5,879,000	1863	88,292,000	7,529,000
1857	65,394,000	6,737,000	1864	92,787,000	8,063,000
1858	65,008,000	6,529,000	1865	98,150,000	8,585,000
1859	71,979,000	7,081,000	1866	101,630,000	9,367,000
1860	80,042,000	7,412,000			

It is impossible to view, without some degree of alarm, so rapid an increase of the coal trade as the preceding figures indicate. Without doubt our production will advance to 200 millions before very many years are past; and the alarming calculation may be made that if we went on increasing our production of coal for 110 years as rapidly as we have done during the last 12 years, our coal seams would be worked out to a depth of 4000 feet. But such a supposition is put forward, not as a serious possibility, but as a *reductio ad absurdum.* The conclusion to be drawn from it is simply that the nation cannot possibly progress in material wealth for 110 years more as rapidly as it has done in the present century. The limited extent of our coal-fields would not allow us to go on increasing the draught of coal as lavishly as we have done. But it is the very necessity of changing from a highly progressive to a less progressive or stationary condition, that is most grievous. Population and production, when once set in motion, move with a certain impetus, and it is the check to such motion which is distressing and threatening.

VIII. The subject wears a more serious aspect still when we consider the coal resources and production of other countries as well as our own.

According to the latest returns which are at hand, it would seem that the total known produce of coal in the world is thus distributed over the chief nations:—

	Tons.
Great Britain	101,630,000
United States	25,800,000
Prussia and the Zollverein	20,610,000
France	10,710,000
Belgium	9,935,000
Austria	4,500,000
British North America	1,500,000
Russia	1,500,000
Spain	300,000
New South Wales	250,000
Ireland	123,500
Total	176,858,500

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* I am kindly informed by Mr. Hunt that when the returns of the consumption of coal in 1867 are completed, the total will probably amount to 104,000,000 tons, showing continued increase in spite of the depression of trade.

It would appear then that of the total *known* produce of coal in the world we raise considerably more than half (57 per cent.), although we form probably not more than one in forty of the population of the world. If to our own coal produce we add that of the United States and our colonies, we may conclude that the Teutonic race enjoys 73 per cent., or almost 3 parts out of 4, of the coal raised. It is hardly possible to over-estimate the forces acting in our favour which are represented by this partial monopoly of the most powerful material agent of civilization.

The total quantity of coal existing can hardly be said to be known in the case of any one country; but some notion of the comparative coal resources of different countries may be gained from the following statement of the area of the coal-measures in the chief coal-producing countries, as estimated by Professor Rogers:—

	Area of Coal Lands in square miles.
United States	196,650
British North American Possessions	7,530
Great Britain	5,400
France	984
Prussia	960
Belgium	510
Bohemia	400
Westphalia	380
Spain	200
Russia	100
Saxony	30

Though Great Britain is far more abundantly provided with coal than any continental nation, our resources sink into insignificance beside those of North America, and no very long period will elapse before this comparative poverty in coal will make itself felt.

IX. It is continually suggested, indeed, that before coal is at all likely to be exhausted, some substitute will be found for it, and appeal is made to some old proverb, like *Necessity is the mother of invention.* But it requires very little philosophy to see that the proverb is very partially true. We live in a chronic state of necessity and difficulty, and the great discoveries which we enjoy are but so many exceptional instances in which we have been unexpectedly relieved from labour and evil. We have no real ground for supposing that when one exceptional advantage is withdrawn from us, another will immediately be extended to us.

The favourite notion that electricity will be the future source of power is entirely fallacious; for the coal-driven engine moving the magneto-electric machine is now the cheapest source of electricity, and by gradual improvements, such as that in Mr. Wilde's machine, coal will become a still cheaper source of electricity. Even the elements of the electric battery have always been practically furnished by the reducing power of coal. If coal then become, as there is every reason to suppose it will, a cheaper and cheaper source of electricity, it is obviously absurd to suppose that electricity should supersede the power of coal.

It is conceivable, indeed, that in the course of ages some wholly new source of power might be discovered; but there is no reason to suppose that this island, which forms but the one four-hundredth part of the total land-area of the globe, would be as richly endowed with the new source of power as it is with coal. If the sun's beams are in the future to be the direct source of power, it is the plains of Africa or of Australia that will be the seats of industry and not this cloud-obscured Isle.

X. The conclusions we must come to on this subject are then as follows:—

1. The power of coal is extending itself and making itself more widely and deeply felt every day. It is more and more taking the place of wind, horse, or manual power, and is becoming the universal assistant.

2. We are naturally led every day to extend our consumption of so invaluable a substance, and experience shows that the more we use the more extensive are our augmentations.

3. Our consumption is already commensurable with our total supply; that is to say, we can form some notion how long our supply will endure with a stationary consumption.

4. As this consumption increases by multiplication, our national life becomes shortened, and it is apparent that the increase cannot go on very long at the present rate.

5. The moment we are forced to draw in, other nations, possessing far more extensive fields of coal compared with their annual consumption, will be enabled to approach and ultimately to pass us.

6. The exhaustion of our mines, as it will probably manifest itself within the next hundred years, will consist not in any stoppage of supplies, but an increase of cost, and the impossibility of increasing the consumption each year as at present.

XI. At some future time then, when coal will be even a more useful agent than at present, we shall stand in a position of comparative inferiority. For such a time we can best prepare ourselves, not by short-sighted restrictions on the consumption or evaporation of coal, but by freeing the nation from its burdens of debt and ignorance and pauperism. We have many great tasks to perform, which can only be undertaken with a fair hope of success when the nation is in a state of high prosperity and progress. It will be too late to think of such great undertakings when our progress is checked, and the pressure of population and the want of employment are grievously felt. It is in a period of free expansion like the present that we can alone take any effectual measures for raising appreciably the standard of education, comfort, and morality of the people; and if we do not use the abundant wealth which our coal resources now afford us to fulfil such duties, we undoubtedly misuse it.

[W. S. J.]

WEEKLY EVENING MEETING,

Friday, March 20, 1868.

His Royal Highness the Prince of Wales, K.G. in the Chair.

Professor Augustus Matthiessen, F.R.S.

On Alloys and their Uses.

The object of this discourse was to show experimentally why alloys are used in preference to their component metals.

Alloys may be, chemically considered, divided into three classes:

1. Chemical combinations.
2. Mechanical mixtures.
3. Solutions of the one metal in the other which has become solid, or, for shortness sake, solidified solutions of the one metal in the other.

Under the term chemical combination such alloys may be considered which are the result of the combination of two metals when these unite together with great energy and evolution of heat, producing an alloy the physical and chemical properties of which we cannot foresee. As an example of such alloys those of gold, with tin, lead, or zinc may be quoted; for if to melted tin, lead, or zinc, gold be added, the two metals unite together with great energy and produce an alloy which is exceedingly brittle and totally unfit for practical purposes.

It is for this reason that the more expensive metals, silver and copper, are used for alloying gold for the purposes of coinage, &c.

With regard to such alloys which may be looked upon as mechanical mixtures, like oil and water, or rather as ether and water, for no two metals are known which, like oil and water, do not dissolve at all in one another, but a few metals are known which, like ether and water, dissolve slightly in one another, for ether will dissolve a certain amount of water, and water a certain amount of ether. If ether and water be mixed together, say in equal parts, two layers will be formed, the top one being ether containing a little water, the lower one water containing a little ether. Two metals, for instance, which behave in exactly a similar manner to ether and water are lead and zinc, for lead when fused with zinc will dissolve 1·6 per cent. zinc, and zinc in its turn will take up 1·2 per cent. lead.

If these two metals be fused together, say in equal parts, they will separate into two layers, like ether and water, the top one, being the specifically lighter, zinc, with a small percentage of lead, the lower one lead, with a small percentage of zinc. If such an alloy be made and cast in a mould, the difference in the behaviour of the two ends may be easily shown; for the top one is so brittle that it cannot be bent without breaking, whereas the lower one may be bent with ease.

Such chemical combinations and mechanical mixtures are, however, comparatively rare; and for alloys in common use, practice has almost invariably chosen such alloys as may be considered as belonging to the third class, rejecting those of the first and second as worthless for practical purposes.

Under the term solidified solutions of the one metal in the other, such alloys may be considered, which, like the chlorides of potassium and sodium when fused together, produce a mass having some of the physical properties totally different from those of the component salts. It cannot be assumed that the chloride of sodium enters into chemical combination with the chloride of potassium. One important property of a solidified solution is, that the components are homogeneously diffused in one another, so that even under the most powerful microscope they can no longer be distinguished from one another.

Alloys are used because they possess certain physical properties to a far greater extent than their component metals. The physical properties may be divided into two classes.

1. Those which in all cases are imparted to the alloy, approximately in the ratio in which they are possessed by the component metals.

2. Those which in some cases are, and in others are not, imparted to the alloy in the ratio in which they are possessed by the component metals.

To the first belong Specific Gravity, Specific Heat, and Expansion due to heat. It is easy to show this experimentally; the specific gravity of an alloy may be shown to be equal to the mean of those of its component metals, by hanging on the one side of a balance the alloy and on the other side the metals composing it unalloyed, and then placing them both in water.

The specific heat of an alloy may be proved equal to that of its components by placing the alloy and its components in boiling water, and then in equal volumes of cold water; when the rise of temperature in the two cases will be found the same as may be shown by a differential-air thermometer.

A brass bar placed in any apparatus for showing expansion by heat is seen to expand exactly as much as a composite bar, of which one portion is of copper, the other of zinc. The length of the zinc portion being proportional to the amount of zinc in brass.

To the second class of physical properties belong, Conduction for Heat and Electricity, Hardness, Tenacity, &c.

As a basis for the conclusion which will be drawn, the electric conducting power for alloys may be taken. Researches into this subject have shown that when tin, lead, zinc, or cadmium are alloyed together, such alloys conduct electricity in the ratio of the relative volumes of the component metals, whilst in all other cases no such simple relation exists between the conducting power of the metals and their alloys. If, for instance, gold be alloyed with silver, say in equal volumes, the conducting power of an alloy will be 15, that of silver being 100, and that of gold 80.

If curves be drawn to represent the conducting power of different series of alloys, three typical forms will be observed: the first represented by nearly a straight line, the second by the letter *L*, and the third by the letter U.

Wiedemann and Franz have proved experimentally that the values obtained for the conducting power of metals and alloys, for heat and electricity, are identically the same; and the truth of this statement may be shown by the following experiment:—If bars of gold and silver and some gold-silver alloys be fixed so that one end of all of them is in a hot-water box and the other end in the bulb of a small air-thermometer, the depression in the columns of the liquid in the tubes of the air-thermometers will indicate the relative conducting powers (approximately) of the several bars; and if through the tops of the columns of liquid a line be drawn, such line will form a curve similar to that referred to as obtained for the electric conducting power.

That this is true is thus shown:—

By the side of this apparatus is placed another of this construction: Into the bulbs of several air-thermometers are fixed wires of the same size and length, and of the same materials as were used in the heat-conducting experiment. One end of each wire is soldered to one thick copper wire, and the other end to another similar wire. These two wires are connected to the poles of a battery. The current will then divide itself, and a portion will pass through every wire proportional to the conducting power of that wire. This current will heat the wire and cause the liquid in the tubes connected with the air-thermometers to descend, and the line drawn through the top of the columns will be nearly similar to the curve already mentioned, which is formed by the bulbs in which the heat-conducting bars are fixed.

The analogy between the relation existing in this case and in some others may be shown experimentally as follows:—

Sonority. When bars of alloys and their component metals are struck, a great difference will be found in the note produced; and in almost every case where the experiment has been made, the most sonorous alloy was found to correspond in composition approximately with that at the turning point of the electric conducting power curve.

Tenacity. When wires of the same diameter of metals and alloys are broken by traction, those of the alloys will require a much greater force than their component metals; and it may be deduced from what is known, that those alloys the composition of which corresponds to the turning point of the conducting power curve are more tenacious than any other alloy composed of the same metals.

Elasticity. When spirals of wires of metals and their alloys are weighted to an equal extent, the alloys will be found on removing the weights to possess the property of resuming their original form in a much higher degree than their component metals. Here again the alloys corresponding in composition to those of the turning point of the conducting power curves are the most elastic.

From what has been said, and from the experiments described, the conclusion may be drawn that the chemical composition of the practically-used two metal-alloys correspond to those situated at the turning points of the heat and electric conducting power curves, and that if a two-metal alloy of a special physical property be required, it would be as well to try that alloy the composition of which would correspond to the turning point of the curve representing the electric conducting power of the alloys of the two metals.

[A. M.]

WEEKLY EVENING MEETING,

Friday, March 27, 1868.

SIR HENRY HOLLAND, Bart., M.D. D.C.L. F.R.S. President,
in the Chair.

WILLIAM B. CARPENTER, M.D. V.P.R.S.

On the Unconscious Activity of the Brain.

MAN'S conscious life essentially consists in an action and re-action between his Mind and all that is outside it,—the *Me* and the *Not-Me*. But this action and re-action cannot take place, in his present stage of existence, without the intervention of a material instrument, whose function it is to bridge over the hiatus between the individual consciousness and the external world, and thus to bring them into mutual communication. So long, therefore, as either the mental or the bodily part of Man was studied to the exclusion of the other, no true progress could be made in Psychological Science; and thus it was that the bygone controversies between the Spiritualists and the Materialists, in which the disputants on either side looked at his composite nature from that side only, were barren of any other good result than that of bringing into view phenomena that might otherwise have escaped attention. But the Psychologist who looks at his subject in the light of that more advanced philosophy of the present day which regards Matter merely as the vehicle of Force, has no difficulty in seeing where both sets of disputants were right and both wrong; and, laying the foundations of his Science broad and deep in the *whole* constitution of the individual Man and his relations to the world external to him, aims to build it up with the materials furnished by experience of every kind, mental and bodily, normal and abnormal,—ignoring no fact, however strange, that can be attested by valid evidence, and accepting none, however authoritatively sanctioned, that will not stand the test of thorough scrutiny.

It is with the view of promoting the advance of such a Psychology, that the lecturer desires to bring into more distinct recognition a doctrine which has been familiar to the Metaphysicians of Germany from Leibnitz to the present time, under the names "Latent Thought," or the "Preconscious Activity of the Soul," and was systematically expounded in this country by Sir William Hamilton: whilst in Physiological language it may be designated as the "Unconscious Action of the Brain," or, more strictly, "Unconscious Cerebration."* To himself it seems of little consequence whether the doctrine be expressed in terms of Metaphysics or in terms of Physiology, provided it be recognized as having a positive scientific basis. But since, in the systems of Philosophy long prevalent in this country, *consciousness* has been almost uniformly taken as the basis of all strictly Mental activity, it seems convenient to designate as Functions of the Nervous System all those operations which lie below that level. And there is this advantage in approaching the subject from the Physiological side,—that the study of the Automatic actions of other parts of the Nervous System furnishes a clue, by the guidance of which we may be led to the scientific elucidation of many phenomena that would otherwise remain obscure and meaningless.

Referring to a discourse delivered by him March 12, 1852, "On the Influence of Suggestion in modifying and directing Muscular Movement independently of Volition," Dr. Carpenter reminded his audience that the doctrine of *Ideo-motor* action therein set forth had been referred to by Professor Faraday as furnishing an adequate scientific *rationale* of the phenomena of "Table-turning" and "Table-talking," which developed themselves epidemically soon afterwards. Whilst the ordinary phenomena of "Table-talking" present a most curious body of illustrations of that principle, cases have occasionally occurred in the experience of persons above suspicion of intentional deception, in which the answers given by the movements of the tables were not only unknown to the questioners, but were even *contrary to their belief at the time*, and yet afterwards proved to be true. Such cases afford typical examples of the doctrine of "Unconscious Cerebration;" for in several of them it was capable of being distinctly shown, that the answers, although contrary to the belief of the questioners at the time, were true to facts of which they had been formerly cognizant, but which had vanished from their recollection,—the *residua* of these forgotten impressions giving rise to Cerebral changes which prompted the responses, without any consciousness, on the part of the agents, of the latent springs of their actions.

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* Dr. Laycock, in an able essay on the 'Reflex Action of the Brain,' published in 1844, brought together a number of phenomena which justified his extension of the doctrine of Reflex action from the Spinal Cord to the Brain; but as he did not draw a distinction between the reflex action of the *Sensory Ganglia* (Sensori-motor) and that of the *Cerebrum* (Ideo-motor), and did not assert that either could take place *without consciousness*, he was not understood at the time to affirm this position, though it appears from his subsequent statements that he certainly meant to do so. The lecturer, having long previously taught the doctrine of the reflex action of the Sensory Ganglia, and having been convinced by Dr. Laycock's reasoning that it might be extended to the Cerebrum, was led by a consideration of the anatomical relations of the Cerebrum to the Sensory Ganglia to believe that a succession of changes might take place automatically in the former, of which the *results* only might rise to consciousness; and to this kind of activity he gave the designation of "Unconscious Cerebration."

In order, however, to present the doctrine in its proper scientific aspect by giving it a definite physiological basis, Dr. Carpenter recapitulated what he considered to be the fundamental doctrines relating to the *original* or *primary*, and the *acquired* or *secondary* Automatic actions of the principal divisions of the Cerebro-Spinal centres. These may be distinguished as,—

1. The *Spinal Cord* (including the *Medulla Oblongata*);
2. The *Sensory Ganglia ;*
3. The *Cerebellum ;*
4. The *Cerebrum.*

Leaving out of consideration the Cerebellum, of which the function has not yet been satisfactorily determined, and fixing our attention upon the other centres, we find that each of them, in addition to its *original* or *primary* Automatic actions, comes to be the instrument of a set of *secondary* Automatic actions, which, though originally prompted by the Will, and still remaining under its control, are habitually performed without any Volitional agency.

Thus the *primary* function of the *Spinal Cord* as an independent centre consists in the performance of the motions of Respiration and Swallowing, which are essential to the maintenance of life; and in many of the lower animals it is certain that the ordinary movements of Locomotion have the same *primary* Automatic character. In Man, however, the power of performing these movements is *acquired* by a process of education; yet when once the co-ordination has been established, the movements are performed Automatically,—continuing when set going by one act of the Will, until they are checked by another act. Of this we have daily experience in the continuance of the act of walking, whilst the attention is closely and continuously occupied upon an internal train of thought; each movement suggesting the succeeding one; and the repetition being thus indefinitely prolonged, until, the attention being recalled, the Automatic impulse is superseded by Volitional control.

The *primary* Automatic action of the *Sensory Ganglia*, again, seems to be chiefly connected with movements of *protection;* as in the sneezing produced by the application of irritants to the nasal surface, or the closure of the eyelids at a flash of light. But their *secondarily*-Automatic agency may be distinctly traced in the guidance of the habitual movements of locomotion, performed under the conditions previously stated. Thus a man in a state of profound abstraction walks through a crowded street, without jostling his fellow-passengers or bruising himself against lamp-posts; and he follows the line of direction which is most familiar to him, even though at starting he had intended to take some other.

The influence of *habitudes acquired by experience*, which take the place in Man of the intuitive capacities of the lower animals, is peculiarly well seen in that co-ordination of the Visual and Tactile perceptions, by which we acquire our notions of the forms and relations of external objects, and regulate our muscular movements in accordance with those notions. A Bird just come forth from the egg will peck at an insect with perfect aim; but an Infant is long in learning to grasp at a bright object held within its reach, being obviously unable in the first instance either to estimate its distance, or to combine the muscular actions needed for its prehension. And the observation of numerous cases in which sight has been first obtained after tactile familiarity with external objects had been fully acquired, enables it to be positively affirmed that no object can be *immediately* recognized by sight alone, when seen for the first time under such circumstances.*

This class of facts is of great importance in our present inquiry; because we have here a distinct instance of the *formation of judgments on the basis of an acquired experience, by a process of which, even when we give our attention to it, we are altogether unconscious.* Thus when we obtain a conception of solid form by the mental combination of two dissimilar pictures in the Stereoscope, that conception *seems* to be so necessary and immediate, that its formation might be supposed to be the result of an *original* intuition, if we had no means of tracing out the antecedent stages of the process, and of thus satisfying ourselves that it is *secondary* or *acquired.* The faculty to which it is due may be said to be the *resultant* of our whole previous training in this direction; which not merely enables us to recognize the forms and relations of objects of which we have some antecedent knowledge, so that they are in some degree suggested by the single picture, but also to create (so to speak) forms and relations of which the single picture gives us no adequate idea. The Physiologist can scarcely doubt, that as the Nervous system, like every other part of the organism, *grows to* the mode in which it is habitually exercised, a *direct* channel of instrumental action here comes to take the place of the *circuit* through which the process was originally performed; so that the *acquired intuition* of Man, in regard to the forms and relations of external objects, comes to be as certain and direct as the *original intuition* of the lower animals, whilst probably far exceeding it in completeness and range.

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* Thus, in a case published about three years ago by Mr. Critchett, of a young woman who first obtained sight at the age of nineteen, it is recorded that when a pair of scissors was first held before her, although she correctly described their shape and metallic lustre, she had not the least idea of their identity with the implement she had been accustomed to handle; but when told what it was, laughed at what she called her own stupidity.

The relation of the *Cerebrum*, or Brain proper, to the Spinal Cord and Sensory Ganglia, can only be properly studied by the light of Comparative Anatomy; and from this we learn that instead of being (as was formerly supposed) the centre of the whole system, in direct connection with the organs of sense and with the muscular apparatus, it is a superadded organ, the development of which seems to bear a pretty constant relation to the degree in which Intelligence supersedes Instinct as a spring of action. The ganglionic matter which is spread out upon the surface of the Hemispheres, and in which their potentiality resides, is connected with the Sensory Tract at their base (which is the real centre of convergence for the sensory nerves of the whole body) by Commissural fibres, long since termed by Reil, with sagacious foresight, "nerves of the internal senses;" and its anatomical relation to the Sensorium is thus precisely the same as that of the Retina, which is a ganglionic expansion connected with the Sensorium by the Optic nerve. Hence it may fairly be surmised (1) that, as we only become conscious of visual impressions on the Retina when their influence has been transmitted to the central Sensorium, so we only become conscious of ideational changes in the Cerebral Hemispheres when their influence has been transmitted to the same centre; and (2) that, as visual changes may take place in the Retina of which we are unconscious, either through a temporary inactivity of the Sensorium (as in sleep), or through the entire occupation of the attention in some other direction, so may ideational changes take place in the Cerebrum, of which we may be at the time unconscious for want of receptivity on the part of the Sensorium, but of which the results may at a subsequent time present themselves to the consciousness as Ideas elaborated by an automatic process of which we have no cognizance.

That the Cerebrum, like the nervous centres on which it is superimposed, has an Automatic activity of its own, cannot be doubted by those who have attended to the phenomena of Somnambulism (whether natural or induced), in which the directing and controlling power of the Will seems completely suspended, and the trains of thought follow the lead either of some dominant idea or of suggestion from without. There are well-authenticated cases in which such automatic action has not only evolved results that were perfect in themselves, but has wrought these out through a shorter and more direct process than had been conceived possible in the waking state; the withdrawal of all distracting influences appearing to favour that undisturbed action of the mental mechanism (if such a phrase be permissible), which is the condition most favourable to the success of the operation. But in all such instances the Automatic action follows the course of the habitual lines of thought, and expresses the result of the whole previous training and discipline of the mind, which has been carried on under Volitional direction. The Lawyer could not thus have written in his sleep a lucid opinion, unravelling the perplexities of a complicated case, if he had not assiduously cultivated the intellectual habit by which it was elaborated; nor could the Mathematician, in the same state, have not merely executed with perfect correctness a lengthened computation, which had baffled him in the waking state, but found out a much more direct means of attaining the result, if his previous training had not been of a kind to develop this self-acting power.

With such evidence that the Cerebrum may work *automatically*, it may further be regarded as Physiologically probable (on the grounds already stated) that such automatic action may take place *unconsciously;* and facts which are within the experience of every one seem to justify this conclusion. Thus, when we have been trying to recollect some name, phrase, or occurrence, and, after vainly employing all the expedients we can think of for bringing the desiderated idea to our minds, have abandoned the attempt as useless, it will often occur spontaneously a little while afterwards, suddenly flashing (as it were) before our consciousness; and this although the mind has been completely engrossed at the time by some entirely different train of thought, so that no link of association can be detected whereby the result has been knowingly apprehended. Now in these cases it seems probable that the train of action we have purposely set going in the first instance has continued in movement when we have withdrawn our attention from it, and goes on all the more regularly in consequence of that withdrawal; for experience shows that we are much more likely to recover the forgotten idea when we cease to trouble ourselves about it, than when we go on searching for it,—just as a rider who has lost himself in some unknown region is more likely to find his way home by dropping the reins on his horse's neck, and letting him take his own course, than by wearying him in trying one road after another.

The same mode of action seems to have a large share in the process of *Invention*, whether artistic, poetical, or mechanical: for numerous instances might be cited, in which, the object to be attained having been kept before the mind for some time without any immediate result, that result has suddenly presented itself either on first awaking out of sleep, or in the midst of some entirely different occupation. And it is a common experience of inventors (whether Artists, Poets, or Mechanicians) that when they have been brought to a stand by some difficulty, the tangle will be more likely to unravel itself (so to speak) if the attention be completely withdrawn from it, than by any amount of continued effort.

The same appears to be true of those acts of *Judgment* in which a great many opposing considerations are involved, and in which we take time to form our conclusion. As was well said by Abraham Tucker[*] of this class of cases, "with all our care to digest our materials, we cannot do it completely; but after a night's rest, or some recreation, or the mind being turned into some different course of thinking, *she finds they have ranged themselves anew during her absence,* and in such manner as exhibits almost at one view all their mutual relations, dependences, and consequences—which shows that our organs do not stand idle the moment we cease to employ them, but continue the motions we put into them after they have gone out of sight, thereby working themselves to a glibness and smoothness, and falling into *a more regular and orderly posture than we could have placed them with all our skill and industry.*" Experience shows that the soundest judgments of the well-disciplined mind are thus formed; all the considerations which ought to be taken into account being first duly brought before it, and then left free to arrange themselves by fixing the attention on some other occupation: and if time be given for this unconscious balancing, we find, when we return to the subject, that the direction in which our minds gravitate is a surer guide than any estimate we might have formed under volitional pressure.

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[*] 'Light of Nature Pursued,' 2nd edition (1805), chap. x. § 4, vol. i. p. 248.

This Unconscious action of the Brain, however, is often exerted in giving a bias to our judgments, of which we may be entirely unaware. Almost every one is thus influenced more or less by the habits of thought and feeling early impressed upon him; and the judgment is especially liable to be warped by these, when the ordinary vigour of the mind is depressed by physical or moral causes. This kind of perversion may be so decided in its evil effects, as to lead to a suspicion of a want of honesty or candour, which may be totally unfounded; the real source of it lying-hid deep down in that stratum of the mental constitution, which represents the results of those early influences for which the individual himself is not responsible. Thus, as Mr. Lecky has shown, the doctrine of Unconscious Cerebration inculcates *toleration* for differences not merely of belief, but of the moral standard.

One of the most frequently-recurring forms of Unconscious Cerebral action, is that by which what we call "Common Sense" decides for us in a great variety of cases, in which we do not think it worth-while to submit the question to a logical discussion. Now this "common sense" is, so to speak, an *acquired intuition;* being the *resultant* of the whole previous activity of the Mind, conjointly with that of the Brain which is its instrument. Its value will consequently depend upon the nature of the training and discipline which the Intellectual powers have received: and it may be affirmed without hesitation, that where those powers have been originally good, and have been thoroughly well cultivated and exercised, the "common-sense" judgment is likely to be even superior to that which may be worked out by an elaborate process of reasoning, wherein some more acute reasoner will almost always be able to find some flaw. Thus the "common-sense" decision of mankind in regard to the existence of an External World, is practically worth more than all the arguments of all the Logicians who have discussed the basis of our belief in it.

If, then, it be true that every form of Intuition, whether *original* or *acquired*, is referable to the ever-flowing under-current, which may be designated as "Unconscious Cerebration" or "Preconscious Activity of the Soul," according as we use the terms of Physiology or of Metaphysics, the question naturally arises what power we have of directing and controlling its course, of strengthening or repressing its power.

We have not that direct mastery over it, which we can gain by a determined exercise of the Will over our conscious activity. We cannot *acquire*, if we have it not in our original constitution, the creative power of genius, so as to *make ourselves* great Poets, Artists, or Musicians; nor can we gain by practice that peculiar *insight* which characterizes the Scientific discoverer of the highest class, or that ingenuity which distinguishes the great Mechanical inventor; for these gifts are of the nature of *instincts*, which may be developed and strengthened by appropriate cultivation, but which no culture will of itself produce, any more than it can raise a crop of corn where there has been no seed.

Still where we cannot create, we may learn to admire the Beautiful, to recognize the True, and to value the Good; and this power of appreciation grows and intensifies, in proportion as it is exercised aright. The more we fix our attention on the highest ideals of Art, and withdraw ourselves from the influence of those lower forms of it which in any way connect themselves with the grosser parts of our nature, the more thorough will be our intuitive appreciation of what is noble and elevating, the more thorough our intuitive distaste for all that is mean and degrading. And so in the pursuit of Truth, the more faithfully, strictly, and perseveringly we aim to disentangle ourselves from all selfish aims, all conscious prejudices, the more shall we find ourselves becoming progressively emancipated from those unconscious prejudices which cling around us as results of early misdirection and erroneous habits of thought, and which are more dangerous to our consistency than those against which we *knowingly* put ourselves upon our guard. And so in those judgments in regard to ourselves or others for which we are all daily appealing to the guidance of Common Sense, the safety of that guidance will depend upon the degree in which we have habitually aimed to cultivate our power of reasoning correctly, to try every question by first principles rather than by the dictates of a supposed temporary expediency, and above all "to be just and fear not." And every course of self-discipline thus steadily and honestly pursued, tends not merely to clear the mental vision of the *Individual*, but to ennoble the *Race;* by developing that power of *immediate insight*, which, in Man's highest phase of existence, will not only supersede the laborious operations of his Intellect, but will reveal to him truths and glories of the Unseen, which the intellect alone can see but "as through a glass, darkly."

[W. B. C.]

WEEKLY EVENING MEETING,

Friday, April 3, 1868.

Sir Henry Holland, Bart. M.D. D.C.L. F.R.S. President,
in the Chair.

Edward Frankland, Ph.D. F.R.S.
PROFESSOR OF CHEMISTRY, ROYAL INSTITUTION.

On the proposed Water Supply for the Metropolis.

Out of every thousand people existing upon this planet at the present moment, three live in London. Any matter, therefore, which intimately concerns the health and comfort of this vast mass of humanity, cannot but merit earnest attention; and, moreover, if that matter be connected with scientific research, I feel sure that the members of this Institution will require no apology even for its being brought under their notice a second time.

A year ago, I discoursed to you about the chemical considerations respecting the present Metropolitan Water Supply, and I mentioned the five schemes then proposed to remedy its obvious and serious defects—excessive contamination with sewage, and great hardness—the first rendering it unfit for drinking, and the second disqualifying it to a certain extent for washing and cleansing purposes. Those schemes to which I alluded on the last occasion were the following. *First*, the sources of the Severn, proposed by Mr. Bateman; *second*, the Cumberland Lakes, proposed by Messrs. Hemans and Hassard; *third*, the Thames water filtered through the Bagshot sands, suggested by Mr. Telford Macneill; *fourth*, extensive reservoirs constructed near the sources of the Thames, the scheme of Mr. Baily Denton; and *fifth*, the waters flowing down the slopes of the Derbyshire and Staffordshire hills, proposed to be brought to the metropolis by Mr. Remington.

At that time the quality of the waters obtainable by any of these schemes had been but little investigated, and that remark still applies to the last three schemes. But in the interval, the water yielded by the two first-named districts has been, at the instance of the Royal Commission on Water Supply, submitted to a searching chemical investigation by Dr. Odling and myself, and I am therefore enabled on the present occasion to speak with confidence as to the quality of the water from both these districts.

There are also one or two points of general scientific interest which have been brought to light during this inquiry, and which I also propose to touch upon:—these are, first, the curious effect of detritus from mines upon the quality of the water with which it is mixed; and secondly, the conditions which determine the action or non-action of water upon lead.

During the past year the processes of water analysis have undergone a complete revolution. It is one of my duties to report monthly to the Registrar-General upon the quality of the metropolitan waters, and in carrying out this work I found the methods of water analysis hitherto employed so untrustworthy as to render an almost entire remodelling of them absolutely necessary.

I propose, therefore, first to glance shortly at some of the innovations which have been made in this branch of chemical analysis. When water is to be submitted to chemical examination, it is of the utmost importance to have a sufficient and well-collected sample. On this occasion the completeness of the investigation, as regards the two proposed schemes, has been very materially assisted by the judicious choice of samples supplied by Dr. Pole, F.R.S., who went down to the districts and collected the samples which were afterwards submitted to chemical analysis by my colleague and myself.

The first thing to be determined in a water analysis is the "total solid impurity," as it is termed, *i.e.* the total amount of solid matter with which the water has been contaminated since it was submitted to the natural process of distillation. This quantity of solid impurity is determined by taking a known volume of the water and evaporating it down to dryness in a previously weighed platinum vessel. The solid impurity contains both organic matter and inorganic or mineral matter. The most important of these two classes of substances contained in the solid residue is undoubtedly the organic matter. Now, even at the present moment, the actual weight of this organic matter cannot be determined by chemical analysis; in fact there is no process known to science by which its weight can be even approximately estimated; but it is possible to determine, in a given bulk of water, the quantity of the two principal constituents of this organic matter, *viz.* the carbon and nitrogen which enter into its composition. For this purpose a separate quantity of the water is evaporated down to dryness; but in this case the process is conducted in a glass vessel, and before evaporation the water is mixed with sulphurous acid in order to expel the carbonic acid, which is partly dissolved in the water and partly combined with lime and magnesia. Other precautions also have to be taken, but I hesitate to enter into the details, which I fear would only weary you. However, I think that it is desirable just to show you the general plans on which the determination of the organic carbon and nitrogen, in the residue thus obtained by evaporation in the glass dish, is effected. The operation is performed in the following manner:—The contents of the glass vessel are very carefully scraped out and rubbed off the sides of the vessel by a substance known as chromate of lead, a finely powdered somewhat gritty material, which very completely effects this object, and enables us to transfer the water residue gradually into a piece of hard Bohemian glass tube closed at one end. This tube is then filled up to within about four inches of the mouth with coarsely granulated oxide of copper, and upon that is placed a small quantity of bright metallic copper to decompose oxidized compounds of nitrogen. The tube is then laid in a gas furnace, called "the combustion furnace." Before combustion commences the entire tube is made perfectly vacuous, all the air is pumped out of it, so as to get rid of the atmospheric nitrogen which would vitiate our result. This is done by means of a mercurial pump invented by Dr. Sprengel, by means of which we can extract almost the last trace of atmospheric air contained in the tube. The latter is then gradually heated to redness, during which process the carbon and nitrogen of the organic matter in the water residue are converted, the first into carbonic acid gas, and the second into nitrogen and nitric oxide gases. From the volume of each of these gases the weights of carbon and nitrogen can be calculated with great precision. (Experiment performed.)

Now the nitrogen in the result of the analysis is also derived from any ammonia present in the water, and it is therefore necessary to determine how much is due to that source. This estimation of ammonia is perhaps the only *rapid* and *easy* process connected with water analysis which may at the same time be regarded as satisfactory. For these simple processes of analysis when they come to be rigorously tested generally prove to be very incorrect; but this has survived the test of experience, and is capable of determining the result with great precision and readiness. I have here five glass cylinders. The water in the first contains no ammonia at all; the second contains a certain small quantity; the third twice as much as the second; the fourth three times as much, and the fifth four times as much as the second. To each of these vessels I shall now add an equal volume of a test solution, which strikes a peculiar yellow or orange-yellow colour with the ammonia in the vessels. This is known as the Nessler test, having been invented by a German chemist of that name. (The experiment was performed, the water in the four last vessels assuming different shades of orange colour, in proportion to the quantity of ammonia contained in them; the water in the first vessel remaining colourless.)

Now we have still one other process at which it is necessary to glance for a moment, *viz.*—the process for determining the nitrogen existing as nitrates and nitrites. It is called combined nitrogen, but it is not organic nitrogen, although it has in most cases been derived from organic matter. The water residue used for the determination of the amount of solid impurity is dissolved in a small quantity of water; sulphate of silver is then added, by which the chlorides are converted into sulphates. The resulting liquid after filtration is transferred to the upper part of a glass tube filled with mercury. It requires to be mixed with rather more than its own weight of sulphuric acid, which is introduced in the same way. It is then only necessary to shake up this mixture, the mouth of the tube being closed with the thumb. Very soon the mercury begins to act on the nitric acid, converting it into a colourless permanent gas called nitric oxide, which only requires to be measured in order to determine the amount of nitrogen originally present in the water in the shape of nitrites and nitrates.

There is only one other determination I will trouble you with, and this I do principally for the purpose of introducing to your notice a very ingenious piece of apparatus, an application of the Sprengel pump, which has just been contrived by my assistant, Mr. McLeod. It is designed to extract the gases which are dissolved in waters. By this instrument we can not only measure the whole of the gases present in the water, but we can determine how much of the gases can be expelled at the ordinary temperature, and how much more will come off when you boil the water *in vacuo.* This gas is then submitted to the usual eudiometrical investigation, to ascertain the quantity of carbonic acid, nitrogen, and oxygen,—the three gases which almost invariably occur in the waters submitted to analysis. (Apparatus shown at work).

Now it is not necessary for me on the present occasion to go at all into the details as regards the sources of the two proposed water supplies for London. This I did on the former occasion pretty fully. I will only refer you for a moment to the large map before you, which shows the districts from which the supplies would be taken and the course of the conduits to the metropolis. By the Welsh scheme, the water would be collected on the slopes of Cader Idris and Plynlimmon, from whence it would be brought by a conduit to within ten miles of London, where it would be stored in reservoirs 400 feet above high-water mark. The other scheme proposes to bring the water from the lakes of Cumberland, past several large towns, laying under contribution the Bala Lake, in Wales, if necessary, and the combined waters would then be brought to the metropolis after distributing a certain amount to the large towns on their route.

It is, perhaps, necessary just to say a word or two in order to disabuse your minds of the idea that these schemes are intended to inflict any injury upon the present water companies. Ample provision is made in these schemes for the complete compensation of the existing companies, and the only conceivable mischief in this respect which can be done by the adoption of one scheme or the other, would be the abolition of certain Boards of Directors which now exist, for the administration of the affairs of the eight or nine companies which supply London.

These schemes are of course very costly. It quite staggers one at first to think of the amount it is proposed to expend upon them. Thus, Mr. Bateman's scheme, which is to bring water from the mountains of North Wales, is calculated to cost, for a supply of 220,000,000 gallons per day, the sum of 10,850,000*l.*; whilst the scheme for bringing water from the lakes of Cumberland is put down, for 250,000,000 gallons a-day, at 13,500,000*l*. Now these are startling figures; but I imagine that all we have to look at is the simple question, How much shall we have to pay for the water when these schemes are carried out? If you go into that matter you will find, according to the calculations of the engineers—I will not say they are always to be implicitly relied upon, perhaps a certain percentage must be allowed—but taking their calculations as correct, it actually follows that after compensating the existing companies, and after expending this enormous amount upon the works, we shall be supplied with this very pure water at a less cost than that which we pay at the present moment. We pay at present about 1*s.* 5*d.* in the pound of rent for water. By Mr. Bateman's scheme we should be charged a domestic rate of 10*d.* in the pound, or two-thirds of what we now pay, *plus* a public rate of 2*d.* Messrs. Hassard and Hemans' scheme would be met by a domestic rate of 1*s.* 1*d.* in the pound. Now I think, if we are actually to be gainers by this transaction, the enormous sums necessary to be expended upon these works need not frighten us, and need not prevent us from taking them into our serious consideration.

Let us just pause for a moment to consider the purely mechanical relations of the proposed to the present metropolitan supply, because this will somewhat help you to comprehend how it is that, having expended all this money upon the works, we shall still have water cheaper. In the first place, every gallon of water which is now delivered in London has to be pumped up from nearly the sea level, to an average height of about 250 feet. Then, again, the present supply is intermittent; the proposed will be constant. With regard to the pumping part of the process, that in the proposed scheme would be replaced by the work of gravitation. The gigantic and magnificent engines employed at the present moment in London for raising this vast volume of water—100,000,000 gallons daily—are painful for the philosopher to contemplate. You have here a stupendous waste of power employed in doing over again an amount of work which was previously executed for us gratuitously. The sun, in his prodigality of power, flings up far above the cross of St. Paul's this daily supply of 100,000,000 gallons, and we, in our imbecility, allow it to soil itself by flowing down again nearly to the level of the sea, and then we erect immense pumping engines and expend 200 tons of coal daily to raise this water a fraction of the height from which we had previously allowed it to fall. All this will be saved by the proposed schemes.

We talk of the exhaustion of our coal fields and of the necessity of conserving our supply as much as possible, and although the amount thus saved would make but a poor figure in Mr. Jevons's 100,000,000 tons a year, yet this is a kind of work which can be done better by solar heat than by the action of coal; and it is not very often that we are thus able to substitute, with advantage, natural for artificial force.

Now with regard to the quality of these waters which it is proposed to bring to London, you have in the following Tables a comparative statement, showing the results obtained by the analysis of the proposed Welsh and Cumberland waters, and of the present metropolitan water supply :—

TABLE A.—*Results of Analysis of Welsh, Cumberland, and London Waters.*

100,000 PARTS OF WATER GAVE—

—	WELSH.			CUMBERLAND.			LONDON.		
	Max.	Min.	Mean.	Max.	Min.	Mean.	Max.	Min.	Mean.
Total solid impurity ..	9·60	2·79	4·85	13·60	2·14	4·74	59·20	23·12	32·66
Organic Carbon..	1·040	·200	·460	1·059	·066	·276	1·020	·064	·270
Organic Nitrogen	·013	·000	·006	·068	·000	·010	·082	·000	·025
Ammonia	·008	·000	·003	·006	·000	·002	·120	·000	·003
Nitrogen as Nitrates and Nitrites	·068	·000	·017	·045	·000	·009	·564	·054	·323
Total combined Nitrogen	·069	·002	·025	·088	·003	·021	·578	·059	·354
Previous sewage or manure contamination	360	0	47	140	0	6	5330	230	2930
Hardness	3·0	·4	1·4	8·0	·7	2·2	30·0	15·4	20·13
Lime	1·126	·217	·599	3·096	·361	1·113	16·3	8·170	9·822
Magnesia	·404	·144	·288	·727	·111	·272	1·048	·754	·890
Potash	·243	·053	·126	·267	·063	·158	·964	·734	·851
Soda	·916	·490	·679	·683	·356	·532	2·240	·834	1·666
Sulphuric Acid	1·746	·290	1·093	1·941	·020	.969	4·650	2·683	3 674
Carbonic Acid	·614	·000	·201	2·276	·163	·691	8·524	5·517	7·187
Silica	·581	·026	·254	·221	·061	·133	·899	·715	·834
Chlorine	1·487	·573	·876	·653	·130	·490	1·526	1·413	1·480

TABLE B.—*Analysis of London Waters,* 1867–68.

100,000 PARTS OF WATER CONTAINED :—

—	Total solid impurity.			Organic Carbon.			Organic Nitrogen.			Previous Sewage Contamination.			Hardness.		
	Maximum.	Minimum.	Mean.	Maximum.	Minimum.	Mean.	Maximum.	Minimum.	Mean.	Maximum.	Minimum.	Mean.	Maximum.	Minimum.	Mean.
THAMES.															
1867	32·8	23·7	28·5	1·020	·161	·272	·082	·000	·013	3290	1050	2062	22·8	16·0	19·3
Jan., 1868..	32·2	29·2	30·9	·542	·271	·399	·062	·027	·018	3360	2920	3150	19·7	15·4	17·3
Feb. „	32·6	30·0	31·4	·360	·324	·339	·055	·031	·043	3130	2790	3010	21·1	18·4	19·3
Mar. „	32·6	28·8	30·0	·289	·136	·216	·040	·012	·028	2830	2150	2388	21·4	18·3	19·3
RIVER LEA.															
1867	35·7	23·1	27·5	·382	·104	·196	·015	·000	·005	2950	230	1611	23·1	16·3	19·3
Jan., 1868	36·0	30·2	33·1	·147	·115	·131	·024	·014	·019	3300	2760	3030	22·8	20·5	21·6
Feb. „	34·4	30·8	32·6	·272	·217	·244	·037	·026	·031	3400	3240	3320	20·5	20·5	20·5
Mar. „	30·0	27·4	28·7	·118	·059	·088	·022	·010	·016	2240	1990	2115	20·5	18·5	19·5
KENT CO.															
1867	42·0	31·8	39·3	·254	·088	·131	·004	·000	·002	4820	2890	3619	29·1	21·1	25·6
Jan., 1868	..	..	44·8	..	..	·064	..	..	·013	..	..	3770	..	..	26·2
Feb. „	..	..	59·2	..	..	·081	..	..	·013	..	..	5330	..	..	30·0
Mar. „	..	..	70·2	..	..	·093	..	..	·029	..	..	3680	..	..	32·3

The quantity of the solid impurity contained in a water is a very important matter, apart from the consideration of the quality of the substances which compose this impurity. Waters leaving a small amount of residue upon evaporation are usually well fitted for domestic use. They are invariably the best for manufacturing purposes, as they effect a great saving in heat when used for steam boilers. I was shown the other day some cakes of carbonate of lime, a quarter of an inch thick, which had been removed from a locomotive boiler at the Deptford Railway Station, in which they had been formed in forty-eight hours; through this substance heat passes with extreme slowness, so that a considerable quantity of fuel is wasted. It will be seen from the first of the above tables that, on an average of all the samples, the total solid impurities amount in the two schemes to about 1-7th of those in the present water supply; but if we might venture to take the water in the proposed large storage reservoirs as equal in this respect to the water now stored in the lakes, it would be about 1-10th of that which is found in London waters.

Now this solid residue is partly mineral and partly organic. Let us glance first at the organic portion. This organic matter present in the original water may be either living or dead. The detection of the former class of impurities belongs more to the province of the naturalist than to that of the chemist; but it may be remarked in passing, that this form of organic impurity must necessarily be in suspension and not in solution. We cannot conceive of organized beings existing in solution—it is impossible. But it does not from this follow that these suspended matters can be removed from water by filtration.

It is well known that the ova of many species of animalculæ cannot be removed thus, they pass through the best filters; and it has also been proved that what is believed to be the cholera poison passes through filters, and cannot be arrested. This is a most important consideration in connection with water which is contaminated with sewage and manure matters; and it is necessary that such water should, at all events, be as well filtered as possible. The present water companies supplying London cannot possibly be blamed for the original quality of the water which they supply. They cannot hinder the 600,000 persons who live on the banks of the Thames from pouring their refuse into the river; but they can filter this impure water. They can, and indeed by Act of Parliament they are supposed to be compelled to deliver this water in a bright, transparent, and filtered condition; and they can in this way, as far it is possible by filtration to do it, remove these suspended organic contaminations from the water.

But how does the matter stand? Here is a sample of water which I drew from the Lambeth Company's main on the 4th of March. You see that the water is not filtered. It is filtered by Act of Parliament! but it is curious to observe that so much pollution can pass through an Act of Parliament. Here too is a sample of the same company's water collected on the 21st of January; and it is a fact, that during the whole of that interval and almost up to the present time, this water has been much in the same condition. Those of my audience who are supplied by the Lambeth Company, or the Southwark and Vauxhall Company, or by the Chelsea Company, will bear me out as to the condition in which those companies have delivered water during the past two months. In fact, not only for the past two months, but during the entire year, water is often delivered in London very imperfectly filtered. The Southwark Company during the whole of last year, with one exception, delivered from its mains, when the samples were drawn for analysis, turbid water, imperfectly filtered—most of the other companies were to a less extent guilty of the same thing. Of the companies which draw from the Thames, the West Middlesex and the Grand Junction are the two which filter their water best; but the only company which delivered water uniformly transparent and well filtered was the New River Company.

I have stated that the absolute quantity of the organic matter in solution in water cannot be ascertained, but the amount of carbon and nitrogen contained in this organic matter can be estimated by the process of combustion which I have exhibited to you. The amount of organic carbon and nitrogen in the several waters I have referred to, is represented in the second and third lines of table A, and in the second and third columns of table B you will see that, with regard to these elements of the organic matter in solution, there is not a very striking difference between the three different classes of waters. There is an excess of organic nitrogen in the case of the London water, and of organic carbon in the case of the Welsh waters.

The organic matter, of which the elements are thus determined, may be either animal or vegetable, and the nature of it has much to do with the probability of its being noxious or innocuous. The animal or vegetable source of the organic matter may be judged of by the proportion of nitrogen to carbon, as determined by analysis: that from animal sources contains a larger proportion than that derived from vegetable sources; and in this way it is easy to see that the organic matter in the Welsh and Cumberland waters is of a different character from that contained in the London waters. The London river-waters, especially when turbid, contain a much larger proportion of nitrogen to carbon than is contained in other waters, thus proclaiming the animal origin of some portions of the organic matter.

When I addressed you on this subject last year I stated that by operating upon one litre of water, one per cent. of unchanged sewage could be detected with certainty, but that smaller percentages ought, in operations upon such a small quantity of water, to be considered as falling within the possible errors of experiment. In like manner, by operating upon 10 litres of water 1-10th of a per cent. of unchanged sewage could be detected. During the past year, however, this process of analysis has been so improved that an amount of organic nitrogen corresponding to at most 3-100ths of a per cent. of unchanged sewage can now be detected with certainty in *one* litre of water.

Now about 4-5ths of the organic nitrogen contained in *perfectly fresh sewage* exists there as urea which undergoes such rapid decomposition, into the mineral compound carbonate of ammonia, that little or none of it ever reaches the Thames from the towns whose sewers debouch into this river. As average London sewage contains 10 parts of combined nitrogen in 100,000 parts, it follows that 100,000 parts of this sewage as it flows into the Thames will contain only 2 parts of organic nitrogen. Further, if the sewage of the 600,000 persons who drain into the Thames above the point whence the water companies draw their supply have the strength of average London sewage, it will amount to 18,000,000 gallons daily, and if the average flow of the river at Teddington be taken at 800,000,000 gallons daily, it follows that the river will there contain 2250 parts of sewage in 100,000 parts, or 2¼ per cent. This quantity of sewage, if in the condition as delivered at the sewer outfall, would contaminate the whole volume of the river, only to the extent of ·045 part of organic nitrogen in 100,000 parts of water. Now on the 21st of January last the water delivered by the five companies drawing their supplies from the Thames contained the following amounts of organic nitrogen in 100,000 parts :—

Chelsea (turbid)	·058	Grand Junction (clear)	·031
West Middlesex (clear)	·027	Lambeth (turbid) ..	·062
Southwark (turbid) ..	·061		

It will be seen, therefore, that three out of the five samples of water actually contained more organic nitrogen than would be due to the admixture of the 18,000,000 gallons of sewage which are poured into the Thames above the point from which these samples came. But Thames water holds in solution a certain amount of peaty matter which contains organic nitrogen; a sufficient proportion of this substance, however, to furnish the above larger quantities of organic nitrogen would render the water brownish-yellow when viewed in a quart decanter, whilst these samples of Thames water were, when filtered, colourless or nearly so. I am therefore of opinion that the Thames water delivered in London by the Chelsea, Southwark, and Lambeth companies on the 21st of January last contained unoxidized sewage. This opinion is confirmed by the results of some experiments which I have recently made in my laboratory, and which show that, contrary to the generally received opinion (which is, however, based upon no reliable experimental data), sewage in which the urea is already decomposed undergoes further change with extreme slowness, even when freely exposed to the air and mixed with large volumes of water. Thus I find that a mixture of weak sewage from one of the London sewers with nine times its volume of water (containing bicarbonate of lime in solution) at a temperature of 20° to 25° C., and well agitated every day by being made to flow in a thin stream through three feet of air, oxidizes but to a slight extent in the course of eight days. Immediately after mixture this sewage-contaminated water contained ·267 part of organic carbon and ·081 part of organic nitrogen in 100,000 parts, whilst after 96 hours it still contained ·250 part of organic carbon and ·058 part of organic nitrogen, and even after the lapse of 192 hours the undecomposed organic matter still contained ·200 part of organic carbon and ·054 part of organic nitrogen.

In connection with the organic matter in water, the investigation of the Welsh and Cumberland samples revealed a very curious effect produced by the admission of the detritus from lead and other mines into the waters of the streams and lakes. It was found, upon analysis, that water thus mixed with the milky streams from the crushing-engines of mines contained a wonderfully small quantity of nitrogenous organic matter. You will see this brought out in the following table:—

Effect of Detritus of Lead Mines upon the Organic Matter in Water.

	Organic Carbon in 100,000 parts of Water.	Organic Nitrogen in 100,000 parts of Water.
CUMBERLAND WATERS.		
Glenridding Beck	·116	·000
Stream flowing into Thirlmere ..	·066	·001
Goldrill Beck	·262	·001
WELSH WATERS.		
Ceryst	·209	·000
Upper Clywedog	·544	·000
Lower Clywedog	·212	·001
Tarannon and Ceryst	·304	·001

This table shows that whilst some of these waters exhibit a rather large quantity of organic carbon, they contain very little or no organic nitrogen. And further, these waters, though they hold in solution a considerable amount of peaty matter, are perfectly colourless when seen in a quart decanter; but when viewed through a stratum fifteen feet thick they exhibit the magnificent blue-green tint of absolutely pure water, a tint which is brought out when water is passed through animal charcoal. We may illustrate the action of this crushed quartz of lead mines and of animal charcoal, by three samples of the water delivered to this Institution by the Grand Junction Company, and which are contained in the tubes before you, each of which is fifteen feet long. The centre tube contains the water just as it passes into the cistern, the water in the second tube has been shaken with powdered flint, whilst the water in the third tube has been passed through animal charcoal. If we now send through each tube a parallel ray of electric light, which ray will have to pass through a stratum of about fifteen feet of water, you will perceive that the first gives a yellow-brown tint upon the screen; the second, a beautiful green tint, and the third, a turquoise colour: the last two powerfully reminding the observer of the lakes of Lauerz and Zug, as seen from the summit of the Rigi. (Experiment performed.) In fact this is doubtless the chief cause of that magnificent colour which we witness in many of the Swiss lakes, and which we see for instance in the Rhone when it leaves the lake of Geneva, and the Limmat as it flows from the lake of Zürich. The streams running into the heads of these lakes come in turbid and filled with finely-crushed quartz and other minerals, the detritus from the glaciers which are the source of those streams. In the lakes these fine particles of mud subside and attract to themselves the peaty colouring matter which is to be found in almost all waters.

We see in two of the English lakes some indications of this blue-green tint appearing, and it is precisely in the localities where the streams from the lead mines come down into the lakes. You see near the mouths of those milky streams which come down into Ullswater from Glenridding, and from the "Old Man," into Coniston Lake, the indications of this precipitation and removal of those brown substances which discolour the natural waters of our lakes. We have thus here, perhaps for the first time, evidence of improvement of the quality of water by the admission into it of manufacturing refuse. Hence the diversion of these waters coming from lead and other mines, which would seem at first sight to be necessary, need not be effected; on the contrary, their admission into the lakes would be of great benefit to the waters, they would to some extent decolorize them, and would tend to reduce the nitrogenous organic matter to the lowest possible amount. There appears to be no need to fear that such streams will carry anything into the lakes which will be deleterious to the drinker. All those streams have been carefully examined for lead, arsenic, copper, &c., and only in two cases has the faintest trace of lead been discovered, and the quantity was so minute that it is absolutely impossible it could be deleterious, even if the water coming from the mines themselves were to be drunk, but mixed with the large quantities of the lake water, it becomes utterly inappreciable.

The fatal effect said to be exerted upon fish by these milky streams from mines is most probably due to a mechanical action of the finely divided quartz upon their organs of respiration—an effect analogous to that (but of an exaggerated kind) from which the Sheffield grinders notoriously suffer.

Having thus discussed the organic portion of the solid impurity of these waters, let us now turn to the inorganic or mineral portion, which may be conveniently divided, as regards its most important constituents, into three subdivisions, *viz.*:—

1. Soap destroying substances.
2. Mineral compounds, constituting chiefly the skeleton of decomposed sewage or manure.
3. Poisonous substances, such as arsenic, copper, and lead.

The first or soap destroying category of substances communicate to water the quality called hardness. These substances are the salts of lime and magnesia; and the quantity of them contained in the proposed, as compared with the present, metropolitan water supply will be seen on reference to the above analytical table. The hardening effect of these substances is also given in a separate line of the same table, from which it will be seen that the proposed is only about 1-10th as hard as the present water supply.

Tastes differ as regards hard or soft water for drinking purposes, and medical arguments have from time to time been advanced, now in favour of and now against each. It has been asserted in this country, for instance, that hard water is necessary for the formation of bone, and that the finger of Providence points to the advantage of hard water by the profusion of calcareous strata occurring in the earth's crust, whilst M. Belgrand states that the inhabitants of the hard-water districts of France notoriously suffer from carious teeth. It would probably be extremely difficult to prove either of these assertions. As regards the enormous advantages of soft water for washing, cleansing, and manufacturing purposes, there is, however, no difference of opinion. In Glasgow alone the annual saving of soap only, by the introduction of Loch Katrine water, for a previous supply of very moderately hard water, has been estimated at 36,000*l.* Having had the opportunity of comparing a six years' experience of the soft water supplied to Manchester, with a subsequent ten years' experience of the hard water of London, I can state that the soft water was for all purposes preferred by every member of my family. On removing from Manchester to London, the repugnance to drink the hard water of the latter city was at least as marked as that which I have sometimes noticed in persons making the transition in the opposite direction.

The hardness of the London waters is chiefly what is termed *temporary* hardness; that is, it is caused by the carbonates of lime and magnesia, the greater portion of which is gradually deposited on boiling the water for half-an-hour. By reason of this softening of such water by boiling, temporarily hard water is considered to be less objectionable than water of the same degree of permanent hardness. My own experience leads me to the conclusion that the advantages of temporary over permanent hardness have been considerably overrated. In reality, water used for domestic purposes is, even when used hot, either not heated to the boiling point, or is boiled for too short a time to remove more than a small proportion of its temporary hardness. Thus, water drawn from the kitchen boilers of a dwelling-house and of the Athenæum Club was usually almost as hard as the cold water with which they were supplied, as is seen from the following table:—

Date and Hour.	Hardness of Cold Water.	Hardness of Hot Water.
Sept. 30th, 1867, 8 P.M.	14°6	13°6
Oct. 1st „ 8 P.M.	14·4	13·9
„ 2nd „ 8 A.M.	14·4	13·4
„ 3rd „ 9 P.M.	14·6	11·6
„ 4th „ 8 A.M.	14·6	7·6
„ 7th „ 8 P.M.	14·4	11·7
„ 8th „ 8 A.M.	14·4	12·1
„ 9th „ 8 P.M.	15·4	14·3
„ 10th „ 8 P.M.	15·9	11·9
„ 11th „ 8 A.M.	15·9	8·4
„ 12th „ 8 A.M.	16·1	11·9
Nov. 8th „ 5 P.M.	18·7	18·4
„ 11th „ 5 P.M.	18·7	18·6
„ 12th „ 6 P.M.	18·7	18·4

The amount of soap destroyed by the use of various waters for washing purposes is seen from the following table, in which certain Welsh and Cumberland waters are also introduced for the purposes of comparison :—

Soap destroyed by 100,000 *lbs. of various Waters.*

	lbs. of Soap destroyed.
METROPOLITAN WATERS.	
Thames Water	212
River Lea	204
Kent Company's Water	265
OTHER WATERS.	
South Essex Company's Water	253
Caterham Company's Water..	84
Water supply of Worthing	285
„ „ Leicester	161
„ „ Manchester	32
„ „ Preston	80
„ „ Glasgow (Loch Katrine)	4
„ „ Lancaster	1
Bala Lake	5
Thirlmere	8
Haweswater	16
Ullswater..	23

In the recent supply of water to Paris from new sources, the importance of soft water attracted the attention of the eminent engineer M. Belgrand; a close investigation of the available sources, however, soon showed that he had unfortunately but little choice, as the really soft streams of the Fontainebleau sands (the minimum hardness is however 6°) and of the granite of Morvan (minimum hardness 2·2°) were more dribblets. Of the latter M. Belgrand says—"Sources qui donnent les eaux les plus pures du bassin de la Seine; déviation vers Paris impossible, en raison du peu d'importance des sources." Hence the river Vanne (17°–20°), somewhat softer than the Thames, was the softest available source, and having first conclusively demonstrated this, he consoles the Parisians by saying—"Les eaux du granite, du greensand et des sables de Fontainebleau, qui sont chimiquement plus pures, sont beaucoup moins agréables à boire."

The second category of inorganic substances contained amongst the solid impurities of waters, consists of *the mineral compounds constituting chiefly the skeleton of decomposed sewage or manure.* The putrescible nitrogenous organic matters present in water, or in the soil through which water percolates, undergo gradual oxidation and decomposition, by which their carbon and hydrogen are converted into carbonic acid and water, and their nitrogen into ammonia, nitrous and nitric acids. The last three remain in the water, constituting a record of previous contamination with putrescible nitrogenous organic matter. But rain-water always contains ammonia, and, as Dr. Bence Jones has shown, also nitrous and nitric acids. The nitrogen in these forms in rain-water, as it finds its way into rivers and springs, amounts in the aggregate to ·032 part in 100,000 parts of water, therefore this amount must be deducted from that found on analysis, as nitrogen derived from aerial sources. The remainder, if any, represents the nitrogen derived from putrefied nitrogenous organic matters with which the water has been in contact. To express this in terms of some known standard, I employ average filtered London sewage, which contains 10 parts of nitrogen in the form of putrescible organic matter in 100,000 parts. Thus, a water which contained one part of nitrogen in 100,000, as nitrous acid, nitric acid and ammonia would contain in 100,000 parts, the nitrogenous remains or skeleton of an amount of putrescible organic matter equal to that contained in 10,000 parts of average filtered London sewage. Such a water therefore is said to have a previous sewage contamination of 10,000 parts in every 100,000 parts. But it may be asked, Is this a true record of the previous history of the water in this respect? I believe it to be so, as far as it goes. *I believe that this nitrogen as truly represents a quantity of previously existing putrescible organic nitrogenous matter, as that the bones of a megatherium demonstrate the previous existence of an individual of that species;* but as the geological record of previously existing organisms is imperfect, so is the nitrogenous record, just as chemical and mechanical agencies have broken up and dissipated the remains of millions of animals during long geological periods, so does the action of growing plants, and perhaps also of living animals, remove from water, in a few hours or days, some portion of this skeleton of previous putrescible organic matter. Thus by storage in large reservoirs, the East London Company reduced the previous sewage contamination of the River Lea last summer from about 2000 down to 230 parts in 100,000. The previous sewage contamination of a water as determined by analysis is therefore a minimum quantity.

But in addition to the aerial for which due allowance is made, can there not be some other source of this skeleton than putrefied sewage or manure matter? Can it not be derived from putrefied vegetable matter—from peaty matter for instance? Without utterly denying the possibility of this, I venture to assert that nowhere, in this country at least, nor probably on the continent of Europe, is there such a quantity of nitrates, nitrites, or ammonia produced from vegetable sources as to appreciably affect the truth of my proposition that the nitrogen in these forms obtained by waters from terrestrial sources is substantially due to the putrefaction and oxidation of sewage and manure matters.

It has been objected to this view of the origin and significance of these forms of combined nitrogen, that waters derived from comparatively deep wells, in the chalk for instance, contain them in large quantities; thus the Kent Company's water exhibits a previous sewage or manure contamination of from 3000 to 5000 parts in 100,000. It is difficult to understand how such an objection could have originated, and it certainly disappears on examination; for instance in the above case, it is well known that a very large proportion of the water collected in the London chalk basin consists of the drainage from manured land, and it is doubtless from this source that the large proportion of nitrates existing in this water is derived.

According to Mr. Way's analysis, the drainage water from cultivated land contains an amount of nitrates corresponding to the following proportions of previous sewage contamination in 100,000 parts:—

	Maximum.	Minimum.	Mean.
Previous sewage contamination of drainage-water from manured land	54,490	7,040	20,370
Ditto from pasture-land, unmanured	2,100	180	830

The results of the examination of various well-waters contained in the following table, further illustrate this point:—

Previous Sewage or Manure Contamination in 100,000 *parts of various Well-waters.*

Names of Waters.	Ammonia.	Nitrogen as Nitrates and Nitrites.	Previous Sewage Contamination.
Artesian Well at Grenelle	..	·006	0
Chalk Well at Caterham	·009	·000	0
Water delivered by Kent Company	·001	·408	3770
Water supplied to Worthing	·000	·426	3940
Water delivered by the South Essex Company	·006	·848	8205
Shallow Well at Leyland, near Preston	·003	2·466	24360
" at Ledbury	·001	1·575	15440
" at Redhill	·002	1·446	14160
" in Aldgate	..	3·840	38080
" in Minories .. .. .. : ..	..	5·738	57060
" in Leadenhall Market	..	5·769	57370
" in St. Nicholas Olave, Churchyard	..	7·596	75640
Well in the Rue Traversine, Paris	..	30·029	299780
Royal Institution Well-water	·001	4·355	43240

With two remarkable exceptions the above results show the greatest previous sewage contamination precisely in those places where it would be predicted; thus the shallow well-water of Leyland, near Preston, consists almost entirely of the drainage from cesspools and market-gardens, through a sandy soil, the latter being heavily manured with night-soil, stable manure, and guano. It need therefore excite no surprise that nearly 25 per cent. of this water has been in a condition equivalent to average London sewage. The quality of the waters taken from four of the city pumps and from the well in the Royal Institution[*] needs no comment; these shallow wells are now recognized as being fed by oxidized and somewhat diluted sewage. It is, however, in the well of the Rue Traversine, in Paris, that this kind of contamination reaches perhaps its maximum. The cesspool system is still in full activity in Paris, and the soil of that city is saturated with liquid manure of such a strength that one gallon of it is equivalent to three gallons of average London sewage.

As already mentioned there are in the above table two remarkable exceptions to the general previous sewage contamination of well-waters. These are the artesian well at Grenelle and the chalk well of the Caterham Water Company. With regard to the first, it is evident that the pressure of water which supports a column of 122 feet above the surface at Grenelle, precludes the possibility of admixture with the drainage of Paris, still there can be little doubt that the water supplying the chalk of the Paris basin is, to some extent at least, contaminated by manure, although the land through which it drains is far less generally cultivated than that through which the water supply of the London chalk percolates. The water from the Caterham Company's well, comes, I believe, from a greater depth than that of the Kent and South Essex Company's, and this circumstance, coupled with the observation of Mr. Dugald Campbell that the water of the deep chalk wells, unlike that

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[*] As this water enjoyed for a long time a very high reputation in the domestic department of this Institution, and as I have been frequently and very earnestly requested to withdraw a prohibition which I placed upon its use in the cholera year, 1866, I append, for my own justification, a more complete analysis.

	In 100,000 parts.
Total solid impurity	93·7
Organic carbon	·440
Organic nitrogen	·085
Nitrogen as nitrates and nitrites	4·355
Ammonia	·001
Total combined nitrogen	4·441
Previous sewage contamination	43240
Actual contamination with unoxidized sewage	4250
Hardness	32·5

The gases dissolved in this water contained scarcely a trace of oxygen. A half-pint glass of it contains nearly a quarter of a pint of water which has previously been in the condition of average London sewage, besides a dessert spoonful of actual or unoxidized sewage. *It seems, therefore, highly probable that filtered and tolerably well-oxidized sewage, in its undiluted condition, would furnish the most popular water supply for London.* Such is the reliability of instinct in these matters.

of the shallower chalk-wells, is free from nitrates, and taken in connection with the fact that there is free water-communication between the upper and lower chalk, points to the conclusion that chalk possesses the property of abstracting nitrates from water. If this be the case, it would also account for the circumstance that the water of the shallow chalk-wells exhibits much less previous sewage contamination than might be expected; the average amount of nitrates found by Mr. Way in drainage water would indicate a previous sewage contamination in the chalk-water, equal to about 20,000 parts in 100,000, whilst the contamination actually exhibited in the case of the Kent, Worthing, and South Essex Companies waters is only: Kent 3770, Worthing 3940, and South Essex 8205 in 100,000 parts.

I have extended this investigation to various river and lake waters, as well as to spring waters, and have been here much indebted, as regards the non-British waters, to M. Boussingault's researches on the presence of nitrates in waters. The following tables exhibit the results of this investigation:—

Previous Sewage, or Manure Contamination, in 100,000 *parts, of various River and Lake Waters.*

Names of Waters.	Ammonia.	Nitrogen as Nitrates and Nitrites.	Previous Sewage Contamination.
RIVER WATERS.			
Nile	..	·102	700
Rhine, at Bâle	..	·026	0
Seine, at Notre Dame	..	·152	1200
Ourcq	..	·223	1910
Thames	·005	·234	2062
Lea	·002	·220	1901
Severn (near source)	·003	·007	0
Lower Clywedog	·004	·006	0
Tarannon	·008	·024	0
Ceryst	·001	·052	210
Carno	·003	·049	190
Banw and Eira	·004	·023	0
Vyrnwy	·003	·011	0
Tylwch	·003	·004	0
Upper Rothay	·003	·002	0
Lowther	·002	·003	0
Kent	·001	·045	140
Sprint	·000	·021	0
Fourteen other Cumberland Streams	..	..	0
LAKE WATERS.			
Bala Lake	·001	·000	0
Thirlmere	·003	·002	0
Haweswater	·004	·000	0
Ullswater	·003	·005	0
Watendlath Tarn	·002	·006	0
Loch Katrine	·002	·031	0
5 Lakes and Tarns examined by Boussingault	..	..	0

Previous Sewage, or Manure Contamination, in 100,000 *parts, of various Spring-waters.*

Names of Waters.	Ammonia.	Nitrogen as Nitrates and Nitrites.	Previous Sewage Contamination.
Mother Ludlaw's Cave	·001	·034	30
Water supplied to Ferette (Haut Rhin)	..	·039	70
Spring near Dürmenach (Haut Rhin)	..	·114	820
Source of the Roppensviller (Haut Rhin)	..	·168	1360
„ „ Arcueil	..	1·111	10790
„ „ But at Montmartre	..	8·563	85310
„ „ Martinet	..	·557	5250
„ „ Trois Meules, St. Etienne	..	·210	1780
Spring at Nimes	..	·129	970
Ebersbronn (Bas Rhin)	..	·447	4150
Water supplied to Woerth-sur-Saüer (Bas Rhin)	..	·259	2270
Source of the Ill, near Winckel (Haut Rhin)	..	·104	720
Liebfrauenberg Spring (Bas Rhin)	..	·005	0
Seltz (Bas Rhin)	..	·008	0
Mineral Spring of Bussang (Vosges)	..	·003	0
Water supplied to Thann (Haut Rhin)	..	·010	0
Source of the Boelacker (Haut Rhin)	..	·018	0
Spring at Castle Fleckenstein (Bas Rhin)	..	Traces.	0
Thermal Spring at Baden	..	·016	0
„ „ Dax	..	·013	0
Source of the Presle (East Pyrenees)	..	·013	0

The results embodied in the above tables throw considerable additional light upon this form of water contamination. They show in the first place that waters which have not been in suspicious company exhibit little if any previous sewage contamination, thus in the whole of the Cumberland and Westmorland district it only occurs in one instance (the Upper Kent which, as every tourist knows, has a little cultivated land on its banks), and that to a small extent only. In the Welsh waters again there are only three instances. The spring-water, which issues from the Greensand beneath an uncultivated but heather-covered surface at Mother Ludlaw's Cave, near Farnham, exhibits a mere trace of this contamination, whilst the waters of nine springs on the Rhine, in the Vosges, and in the Pyrenees, examined by M. Boussingault, exhibit no indications of previous sewage contamination. On the other hand, the spring forming the source of the But and issuing not far from the cemetery at Montmartre at once discloses its antecedents, and exhibits a previous sewage contamination of 85,310 parts in 100,000. It will be seen that the water of the Ourcq, which is now used only for watering the streets of Paris, exhibits a previous sewage contamination somewhat less than Thames water.

But what is the import of this previous sewage contamination? These skeleton compounds are innocuous, why trouble ourselves about them? True, they are innocuous, or nearly so; but inasmuch as they show that the water has been in contact with animal refuse they bring a heavy charge of suspicion against it. These refuse animal matters are known to contain that which is hurtful to human life. This hurtful matter is believed, on very strong evidence, to consist of spores, or germs of organisms, which are capable, under favourable circumstances, of producing in man such diseases as cholera, typhoid fever, and dysentery. Now such spores or germs, endowed as they are with vitality, will be likely to resist the oxidizing agencies which convert the rest of the animal refuse into carbonic acid, water, nitric acid, nitrous acid, and ammonia. For instance, if the contents of an egg were beaten up with water and poured into the Thames at Oxford, the organic matter would probably be entirely oxidized and converted into mineral compounds before it reached Teddington; but if the egg were thrown whole into the Thames at Oxford, it would, if it retained its vitality, be carried down to Teddington without any decomposition of its organic matter. There can be no doubt that the spores or germs of many organisms are in like manner capable of resisting for a long time the decomposing action of water. Now no practicable process is known by which these spores, once introduced into water, can be again removed or can have their vitality destroyed. Filtration will not do it; in fact it is well known to engineers that water is often contaminated with visible suspended matter which cannot be separated by filtration; thus M. Belgrand says, "Lorsque l'eau est troublée dans le fleuve, elle sort louche de nos filtres." And again, speaking specially of the London water supply, "Le mode de dégrossissage employé par les grandes compagnies anglaises, *très convenable à Londres, où l'on ne boit pas d'eau, ne vaut rien à Paris, où les femmes, les enfants, les vieillards de la classe ouvrière n'ont pas d'autre boisson.* J'ai constaté par moi-même, et les ingénieurs anglais n'en disconviennent pas, que l'eau sort des filtres *très chargée de matière organique.*" Again, in the account of his highly remarkable researches on vaccine and small-pox poisons, recently communicated to the Academy of Sciences, M. Chauveau says regarding the organic germs contained in these poisons, that they "ne se déposent jamais complètement dans les couches profondes du milieu ambient, et passent à travers tous les filtres."

Boiling even for several hours cannot be relied upon for the destruction of such germs, some of which have recently been shown to retain their vitality after four hours boiling; in fact there can now no longer be any doubt that, as contended by M. Pasteur, the cases of so-called spontaneous generation have all had their origin in ignorance of the excessive tenacity of life in the germs of the lowest organisms.

Nothing short of distillation therefore, as it is carried on in nature, can be relied upon to free, completely, sewage-contaminated water from its noxious constituents. Excessive filtration is doubtless to some extent a safeguard, and hence previous sewage contamination in chalk-water, if we could be certain that the water had been fairly filtered through some 100 feet of chalk, and that none of it gained access to the wells through fissures or swallow-holes, would have far less significance than it has in the case of a river water where the fine-suspended and noxious matters of sewage have but a comparatively slender chance of removal before the water reaches the consumer. We must also not forget that mere dilution fails, in the case of these suspended germs, to destroy their noxious quality, differing as they do in this respect remarkably from soluble poisons. The daily casting of a thousand fatal doses of strychnine into the Thames at Oxford ought not to occasion so much alarm amongst the London water-drinkers as the present flow of the Oxford sewage into the river, because the excessive dilution of the soluble strychnine would effectually prevent its producing any physiological effect. Each noxious living germ, on the other hand, contains within itself the power of indefinite multiplication and mischief. One such germ may be present in a wineglass-full of water, whereas it would be necessary to drink many thousand gallons of water to imbibe a noxious amount of strychnine, under the conditions just alluded to. I am therefore of opinion that water once contaminated with sewage or manure matter ought never again to be used for domestic purposes, if any other supply can be obtained; and I endorse the advice of M. Belgrand and the principle which guided him in the selection of his new water supply for Paris:—"On a dit des eaux potables, qu'elles étaient comme la femme de César, qu'elles ne devaient pas même être soupçonnées, et c'est mon avis."

A few words will now suffice regarding the third class of mineral matters that may be present in the solid impurities of waters, *viz. poisonous substances, such as arsenic, copper, and lead.* These substances are only likely to occur in waters connected with mineral workings—one of them only, lead, has been detected in the proposed supplies, and that only in two streams in Cumberland, and in quantity far too minute to require any further notice.

The following table exhibits a comparative view of the amount and quality of the gases contained in the proposed and present supplies:—

Gases expelled on boiling 100 *volumes of various Waters.*

Names of Gases	Welsh Waters.			Cumberland Waters.			Thames Water.			Distilled Water.
	Max.	Min.	Mean.	Max.	Min.	Mean.	Max.	Min.	Mean.	
Nitrogen ..	1·412	1·226	1·323	1·551	1·310	1·424	1·723	1·588	1·656	1·133
Oxygen ..	·642	·566	·612	·749	·667	·726	0·832	0·771	0·801	·617
Carbonic Acid	·335	·107	·227	·793	·085	·281	4·121	3·182	3·652	·105
	2·389	1·899	2·162	3·093	2·062	2·431	6·676	5·541	6·109	1·855

You will find that the gases contained in the Welsh and Cumberland waters are very similar in quantity and proportion to those found in recently distilled water. Collected as these waters are near the mountain ranges which constitute the great condensers of natural distillation, this is exactly the result we should expect. The London waters differ mainly in containing more carbonic acid in solution, which makes them more sparkling in appearance. Sparkling waters are generally preferred by the public, although they commonly owe their briskness to extensive contact with decaying organic matter. It is thus that the highly-sparkling pump-waters of London are still preferred by many. On the other hand, soft waters are not necessarily vapid; no draught of water could be more delicious than that which is obtained from the public drinking-fountains of Glasgow, supplied from Loch Katrine, although I find that 100 volumes of this water contain only the following gaseous constituents:—

Nitrogen	1·731	vols.
Oxygen	·704	„
Carbonic Acid	·113	„
	2·548	

Action upon Lead.

The conditions which determine the action or non-action of water upon lead, have hitherto been involved in much obscurity. Messrs. Graham, Miller, and Hofmann, the Government Commission of 1851, on the supply of water to the metropolis, established the fact that the presence of dissolved oxygen and the absence of more than three volumes of carbonic acid in 100 volumes of water, are amongst the conditions necessary for the attack of lead.

The whole of the present water supply of the metropolis is perfectly protected from acting upon lead by the large quantity of carbonic acid which it contains. Still there are obviously other conditions involved in the problem; for all the samples of water from the Welsh and Cumberland districts contain, as shown in the above analytical results, dissolved oxygen, whilst not one of them possesses an amount of carbonic acid even remotely approaching that which is necessary to protect it, and yet some of these waters act violently upon lead, whilst others are entirely without action upon the metal. Having recently had occasion to observe that a sample of distilled water, which acted powerfully upon lead, completely lost this quality by momentary contact with animal charcoal, I found on further investigation, that a minute quantity of the chief constituent of bone-black, *viz.* phosphate of lime, completely protects water from action upon lead. I then carefully examined for phosphate of lime, the water of the River Kent (Upper Kent) which is eminently distinguished for its violent action upon lead, and the water of the River Vyrnwy, which, although nearly as soft as distilled water, has not the slightest action upon lead, even when placed in contact with a bright and freshly-cut surface of the metal for 24 hours. This examination established the fact that the water of the Vyrnwy contained an appreciable amount of phosphate of lime, whilst not the slightest trace of this substance could be detected in that of the Upper Kent. The waters from both the proposed districts have been carefully examined as regards their behaviour towards lead, and, as the result of this examination, it may be safely affirmed that no danger on this score need be apprehended from the introduction of water from either district into the metropolis. I here exhibit to you samples of lead in various waters exemplifying the points upon which I have just spoken.

Lastly, I place before you in these large cylinders, holding more than a gallon each, samples of the Welsh and Cumberland waters, side by side with a similar sample of Thames water, which fairly represents the condition of more than 3-5ths of this water as it has been delivered in London since the 21st of January last. But it may be asked, will these waters from Wales or Cumberland reach the metropolis in this colourless, transparent, and soft condition after passing through a conduit from 180 to 280 miles in length? Let me tell you what I conceive will be the effect of such a conduit upon the water. For the first two or three years a certain amount of lime from the surface of the cement in the conduit will dissolve in the water, communicating at first an amount of hardness probably not exceeding 5°; this effect will gradually subside, and after two or three years the water will be delivered in London in a slightly better condition than that in which it leaves the storage reservoirs—a slightly better condition because it will be somewhat better aërated than when it starts on its journey. At the present moment the water of Loch Katrine passes through a conduit 26 miles long, and I have lately carefully taken its hardness as it leaves the lake and as it is delivered to consumers in Glasgow. Its hardness on delivery in Glasgow is only 0·3°, exactly the same as in the lake, and its transit through 26 miles of conduit has therefore added no hardening constituent to the water. Now, if 26 miles of conduit fail to alter the hardness, I can only conclude that 180 or even 280 miles of conduit, if properly constructed, will also, after the lapse of a few years, be equally incompetent to produce any substantial increase in the hardness. At the time the above experiments were made, Loch Katrine water had flowed through the conduit for seven years.

These are the principal points I have to bring before you in connection with the proposed supply, and as a summary of the chemical investigation of the present water supply on the one hand, and of the samples furnished by the Welsh and Cumberland districts on the other, I may state the following conclusions to which these investigations have led me:—

1. The present water supply of the metropolis is largely contaminated with sewage. Both analysis and statistics concur in the statement that each glass of Thames water taken from the river by the companies, contains one tea-spoonful of sewage.

2. Although this sewage is generally to a great extent oxidized before the delivery of the water in London; yet there is no guarantee whatsoever that all its noxious qualities are removed, because these noxious qualities are, in all probability, contained in the mechanically suspended and least oxidizable portion of the sewage.

3. The river water supplied in London is often very imperfectly filtered; and thus even the visible suspended matters of sewage are not wholly excluded from the water supply. Only on one occasion during the whole year 1867, have I obtained a transparent sample of water from the Southwark Company's mains. The Grand Junction Company's water was turbid, four times out of twelve, the Chelsea thrice, the West Middlesex, Lambeth, and East London each twice, out of the twelve occasions when the samples were drawn for analysis. The New River Company alone delivered perfectly filtered water during the whole year.

4. The quality of the water supplied to London is greatly inferior to that of any other town in the United Kingdom, whose supply I have examined.

5. The distribution of water in the metropolis still continues, with but slight exceptions, on the intermittent system, a system which has been abolished in almost every town of importance in the United Kingdom.

6. The water which it is proposed to supply either from the Welsh or the Cumberland districts is of excellent quality. It is equal or superior to that supplied to any town in Great Britain.

7. The water from each of the proposed districts is extremely soft, pleasant to drink, and of good aëration.

8. These waters have never been contaminated with sewage, and are therefore above all suspicion.

9. They can be distributed in the present system of supply pipes without any danger of lead contamination.

The choice between the present and proposed supply rests virtually with the intelligent inhabitants of the metropolis. Will you go to a source of pure water uncontaminated with sewage, or will you continue the existing supply? I can anticipate your verdict, but you must not delay to record it. These splendid sources now available will not remain much longer within your reach.

In conclusion, I beg to quote the opinion of one of our highest medical authorities on the dangers of sewage-contaminated water. Unpleasant as the theme may be, this opinion is in the highest degree deserving the earnest attention of every individual who has progressed beyond the state of savagery. In his Report on the Cholera Visitation of 1866, Mr. Simon, the medical officer of the Privy Council, says:—"It cannot be too distinctly understood that the person who contracts cholera in this country is *ipso facto* demonstrated with almost absolute certainty to have been exposed to excremental pollution; that what gave him cholera was (mediately or immediately) cholera-contagium discharged from another's bowels; that, in short, the diffusion of cholera among us depends entirely upon the numberless filthy facilities which are let exist, and especially in our larger towns, for the fouling of earth and air and water, and thus secondarily for the infection of man, with whatever contagium may be contained in the miscellaneous outflowings of the population. Excrement-sodden earth, excrementreeking air, excrement-tainted water, these are for us the causes of cholera. That they respectively act only in so far as the excrement is cholera-excrement, and that cholera-excrement again only acts in so far as it contains certain microscopical fungi, may be the truest of all true propositions; but whatever be their abstract truth, their separate application is impossible. Nowhere out of Laputa could there be serious thought of differentiating excremental performances into groups of diarrhœal and healthy, or of using the highest powers of the microscope to identify the cylindro-tænium for extermination. It is excrement, indiscriminately, which must be kept from fouling us with its decay.

"And thus it is that my practical advice remains substantially what it has been for years. The local conditions of safety are, above all, these two:—(1) that, by appropriate structural works, all the excremental produce of the population shall be so promptly and so thoroughly removed, that the inhabited place, in its air and soil, shall be absolutely without fœcal impurities; and (2) that the water supply of the population shall be derived from such sources, and conveyed in such channels, that its contamination by excrement is impossible.

"What good results are got even by rough approximation to those sanitary standards has already been abundantly shown here. The way in which the southern districts of London, with their three-fourths of a million of population, have gradually gained *comparative* immunity from cholera *in proportion* as their two water companies have ceased to distribute sewage-tainted water among them, is a matter of familiar history.

"That cholera is still a terror to Europe shows how scantily such illustrations are yet understood. Even here in England the objects which I have named as essential are at best but rarely fulfilled; indeed for vast numbers of our population scarcely rudimentary endeavours have been made to attain them. Town after town might be named, with myriad on myriad of population, where there is little more structural arrangement for the removal of refuse than if the inhabitants were but tented there for a night. The case of the water supply is no better: my reports are incessantly showing the too frequent foulness of private supplies; while, as regards public water supplies, *such as generally are in the hands of commercial companies*, it has again and again been shown (and seldom more pointedly than in the present volume), that their conveniences and advantages are countervailed by dangers to life on a scale of gigantic magnitude, unless those who administer the supplies act under a very deep sense of responsibility.

"Cholera, ravaging here at long intervals, is not Nature's only retribution for our neglect in such matters as are in question. Typhoid fever and much endemic diarrhœa are, as I have often reported, incessant witnesses to the same deleterious influence; typhoid fever which annually kills some 15,000 to 20,000 of our population, and diarrhœa which kills many thousands besides. The mere quantity of this wasted life is something horrible to contemplate, and the mode in which the waste is caused is surely nothing less than shameful. It is to be hoped that, as the education of the country advances, this sort of thing will come to an end; that so much preventable death will not always be accepted as a fate; that for a population to be thus poisoned by its own excrement, will some day be deemed ignominious and intolerable."

[E. F.]

GENERAL MONTHLY MEETING,

Monday, April 6, 1868.

Colonel Philip James Yorke, F.R.S. Vice-President, in the Chair.

Richard Melville Beachcroft, Esq.
Matthew Boulton, Esq.
Mrs. Cattley
Roger Eykyn, Esq. M.P.
William Millar, Esq.
Charles Henry Mills, Esq.
Donald Nicoll, Esq.
Charles Pemberton, Esq.
Archibald Gilchrist Potter, Esq.
Sir George R. Prescott, Bart.
Sir Henry Thompson, F.R.C.S.

were *elected* Members of the Royal Institution.

Peter Henry Berthon, Esq.

was *admitted* a Member of the Royal Institution.

The special thanks of the Members were returned for the following additions to "the Donation Fund for the Promotion of Experimental Researches":—

T. Williams Helps, Esq. (3rd Donation)	£10.

The **Presents** received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

British Museum Trustees—Catalogue of Fishes. Vol. VII. 8vo. 1868.
List of Specimens of Birds. Part 3. 12mo. 1868.
Guide to the Blacas Collection of Antiquities. 12mo. 1868.
Astronomical Society, Royal—Monthly Notices, Vol. XXVIII. No. 5. 8vo. 1868.
Memoirs. Vol. XXXV.-XXXVI. 4to. 1867.
Barry, E. M. Esq.—The Architect of the New Palace of Westminster. By Alfred Barry, D.D. (K 95) 8vo. 1868.
Cambridge Philosophical Society—Transactions, Vol. X. Part 2. Vol. XI. Part 1. 4to. 1864–6.
Chemical Society—Journal for March, 1868. 8vo.
Cooke, Rev. T. F. M.A. (*the Author*)—Authorship of the Practical Electric Telegraph of Great Britain. 8vo. 1868.
Cunningham, Alexander W. Esq. (*the Author*)—Notes on the History, Methods, and Technological Importance of Descriptive Geometry. (K 95) 8vo. 1868.

Editors—American Journal of Science, March, 1868. 8vo.
Artizan for March, 1868. 8to.
Athenæum for March, 1868. 4to.
British Journal of Photography for March, 1868. 4to.
Chemical News for March, 1868. 4to.
Engineer for March, 1868. fol.
Geological and Natural Repository. March, 1868. 8vo.
Horological Journal for March, 1868. 8vo.
Journal of Gas-Lighting for March, 1868. 4to.
Mechanics' Magazine for March, 1868. 8vo.
Pharmaceutical Journal for March, 1868.
Photographic News for March, 1868. 4to.
Practical Mechanics' Journal for March, 1868. 4to.
Revue des Cours Scientifiques et Littéraires. March, 1868.
Horticultural Society, Royal—Proceedings, No. 10. 8vo. 1868.
Journal, No. 5. 8vo. 1868.
Jervis, W. P. Esq. (*the Author*)—The Mineral Resources of Central Italy. 8vo. 1868.
Linnean Society—Journal, Nos. 43, 44. 8vo. 1868.
Meteorological Society—Proceedings, No. 35. 8vo. 1868.
President's Address. 8vo. 1868.
Photographic Society—Journal, No. 191. 8vo. 1868.
Royal Society of London—Proceedings, No. 99. 8vo. 1867.
Sidney, Rev. Edwin, M.A.—[Peter Lombard] Textus Sententiarum cum Conclusionibus Henrici Gorichem. Basel, 1510. 4to.
Statistical Society of London—Journal, Vol. XXXI. Part 1. 8vo. 1868.
Strachan, R. (*the Author*)—Principles of Weather Forecasts. (K 95) 8vo. 1868.
Sykes, Colonel, M.P. F.R.S. (*the Author*)—Storm Warnings. (K 95) 8vo. 1867.
Symons, G. J. Esq. (*the Author*)—Symons' Monthly Meteorological Magazine, March, 1868. 8vo.
British Rainfall, 1867. 8vo. 1868.
Tyndall, John, Esq. LL.D. F.R.S. (*the Author*)—Faraday as a Discoverer. 12mo. 1868.
White, William, Esq. (*the Author*)—Emanuel Swedenborg: his Life and Writings. 8vo. 1867.
Williams and Norgate, Messrs.—Sketch of a Philosophy: Part 2. Matter and Molecular Morphology. 8vo. 1868.
United Service Institution, Royal—Journal, No. 47. 8vo. 1868.
Yorkshire (*West Riding*) *Geological and Polytechnic Society*—Report of Proceedings, 1867. 8vo. 1868.

WEEKLY EVENING MEETING,

Friday, April 24, 1868.

Sir Henry Holland, Bart. M.D. D.C.L. F.R.S. President, in the Chair.

J. H. Gladstone, Ph.D. F.R.S.

On some New Experiments on Light.

The speaker commenced by referring to the fact that we are constantly making new experiments or observations on light: in fact, all seeing is but a comparison of different degrees of light and shade, and the contrast of colours. Most of the rays that meet our eyes from surrounding objects are reflected rays, but some of the commonest things, such as the water-bottles and tumblers of cut-glass on our dining tables, exhibit beautifully the bending, the magnifying, the diminishing, and the production of coloured fringes, due to refraction. The purpose of this discourse was to rise from the simplest phenomena of this kind to a consideration of Refraction-equivalents, and to describe the state of our present knowledge in regard to them.

By means of the electric lamp it was shown that a piece of glass, or other transparent body, will throw a perfectly black shadow if the two surfaces through which the ray passes be not parallel; that the light is then bent on one side, and at the same time spread out into its component colours; that this bending (refraction) varies with the amount of inclination of the two surfaces to one another, but in such a way that the sine of the angle of refraction bears a constant ratio to the sine of the angle of incidence; that this constant number, termed the index of refraction, or μ, belongs only to the one substance, each solid, liquid, or gas having its own index; that there is no necessary connection between the amount of refraction and the length of the spectrum (dispersion) caused by different substances, whether gaseous, liquid, or solid—for instance, a solution of an iodide always disperses more than a solution of the chloride of the same metal, even though it be diluted to the same amount of refraction.

This index of refraction is affected by change of temperature. In liquids, and probably in all gases, the bending decreases as the thermometer rises; in solids, on the contrary, as lately shown by Fizeau, the change is in the opposite direction, crown glass always remaining the same, and fluor spar being the only case where he observed a diminution. This was experimentally demonstrated in regard to liquids. Thus a yellow sodium ray, which had passed through a hollow prism filled with oil of nutmeg, and thence through another filled with bisulphide of carbon, moved some inches along the screen, when the nutmeg oil was warmed a few degrees by stirring it with heated iron wire. This index of refraction is still more materially affected when a body passes from the solid to the liquid, or from the liquid to the gaseous condition; a fact that was illustrated by the visibility of the water melted in crystalline spaces in the middle of a block of ice.

The index of refraction of a mixture is moreover not always the mean of the indices of its constituents. Thus a ray passed successively through two hollow prisms filled with equal quantities of alcohol and water respectively, fell on the screen in a certain position; but when the two liquids were mixed together, and divided between the two prisms, the ray was visibly refracted to a greater distance.

These changes depend on the alterations of volume which the substances undergo; and the speaker, in conjunction with the Rev. T. Pelham Dale, had observed in liquids that the index of refraction, minus unity, divided by the density (in symbolic language $\frac{\mu-1}{d}$) is constant for all temperatures, and for all mixtures, or rather that the coincidence is very close but not quite perfect on account of some other law not yet understood. This conclusion has been abundantly verified by Landolt of Bonn, Ketteler, and Wüllner, and the former experimenter has founded upon it a method of analyzing mixtures of liquids.

This unchangeable number was termed the "specific refractive energy" of the substance, and it seemed to hold good notwithstanding a change from the solid to the liquid or the gaseous condition. It was early observed that the specific refractive energy of a compound bore a close resemblance to the mean of the specific refractive energies of its components. Landolt, by multiplying this number by the chemical equivalent, facilitated the calculation greatly. He termed this new number the "refraction-equivalent," $P\frac{\mu-1}{d}$, and proofs have rapidly accumulated that the number is little affected, not only by temperature, change of aggregate condition, mixture, or solution, but even by strong chemical combination.

Thus diamond, which is crystallized carbon, has the refraction-equivalent 5·0; sulphur has 16·0. Bisulphide of carbon, $C\,S_2$, which is nearly the most refractive liquid known, should therefore be represented by $5 + 2 \times 16$, that is 37·0. The experimental number is 37·3. But the diamond will burn in oxygen, and is thus converted into carbonic anhydride, while it is possible to reduce this gas into another containing only half the amount of oxygen, namely, carbonic oxide. The refraction-equivalents of these gases, as deduced from Dulong's observations, are respectively 10·03 and 7·53; but the difference between $C\,O_2$ and $C\,O$ is one equivalent of oxygen, and the difference between the above numbers is 2·5. This then may be taken as the refraction-equivalent of oxygen, and subtracting it from $C\,O = 7·53$ we have remaining $C = 5·03$, practically the same number as that obtained directly from crystallized carbon. Similarly, but generally by more indirect methods, it has been determined that this element, whether pure as diamond or combined with other elements to form gases as the above-mentioned, coal-gas, or cyanogen, or liquids as chloride of carbon, benzole, oil of turpentine, alcohol, or ether, or solids as paraffin, sugar, or camphor, is still exerting the same influence on the rays of light that set its particles in motion, an influence that we can express by the number 5·0. Again to revert to sulphur, the two salts sulpho-cyanide and cyanide of potassium—K S Cy and K Cy—differ by one equivalent of this element, and their refraction-equivalents as determined from their aqueous solutions are respectively 33·4 and 17·1, numbers differing by 16·3, a number almost identical with that reckoned from molten sulphur. In this way the refraction-equivalents of a large number of the elements have been determined; and the following table comprises what seem the most probable numbers among those that have been hitherto published by Landolt, Haagen, and Schrauf, as well as the speaker :—

	Atomic weight.	Refraction-equivalent.
Hydrogen	1·0	1·3
Chlorine	35·5	9·8
Bromine	80·0	15·7
Iodine	127·0	24·4
Oxygen	16·0	3·0
Sulphur	32·0	16·0
Carbon	12·0	5·0
Silicium	28·0	6·2
Nitrogen	14·0	4·1
Phosphorus	31·0	18·5
Arsenic	75·0	16·0
Antimony	122·0	25·7
Vanadium	51·4	25·4
Sodium	23·0	4·9
Tin	118·0	19·2
Copper	63·4	11·2
Mercury	200·0	21·6

The above numbers are reckoned for the red ray. Most of them can as yet claim to be considered only as approximative; and it seems certain that some elements, as oxygen and sulphur, have more than one refraction-equivalent.

Vanadium, though included in the above table, has only just been determined, and that from the oxy-trichloride which Professor Roscoe exhibited a few weeks before. It is interesting, as it supports his theory of the close analogy of phosphorus and vanadium, for these two bodies, with sulphur, exceed all others in refraction and especially in dispersion.

The speaker stated that he was now engaged in examining the effect of salts in solution on the rays of light, and that he hoped to determine in this way the refraction-equivalents not only of a multitude of salts, but of the metallic elements themselves.

But the question may be asked, "If a substance has a refraction compounded of the refraction of its constituents, how can bodies such as Iceland spar have two refractive indices?" Now these are crystalline bodies, or if uncrystallized they have become doubly refracting by being unequally heated or compressed. In either case we may suppose a different amount of tension in different directions; and the fact of the two rays being oppositely polarized points to some such difference of molecular arrangement. It is easy to understand that the change of tension or internal structure may act in the same way as a change of density in modifying the velocity of transmitted light, and therefore the amount of its refraction. But if we take the crystal to pieces by dissolving it, there can then no longer be unequal tension or unsymmetrical arrangement of particles, and it must have one refraction-equivalent. And this is always the case. The numbers deduced from Brewster's observations of the two rays of crystallized nitre are 16·3 and 25·0, while the equivalent of nitre dissolved in water is the intermediate number 21·8.

[J. H. G.]

ANNUAL MEETING,

Friday, May 1, 1868.

Sir Henry Holland, Bart. M.D. D.C.L. F.R.S. President, in the Chair.

The Annual Report of the Committee of Visitors for the year 1867 was read and adopted.

The Books and Pamphlets presented in 1867 amounted to 131 volumes, making, with those purchased by the Managers, a total of 319 volumes added to the Library in the year, exclusive of periodicals.

Forty new Members were elected in 1867.

Sixty-three Lectures and Twenty Evening Discourses were delivered during the year 1867.

Thanks were voted to the President, Treasurer, and Secretary, to the Committees of Managers and Visitors, and to the Professors, for their services to the Institution during the past year.

The following Gentlemen were unanimously elected as Officers for the ensuing year:—

President—Sir Henry Holland, Bart. M.D. D.C.L. F.R.S.
Treasurer—William Spottiswoode, Esq. M.A. F.R.S.
Secretary—Henry Bence Jones, M.A. M.D. F.R.S.

Managers.

Henry Wollaston Blake, Esq. M.A. F.R.S.
Charles Brooke, Esq. M.A. F.R.S.
Adm. Sir Henry John Codrington, K.C.B.
Captain Douglas Galton, C.B. F.R.S.
John Peter Gassiot, Esq. F.R.S.
William Robert Grove, Esq. M.A. Q.C. F.R.S.
Cæsar H. Hawkins, Esq. F.R.S.
Sir John Lubbock, Bart. F.R.S. Pres. Entom. Soc.
Sir Roderick I. Murchison, Bart. K.C.B. D.C.L. F.R.S.
William Pole, Esq. M.A. F.R.S.
William Frederick Pollock, Esq. M.A.
Robert P. Roupell, Esq. M.A. Q.C.
Lieut.-Gen. Edward Sabine, R.A. D.C.L. Pres. R. S.
Sir Charles Wheatstone, D.C.L. F.R.S.
Colonel Philip James Yorke, F.R.S.

Visitors.

Andrew Whyte Barclay, M.D.
Charles Beevor, Esq. F.R.C.S.
John Ashton Bostock, Esq.
John Charles Burgoyne, Esq.
Rev. Charles Fynes Clinton, M.A. F.R.G.S.
Alfred Davis, Esq.
William Dell, Esq.
Rev. G. Godwin Pownall Glossop, A.M.
Alfred Gutteres Henriques, Esq.
Edward Henry Moscrop, Esq.
William Newmarch, Esq. F.R.S.
Arthur Giles Puller, Esq. M.A. F.S.A.
Samuel Scott, Esq.
Edward Owen Tudor, Esq. F.S.A.
Robert Ballard Woodd, Esq. F.S.A. F.R.BS.

WEEKLY EVENING MEETING,

Friday, May 1, 1868.

Sir Henry Holland, Bart. M.D. D.C.L. F.R.S. President, in the Chair.

Francis T. Palgrave, Esq.

LATE FELLOW OF EXETER COLLEGE, OXFORD.

How to form a Good Taste in Art.

During the last hundred years the Fine Arts have been successfully practised in England, and now hold a great and increasing place in our interests. Good taste in judging art is therefore a matter of some value. The popular view is, however, that Taste is not subject to rules. The proposition now advocated is, on the contrary, that Taste is, like all other matters of human knowledge, an educated natural bias; that practically Taste is knowledge. What such knowledge leads to is, not identical judgments, but judgments founded on rational rules; not uniformity, but unity of taste.

Three main practical applications of such knowledge considered, corresponding to the main elements which form the interest of works of art:—

I. Knowledge of natural fact.

II. Knowledge of the material conditions of each art, and of the chief phases of thought or emotion or sentiment which are expressed by art.

These branches of knowledge are, at the same time, those which the artist himself should possess. Reasons, however, why the artist is not generally the best judge of air.

III. Knowledge of the historical conditions under which works of art have been produced.

Results of these forms of knowledge in forming a correct and rational taste. Such a taste will greatly increase our sense of that which is the object of all the Fine Arts—high and lasting pleasure. It will also be a large and comprehensive taste, free from fastidiousness and petty egotistic judgments. The more accurately we enjoy, the more deeply we enjoy. We love more things, the more wisely we love.

[F. T. P.]

GENERAL MONTHLY MEETING,

Monday, May 4, 1868.

Sir Henry Holland, Bart. M.D. D.C.L. F.R.S. President, in the Chair.

The following Vice-Presidents were nominated for the ensuing year:—

William Robert Grove, Esq. F.Q.C. F.R.S.
Lieut.-Gen. E. Sabine, R.A. Pres. R.S.
Sir Roderick I. Murchison, Bart. K.C.B. F.R.S. and
William Spottiswoode, Esq. F.R.S. the Treasurer.

William Anderson, Esq. LL.D.
Captain N. D. C. F. Douglas,
Frederick Green, Esq.
Swann Hurrell, Esq.
John Edward Taylor, Esq.

were *elected* Members of the Royal Institution.

William Millar, Esq.

was *admitted* a Member of the Royal Institution.

The following Professors were re-elected:—

John Tyndall, Esq. LL.D. F.R.S. as Professor of Natural Philosophy.
Edward Frankland, Esq., Ph.D. F.R.S. as Professor of Chemistry.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

Académie Impériale de Médecine, Paris—Bulletins, tome 33, No. 6. 8vo. 1868.
Actuaries, Institute of—Journal, No. 71. 8vo. 1868.
Astronomical Society, Royal—Monthly Notices, Vol. XXVIII. No. 6. 8vo. 1868.
Belgique, Académie Royale de—Bulletins, tome 24. 8vo. 1867.
British Architects, Royal Institute of—Sessional Papers. 4to. 1867–8.
Chemical Society—Journal for April, 1868. 8vo.
East India Association—Journal, Vol. II. No. 1. 8vo. 1868.

Editors—Artizan for April, 1868. 4to.
Athenæum for April, 1868. 4to.
British Journal of Photography for April, 1868. 4to.
Chemical News for April, 1868. 4to.
Engineer for April, 1868. fol.
Geological and Natural History Repository. April, 1868. 8vo.
Horological Journal for April, 1868. 8vo.
Journal of Gas-Lighting for April, 1868. 4to.
Mechanic's Magazine for April, 1868. 8vo.
Pharmaceutical Journal for April, 1868.
Photographic News for April, 1868. 4to.
Practical Mechanic's Journal for April, 1868. 4to.
Revue des Cours Scientifiques et Littéraires. April, 1868. 4to.
Franklin Institute—Journal, No. 506. 8vo. 1868.
Linnean Society—Journal, No. 40. 8vo. 1868.
Lubbock, Sir John, Bart. F.R.S. M.R.I. (the Author)—On the Pauropus; and the Thysanura (Trans. Linn. Soc.) 4to. 1868.
Macilwain, George, Esq. F.R.C.S. M.R.I. (the Author)—Surgical Commentaries. 16to. 1868.
Mechanical Engineers' Institution—Proceedings, June, 1867. Part II. 8vo.
Medico-Chirurgical Society, Royal—Proceedings, Vol. VI. No. 1. 8vo. 1868.
Meteorological Society—Proceedings, No. 36. 8vo. 1868.
Philosophical Transactions, Vol. CLVII. Part II. 4to. 1867.
Photographic Society—Journal, No. 192. 8vo. 1868.
Pictet, Captain F. (the Author)—Railways of England, Scotland, and Ireland; a comprehensive Scheme for the Redemption of Capital, &c. (K 95) 8vo. 1868.
Royal Society of London—Proceedings, No. 99. 8vo. 1868.
Catalogue of Scientific Papers (1800-1863). Vol. I. 4to. 1867.
Smee, A. H. jun. Esq. (the Author)—Causes of Death, tabulated (M 8) 4to. 1868.
Society of Arts—Journal for April, 1868. 8vo.
Symons, G. J. (the Author)—Symons' Monthly Meteorological Magazine, April, 1868. 8vo.

WEEKLY EVENING MEETING,

Friday, May 8, 1868.

Sir Henry Holland, Bart. M.D. D.C.L. F.R.S. President, in the Chair.

C. Greville Williams, Esq. F.R.S.

On the Artificial Formation of Organic Substances.

Chemical researches are liable at various epochs to take special directions. Before 1830 organic chemistry was comparatively little studied. The simplification of the methods of organic analysis by Liebig took place at a most opportune moment, and gave an extraordinary impetus to the study of carbon compounds. So great was this influence that proximate and ultimate analysis made a progress the rapidity of which was unexampled in the history of science.

But chemists soon became dissatisfied with merely determining the composition of substances, and they very soon began eagerly to study their products of decomposition, and in this manner get a clue to the way in which nature had put them together.

The successful attack on this problem led to a much grander one suggesting itself. This was to utilize the insight analysis had given them into the constitution of substances, and to endeavour to build them up without the assistance of life. The speaker showed that we thus arrive at the two great engines of chemical research, *analysis* and *synthesis*.

He then proceeded to define and illustrate experimentally these terms.

In organic chemistry the information supplied by the analysis of a substance often renders its synthesis easy. Water was decomposed by a battery, and its properties and quantitative relations shown. The mixed gases were then introduced into a soap-bubble, so prepared as to last a considerable time. It was by a simple contrivance attached to a thread, and the lightness of the enclosed gases was shown by the fact that the bubble was able to raise the thread and a disc of paper into the air. The energy with which the mixed gases combine to form water was then shown by applying a light to the bubble, when it burst with a loud report. The *quantitative* synthesis of water was experimentally shown by passing hydrogen over cupric oxide in an apparatus which allowed of the collection of the water. It was then shown that in organic chemistry the molecules are generally too complex to be put together so easily; and this statement was proved by reference to the constitution of methylamine, the simplest of the organic alkaloids.

The speaker then went somewhat fully into the question of the propriety of the use of the terms "organic" and "inorganic." He showed also that all the attempts hitherto made at separating chemistry into two distinct branches had failed. Liebig's definition of organic chemistry as the "chemistry of compound radicles" being obviously inadequate, inasmuch as some compound radicles (such as sulphuryl and phosphoryl) are certainly inorganic.

Laurent's definition, "chemistry of carbon," is equally insufficient, inasmuch as carbonic anhydride and carbonic tetrachloride are as clearly inorganic as sulphuric anhydride or sodic chloride. He then proceeded to argue that chemistry was "one and indivisible," and stated that one of the chief aims of his discourse was to prove that assertion.

It was shown that until within the last few years all the specific attempts made to break the apparently natural barriers between organic and inorganic chemistry had proved failures.

It was true that in the course of the innumerable researches and experiments made by chemists, one or two of the simple organic bodies had presented themselves; but, like urea and cyanogen, they were substances which, as it were, hovered on the confines of inorganic chemistry, and would have been called inorganic had they not contained carbon.

The grand problem, which consisted in taking the elements themselves, and building them up *gradatim* into the proximate principles existing in the tissues of plants and animals, until lately appeared almost hopeless. This apparent difficulty was shown to arise from the mistake of supposing the proximate principles of animals and vegetables to result from an occult power vaguely termed the "vital force." It was at one time supposed that the laws which regulate combination were either suspended or modified in the tissues of living creatures, but the speaker urged that whenever the proper reagents were made to act upon each other under the proper conditions, the same substances were produced which at one time were supposed to require the aid of vitality for their formation.

The problem of the "synthesis," or building up of the so-called organic substances, was then shown to present itself (in the present state of chemistry) under two aspects:—1st. Where they are prepared by the aid of reagents, which have themselves been produced directly or indirectly from animals or vegetables. 2nd. Where the synthesis was effected from the free elements themselves, from hydrogen and pure carbon.

The speaker then proceeded to enumerate some of the principal instances where substances originally derived from animals or vegetables had been formed synthetically. Wöhler's synthesis of urea was shown to be one of the earliest in point of date, and his method was described, and also Kolbe's new process by the mere heating of ammonic carbonate to a point just below that at which urea is decomposed.

One of the next most important steps in the history of synthesis was shown to be the conversion of carbonic disulphide into carbonic tetrachloride or perchlorinated marsh gas. Inasmuch as carbonic disulphide is a purely inorganic body, it is evident that any substance which can be formed from it is a case of true synthesis.

The following equations represent the steps by which acetic acid may be produced from carbonic disulphide:—

$$\underbrace{CS_2}_{\substack{\text{Carbonic}\\\text{disulphide.}}} + 6Cl_2 = \underbrace{CCl_4}_{\substack{\text{Carbonic}\\\text{tetrachloride.}}} + \underbrace{2(SCl_4)}_{\substack{\text{Sulphuric}\\\text{tetrachloride.}}}$$

$$\underbrace{2(CCl_4)}_{\substack{\text{Carbonic}\\\text{tetrachloride.}}} = \underbrace{C_2Cl_4}_{\substack{\text{Carbonic}\\\text{dichloride.}}} + 2Cl_2$$

$$\underbrace{C_2Cl_4}_{\substack{\text{Carbonic}\\\text{dichloride.}}} + Cl_2 = \underbrace{C_2Cl_6}_{\substack{\text{Carbonic}\\\text{trichloride.}}}$$

$$\underbrace{C_2Cl_6}_{\substack{\text{Carbonic}\\\text{trichloride.}}} + 2(H_2O) = \underbrace{C_2HCl_3O_2}_{\substack{\text{Trichloracetic}\\\text{acid.}}} + 3(HCl)$$

$$\underbrace{C_2HCl_3O_2}_{\substack{\text{Trichloracetic}\\\text{acid.}}} + 3H_2 = \underbrace{C_2H_4O_2}_{\substack{\text{Acetic}\\\text{acid.}}} + 3HCl$$

This important series of reactions, then, result in the production of acetic acid, one of the most marked of the so-called organic acids, from purely inorganic materials.

The synthesis of oxalic acid by the direct union of carbonic anhydride with sodium, as recently accomplished by Dr. Drechsel, was next described, and it was shown that as oxalic acid, by mere distillation, yields formic acid, the synthesis of the first acid leads directly to a new synthesis of the second.

The other modes of effecting the synthesis of formic acid were then pointed out, *viz.*:—Berthelot's process, which consists in heating potassic hydrate in an atmosphere of carbonic oxide; and Kolbe and Schmidt's method, by exposing potassium to a warm moist atmosphere of carbonic anhydride.

The speaker, in the course of his remarks on the constitution of formic acid, showed that the quantity of oxygen in it was so large that it only required one atom more to convert it into carbonic acid and water. Its easy oxidation was illustrated by letting it fall on plumbic dioxide in an apparatus which caused the evolved gas to pass into a solution of baric hydrate, the result being a copious precipitation of baric carbonate.

Having shown that acetic acid can be formed from carbonic disulphide and the chlorides of carbon, and oxalic and formic acids from the oxides of carbon, the speaker proceeded to indicate the modes in which complex bodies, hitherto obtained from animal and vegetable sources, can be built up from elemental carbon and hydrogen.

If carbon can only be made to combine directly with hydrogen, no matter how simple the resulting compound may be, it becomes possible to effect the synthesis of a vast number of the most characteristic substances found in animals and vegetables.

This brilliant result has been accomplished through the agency of acetylene, a most remarkable hydrocarbon which was first noticed by Edmund Davy as long ago as 1836.

There are two methods by which acetylene can be formed from inorganic materials—one devised by Berthelot, and the other by the speaker. The first consists in passing a stream of hydrogen through a globe in which the voltaic arc (from 70 or 80 cells of a Grove's battery) is produced between carbon points. At this tremendous temperature the carbon unites directly with the hydrogen. The experiment was made, and the production of acetylene shown, by the formation of a precipitate in a solution of ammoniacal cuprous chloride.

The speaker then showed, experimentally, that much larger quantities of acetylene can be formed by the decomposition by the induction spark of carbonic tetrachloride in presence of hydrogen, in accordance with the equation :—

$$2(CCl_4) + 5H_2 = C_2H_2 + 8(HCl).$$

The experiment succeeded perfectly, and a large quantity of the cuprous acetylide was rapidly produced.

But the most simple and ready means of preparing acetylene was shown to be by drawing air through the flame of a common glass spirit-lamp, by means of an aspirator. So readily is the cuprous precipitate obtained by this means, that it suffices to draw a few cubic centimetres through the solution of ammoniacal cuprous chloride to obtain evidence of the presence of acetylene in the flame. The experiment was then made, and in a few seconds the solution became thick with the suspended precipitate.

The speaker had ascertained that all the homologues of olefiant gas give acetylene in abundance when subjected to the induction spark. Amylene does it readily in accordance with the annexed equation:—

$$2\underbrace{(C_5H_{10})}_{\text{Amylene.}} = 5\underbrace{(C_2H_2)}_{\text{Acetylene.}} + 5H_2.$$

That the spark acted only in consequence of its high temperature, the speaker said, is rendered probable by the fact that if hydrogen be passed through carbonic tetrachloride, and then into a globe containing a platinum spiral, when the latter was heated to dull redness by three cells of a Grove's battery, no acetylene was produced; but when five cells were used, and the spiral became white hot, the cuprous precipitate was obtained readily. The same result was stated to occur with amylene.

Simple as the formula of acetylene is, almost all the animal and vegetable substances which have been formed by pure synthesis may be obtained from it.

The following equations represent the steps by which alcohol may be "synthesised." It is proper to premise that the conversion of acetylene into olefiant gas is accomplished by treating the cuprous acetylide with zinc and ammonia, so as to obtain nascent hydrogen:—

$$\underbrace{C_2H_2}_{\text{Acetylene.}} + H_2 = \underbrace{C_2H_4}_{\text{Ethylene.}}$$

$$\underbrace{C_2H_4}_{\text{Ethylene.}} + H_2SO_4 = \underbrace{H,\ C_2H_5SO_4}_{\substack{\text{Ethyl-sulphuric} \\ \text{acid.}}}$$

$$\underbrace{H,\ C_2H_5SO_4}_{\substack{\text{Ethyl-sulphuric} \\ \text{acid.}}} + H_2O = \underbrace{C_2H_6O}_{\text{Alcohol.}} + H_2SO_4$$

The synthesis of succinic acid from acetylene was next shown in accordance with the annexed equations, omitting the synthesis of ethylene, which has been already given:—

$$\underbrace{C_2H_4}_{\text{Ethylene.}} + Br_2 = \underbrace{C_2H_4Br_2}_{\substack{\text{Ethylenic} \\ \text{dibromide.}}}$$

$$\underbrace{C_2H_4Br_2}_{\substack{\text{Ethylenic}\\ \text{dibromide.}}} + \underbrace{2(KCN)}_{\substack{\text{Potassic}\\ \text{cyanide.}}} = \underbrace{C_2H_4(CN)_2}_{\substack{\text{Ethylenic}\\ \text{dicyanide.}}} + 2(KBr)$$

$$\underbrace{C_2H_4(CN)_2}_{\substack{\text{Ethylenic}\\ \text{dicyanide.}}} + 2(KHO) + 2(H_2O) = \underbrace{C_4H_4K_2O_4}_{\substack{\text{Potassic}\\ \text{succinate.}}} + 2(NH_3)$$

This mode of effecting the synthesis of succinic acid is due to the researches of Maxwell Simpson.

The beautiful appearance of succinic acid under the influence of polarized light was shown by the aid of the electric lamp. The specimen used had been artificially prepared.

The speaker then proceeded to show that the synthesis of succinic acid was a direct step to that of tartaric acid. This latter reaction is due to the researches of Perkin and Duppa.

The artificial formation of succinic acid, starting with acetylene, having been proved, it is only necessary to start with that acid to prove the synthesis of tartaric acid from acetylene :—

$$\underbrace{C_4H_6O_4}_{\substack{\text{Succinic}\\ \text{acid.}}} + 2Br_2 = \underbrace{C_4H_4Br_2O_4}_{\substack{\text{Dibromosuccinic}\\ \text{acid.}}} + 2HBr$$

$$\underbrace{C_4H_2Ag_2Br_2O_4}_{\substack{\text{Argenetic}\\ \text{dibromosuccinate.}}} + 2(H_2O) = \underbrace{C_4H_6O_6}_{\substack{\text{Tartaric}\\ \text{acid.}}} + 2(AgBr)$$

The speaker next proceeded to show how the synthesis of the organic alkaloids could be effected from inorganic materials.

In the first place the fact that cyanides can be produced by heating carbon in presence of nitrogen and an alkali, is well known. The next step is to procure prussic acid by distilling cyanides with acids. From pure prussic acid methylamine, the simplest of the primary monamines, can easily be obtained, either by the aid of nascent hydrogen, or free hydrogen in the presence of spongy platinum.

$$\underbrace{CHN}_{\text{Prussic acid.}} + 2H_2 = \underbrace{CH_5\,N}_{\text{Methylamine.}}$$

This equation has been realized by both the methods given above, the first by Mendius, the second by Debus.

It is also evident that as alcohol can be obtained from acetylene, that all the ethylic, primary, secondary, and tertiary monamines of Hofmann can now be synthetically formed. The steps being, (1) conversion of acetylene into olefiant gas; (2) passage of olefiant gas into alcohol; (3) alcohol into iodide of ethyl; (4) action of iodide of ethyl on ammonia.

Again, acetic acid, it has been shown, can be prepared from carbonic disulphide. Now acetic acid, by the action of a red heat, can be made to yield a number of the homologues of olefiant gas. The latter by treatment with excessively strong hydriodic acid, become converted into the iodides of the alcohol radicals (Berthelot). By following up this last reaction with pentylene, heptylene, octylene, and nonylene, the speaker succeeded in obtaining pentylamine, heptylamine, octylamine, and nonylamine.

The direct ascent from acetylene to the coal tar colours was then shown according to the following equations :—

$$\underbrace{3(C_2H_2)}_{\text{Acetylene.}} = \underbrace{C_6H_6}_{\text{Benzol.}}$$

$$\underbrace{C_6H_6}_{\text{Benzol.}} + HNO_3 = \underbrace{C_6H_5NO_2}_{\text{Nitrobenzol.}} + H_2O$$

$$\underbrace{C_6H_5NO_2}_{\text{Nitrobenzol.}} + 6(FeO) + H_2O = \underbrace{C_6H_7N}_{\text{Aniline.}} + 3(Fe_2O_3)$$

$$\underbrace{C_6H_5Br}_{\text{Bromobenzol}} + \underbrace{CH_3Br}_{\substack{\text{Methylic}\\ \text{Bromide.}}} + Na_2 = \underbrace{C_7H_8}_{\text{Toluol.}} + 2NaBr$$

$$\underbrace{C_6H_7N}_{\text{Aniline.}} + \underbrace{2(C_7H_9N)}_{\text{Toluidine.}} = \underbrace{C_{20}H_{19}N_3}_{\text{Rosaniline.}} + 3H_2$$

These transformations were all described at length. In effect acetylene passed through a red-hot tube becomes polymerized into benzol.

The passage of toluol into nitro-toluol and toluidine is omitted in the above equations, because the reactions are identical in kind with those of benzol into aniline.

In describing benzol, experiments were shown illustrating the density of its vapour as compared with air. In one of these, benzol was poured into a large beaker containing a hot iron; at first the benzol assumed the spheroidal form, but, as the temperature fell, it became converted into vapour, which filled the beaker. A glass syphon was then introduced into the beaker, and the vapour drawn off as if it had been a liquid, and inflamed. The vapour descending through the syphon was then received into a warm beaker, from which it was decanted into another beaker in which it was inflamed.

The speaker then proceeded to show the way in which the synthesis of zinc ethyl could be effected; it is, however, unnecessary to follow the equations in detail, because, having already explained the manner in which alcohol can be synthesized from acetylene, it is obvious that zinc ethyl can be directly derived from that fluid.

Wanklyn's interesting synthesis of acetic acid from sodium methyl was then shown to take place in accordance with the expression:

$$\underbrace{CH_3Na}_{\text{Sodium methyl.}} + CO_2 = \underbrace{C_2H_3NaO_2}_{\text{Sodic acetate.}}$$

The method appears to be general, inasmuch as the same chemist has effected the synthesis of propionic acid:

$$\underbrace{C_2H_5N}_{\text{Sodium ethyl.}} + CO_2 = \underbrace{C_2H_5NaO_2}_{\text{Sodic propionate.}}$$

And, substituting carbonic oxide for carbonic anhydride, we have—

$$2(CH_3Na) + CO = \underbrace{C_3H_6O}_{\text{Acetone.}} + 2Na$$

The speaker stated that one of the most interesting of the cases of synthesis recently accomplished was that in which Mr. W. H. Perkin had succeeded in producing artificially the odoriferous principle of new hay and the tonquin bean.

The delicious fragrance of new hay is entirely due to the presence of the sweet-scented vernal grass, *anthoxanthum odoratum.* It is the same substance which is the cause of the sweet smell of the woodruff, *asperula odorata;* and the melilot, *melilotos officinalis.* It is also the flavouring ingredient in the *mai wein* of the Germans, which is perfumed with woodruff.

Until lately, nothing was known about coumarin, except that it was a colourless crystalline body, having the formula—

$$C_9H_6O_2.$$

The crystals of coumarin appear very beautiful under the influence of polarized light. The image of some artificial coumarin, which had been fused and allowed to crystallize in a plate of glass, was then thrown upon the screen, and the light being polarized by the aid of Nicol's prisms, the crystals assumed the most gorgeous and varying colours as the prisms were rotated.

The clue to its constitution was shown to be the circumstance that when heated with potassic hydrate it yields salicylic and acetic acids. The production of salicylic acid from coumarin was then shown, experimentally, the presence of the acid being proved by its yielding a deep purple coloration with ferric chloride.

Artificial coumarin was obtained from the hydride of salicyl. By treatment with sodium it yielded hydride of sodium salicyl; this substance, heated with acetic anhydride, gave hydride of aceto-salicyl. This last substance was then distilled with acetic anhydride and sodic acetate, and when the temperature reached 290°, the distillate solidified to a mass of crystals of pure coumarin, having all the fragrance and beauty of that obtained from the tonquin bean.

The speaker then submitted that the assertion he had made at an early period of his discourse that there was no natural barrier between organic and inorganic chemistry, had been amply proved by the instances he had brought forward. He said that they had studied together that evening several cases where, starting from inorganic matter, they had ascended step by step until they had reached some, of the most complicated bodies secreted by animals and vegetables. What, he said, could be more distinctly inorganic than nitrogen, carbon, and oxygen? What more distinctly an animal secretion than urea? What more completely inorganic than acetylene? What more distinctly vegetable in origin than coumarin?

Chemists have then, so far, done what a very few years would have been regarded as possible only by aid of the vital force. A true organized substance, he said, is so definite that we can almost invariably determine its molecular weight, and it is generally crystalline. But when we come to the tissues we are dealing no longer with organic substances, but with organized beings, and we feel that we are approaching the barriers which separate the study of life from the study of matter. The bonds which unite them are so close that we cannot imagine life *without* matter, and it is equally difficult to conceive the assumption of vitality *by* matter; but we must never cease to look anxiously for the solution of the problem. The impossible is a horizon which recedes as we advance, and the *terra incognita* of to-day will to-morrow be boldly mapped upon every schoolboy's chart!

[C. G. W.]

WEEKLY EVENING MEETING,

Friday, May 15, 1868.

Sir Henry Holland, Bart. M.D. D.L.C. F.R.S. President,
in the Chair.

Emanuel Deutsch, Esq.

On the Talmud.

The speaker introduced his subject by alluding to the different and generally unfavourable judgments formed about the Talmud. Talmudical investigators, he said, were like those explorers sent by Moses into the Holy Land, the majority of whom returned with tales of iron walls and monstrous giants, while the few came back carrying a huge bunch of grapes. Many were the poetical similes suggested by that strange work; but, treated strictly as a book, the nearest approach to it was Hansard. Like Hansard, it is a law-book: a collection of Parliamentary debates, of bills, motions, and resolutions. Only that while the former shows how the proposition gradually grows into an Act, in the Talmud the Act is the starting-point. The discussions in the Talmud merely seek to evolve the reasons for it out of Scripture, of which itself is a development and an outgrowth, while at the same time supplementary paragraphs are constantly evolved out of its own legal text. These bills or acts are called *Mishnah*—both collectively and individually; the discussions, *Gemara*; both together, *Talmud.*

But if Hansard contains the Debates of the Lords and Commons, the Talmud contains much more. All those manifold Assemblies wherein a people's mental, social, and religious life are considered and developed, are here represented. Parliament, Convocation, Law-courts, Academies, Colleges, the Temple, and the Synagogue—nay, even the Lobby and the Common Room have left their realistic trace upon it. The authors of this book, who number by hundreds upon hundreds, were always the most prominent men of the people in their generation, and thus designedly and undesignedly show the fullness of this people's life and progress at every turn. The Talmud, in this wise, contains—apart from the social, moral, criminal, international, human and divine Law—an account also of the education, the arts, the sciences, the history, and religion for about a thousand years:—most fully perhaps of the time immediately preceding and following the birth of Christianity. It shows us the teeming streets of Jerusalem, the tradesman at his work, the women in their domestic circle, the children at play in the market-place. The priest and the Levite ministering in their holy rites, the preacher on the hillside surrounded by the multitudes, nay, even the story-teller in the bazaar: they all live and move and have their being in these pages. Nor is it Jerusalem or even the hallowed soil of Judea alone, but the whole antique world that seems to lie embalmed in it. Athens and Alexandria, Rome and Persia, their civilizations and their religions, old and new, appear at every turn. That cosmopolitanism which for good or evil has ever been the characteristic trait of the Jewish people, is most vividly reflected in this book. One of the most striking historical points is their always coming in contact—mostly against their will—with the most prominent nations, exactly at the moment when the latter seem to have reached the highest point in their own development. Passing the three different stages of the people as Hebrews, Israelites, and Jews, we find them connected with Chaldea, Egypt, Phœnicia, Assyria, Babylonia, Persia, Greece, Rome, Arabia. Yet that cosmopolitanism never for one moment interfered with the most marked mental individuality. There always remained that one central sun, the Bible. Around this ever revolves that great cosmos, the Talmud, and from it, as shown in the *Gemara*, the *Mishnah* is begotten.

After briefly alluding to the "Sinaitic" injunctions, which had led some to invent the tale of the Talmud, as such, claiming to be "inspired"—a notion from which its own authors would have shrunk with horror—the speaker proceeded to dwell more fully on the "dates" of the individual dicta in the book: a subject which seems to have puzzled many not fully acquainted with the nature of eastern tradition. Nothing can be more authentic than the memory of the East. Many and startling instances are offered by the Brahmins and the Parsee priests who at this moment without the slightest conception of their contexts recite parrot-like entire chapters of their sacred books correct even as to accent. But in the Talmud we have, apart from the clearest and most irrefutable evidences of witnesses, all the ordinary internal evidences of history. We have an array of carefully preserved historical names and dates from beginning to end; names and dates the general faithfulness and truth of which have never yet been called into question. From the Great Synagogue down to the final completion of the Babylonian Gemara, we have the legal and philosophical development of the nation always embodied as it were in the successive principal schools and men of their times.—After entering into some historical and chronological details, the speaker alluded to those ethical sayings, parables, gnomes, &c., which were the principal vehicle of the common Jewish teaching from an almost prehistoric period. However sublime and tender and poetical their expression often be in the Talmud, he failed to see anything surprisingly new in them: anything, in fact, that was not substantially contained in the canonical and uncanonical writings of the Old Testament.

Turning to its authors, the speaker touched upon the "Priests and Pharisees," and hinted that the cry of separation of Church and State might perhaps be first heard in the Talmud, though but faintly. The fact being that the priests had sadly deteriorated, as a body, bright exceptions apart, since the days of the Maccabees, when they by an accident suddenly found themselves in political power. From being, as Moses had intended them to be, the receivers of the people's free gifts, their messengers—not mediators—and their teachers, they had become, chiefly in their upper strata, an encroaching, and at the same time ignorant faction. The ordinary priests had mostly sunk into mere local functionaries of the Temple, while many of the High Priests, who in those latter days *bought* their sacred office from the ruling foreign power, had forgotten the very elements of that Bible which they had been especially appointed to teach. The Pharisees, on the other hand, in view of the clouds that they saw gathering round the Commonwealth, had but one cry—Education: Education catholic, gratuitous, and compulsory. From one end of the Talmud to the other there resounds but one echo: learn—teach; teach—learn. The Priesthood, the Sacrifices, the Temple, as they all went down at one sudden blow, seemed scarcely to leave a gap in the religious life of the nation. The Pharisees had long before undermined these things, or rather transplanted them, into the people's houses and hearts. Every man in Israel, they said, is a priest, every man's house a temple, every man's table an altar, every man's prayer his sacrifice. Long before the Temple fell, it had been virtually superseded by hundreds of synagogues, schools, and colleges, where laymen read and expounded the Law and the Prophets. The priest as such, or the Levite, played but a very insignificant part in the synagogue or school. The function of pronouncing the "Benediction" on certain occasions and a kind of vague "Precedence" was all that the synagogue had preserved of the whilom high estate of these Aaronides.

Yet, on the other hand, instances are not wanting of these men, having lost their former privileges, applying themselves all the more vigorously to study and the great national work of Education. Nor was there any real personal antagonism between the "pharisaical" or "popular" party, and the descendants of the "sacred" tribe and family. On the contrary, one of the most cherished legends—and here as usual the legend faithfully interprets the people's real feeling—tells us how, when the enemy entered the Holy of Holies, the Priests and Levites, led by the High Priest himself, bearing aloft the golden key of the sanctuary, were seen precipitating themselves, with all the tokens and emblems of their sacred trust, into the blazing ruins of the Temple—rather than deliver them up to the conquerors.

Regarding that education which the Pharisees advocated so strenuously and indefatigably, the speaker related how they had succeeded, after many unsuccessful attempts, to make it compulsory all over the land, save Galilee. Peculiar geographical circumstances (Samaria, Phœnicia, &c.) had reduced that beautiful country to the Bœotia of Palestine. The faulty pronunciation of its inhabitants was the standing joke of the witty denizens of the metropolis. This state of things, however, was altered after the fall of Jerusalem, when Galilee in her turn became the seat of some of the most exalted Academies.

The regulations and provisions for public instruction were extremely strict and minute. The number of children allotted to one teacher, the school buildings and their sites, the road even that led to them, everything was considered; no less the age of the pupils and the duties of the parents with regard to preliminary preparation and continuous domestic supervision of their tasks. The subjects, the method, the gradual weaning even of the pupil into a teacher or helpmate of his fellow-pupils—all these things are carefully exposed in the Talmud. Above all is the great principle *Non multa sed multum*, the motto of all schooling in the Talmud. Good fundamental grounding, elementary *maternal* teaching, and constant repetition are some of the chief principles laid down. The teachers, in most cases, taught gratuitously: considering theirs a holy and godly office, for which the reward would surely not fail them. The relation between master and disciple was generally that of father and child, or friend and friend. Next to Law, Ethics, History, and Grammar—Languages were one of the principal subjects of study. We hear of Coptic, Aramaic, Persian, Median, Latin, but above all Greek. The terms in which this last language is spoken of verges indeed on the transcendental. This also is the only language which it seems to have been incumbent to teach even to girls. Medicine was another necessary subject of instruction: the hygienic laws and the anatomical knowledge (bound up with religion) transmitted to us in the book show indeed no small proficiency for its time. Mathematics and astronomy formed another part of instruction, and were indeed considered indispensable. We hear of men to whom the ways of the stars in the skies were as familiar as the streets of their native city, and others who could compute the number of drops in the ocean, who foretold the appearance of comets, &c. Next came Natural History, chiefly Botany and Zoology. The highest point, however, was reached in Jurisprudence, which formed the most extensive and thoroughly national study.

The chief aim and end of all learning—the Talmud is never tired of repeating—is *doing*. All knowledge is but a step to "modesty and the fear of heaven;" and innumerable are the parables whereby this lesson is inculcated. After briefly adverting to Prayers and Sermons and the whole worship of Temple and Synagogue at the time of Christ, the speaker turned to the "political" portions of the "Law" under consideration, and pointed out how almost the modern theory of constitutionalism was contained in it. He briefly touched upon the relationship between Royalty, State, and subjects, and the provisions for taxes, for war, the legislative and judicial powers, &c. Both this, the legal, and the other, the ethical part of the book—so closely intertwined that they can hardly be separated—may be said to grow out chiefly of one fundamental axiom of the Talmud, *viz.* the utter and absolute equality of all men and the obligation to "follow God," by imitating the mercy attributed to Him by Scripture.

Next the speaker alluded to the holy influence exercised by the women, of whom the Talmud not only records the noblest deeds, but whom, even as the Angels themselves, it makes at times the bearers of most sublime thoughts. Regarding the latter, it was shown at some length how both they and their counterparts the 'demons' were—though partly adopted from Persian or rather Zoroastrian metaphysics—made the vehicles of national Jewish doctrines. Indeed, all those pantheistic and dualistic principles which the people had gathered from the creed of other nations, were transformed under the skilful hand of the Talmudical masters into strictly monotheistic elements, by being either idealized into abstract notions of right and wrong, or surrounded by a poetical halo which deprived them of any real existence. Thus Satan (Sammael, the "Primeval Serpent"), though mythologically his functions are precisely similar to those of the Persian "Evil Spirit," *i. e.* those of Seducer, Accuser, and Angel of Death, is yet explained away philosophically as meaning merely "Passion," which seduces, produces remorse, and kills. The speaker adduced among other instances the legend of Isaac, in which "Satan," as the Angel of Death, appears first as an accuser of Abraham (as of Job) before God, next as a seducer to Abraham in the garb of an old man, to Isaac in that of a youth, finally to Sarah, informing her of the danger in which her son had been placed. The speaker further alluded to the legend of the death of Moses, in which Satan, eager to vanquish the "divine man," is thwarted by God's Name even to the end.

In the same manner Asmodeus (the Persian Aêshma), "Lilith," and the rest of the demoniacal powers, as well as those allegorical monsters the "Leviathans," the "Cocks," the "Bulls," and the rest of the ever-repeated reproaches to the Talmud (all of which are taken almost bodily from the Zendavesta), have to play their instructive part. They are either reduced into their original meanings in the Talmud, or they are ridiculed and made to inculcate some moral lesson. On the other hand the famous "Sea Fairy Tales," taken from Indian sources, are made into guises of political, if not religious satires.

After dwelling on the causes of the obscurity of some of the matters found in the Talmud and their apparent want of dignity—occasioned partly by the circumstances and the manners of the period, and partly by the neglect of copyists, and the undying fanaticism which ever tried to "improve" this important record of humanity—the speaker instanced the various modes in which the Talmudical authors figured to themselves the Messianic times, and the utter and absolute freedom with which they expressed their opinion on this as on every other religious topic.

Further remarks on the value of the Talmud as a "human study" in our days, and the scientific manner in which it should be treated, followed. It required, the speaker said, a certain system and method entirely of its own, being itself in almost every respect an exceptional work. Above all, however, the investigator should not only be armed with patience and perseverance such as is scarcely needed for any other branch of study, but he must leave all and every prejudice, religious and otherwise, behind him. Then, and then only, might he hope to gather in it some of the richest and most precious fruits of human thought and fancy.

The legend of Elijah standing on the mountains of Judea three days before the appearance of the Messiah, proclaiming peace and redemption to all mankind, formed the conclusion of the discourse.

[E. D.]

WEEKLY EVENING MEETING,

Friday, May 22, 1868.

Sir Henry Holland, Bart. M.D. D.C.L. F.R.S. President, in the Chair.

William Odling, M.B. F.R.S.

On some Effects of the Heat of the Oxy-hydrogen Flame.

I.

Chemical changes, whether of combination or decomposition, result in the production of new bodies which, under the conditions of the change, have for the most part a greater stability than the original bodies.

One evidence of this greater stability is afforded by the development of a quantity of heat—the heat of chemical action—from the produced bodies having a smaller potential heat than the original ones.

It results, both from reason and experiment, that in order to undo or reverse any definite chemical action, just so much heat must be directly or indirectly expended as was evolved by the original action.

For the same quantity of heat evolved, the resulting temperature varies with the mass and kind of matter heated, and with the rapid or gradual evolution of the heat.

When the evolution of heat is instantaneous, the resulting temperature may be calculated from the quantity of heat evolved, and the mass and specific heat, &c., of the matter heated.

By a unit of heat is meant the quantity of heat necessary to raise the temperature of one kilogramme of water one degree centigrade, or more accurately from 0° to 1°.

II.

Every 18 grammes of water is a combination of two 1-gramme proportions of hydrogen H, with one 16-gramme proportion of oxygen O; and, by the combination of two grammes of hydrogen with sixteen grammes of oxygen, there are developed 68 units of heat.

Of these 68 units of heat, however, little more than 57 units are really due to the chemical action,—nearly 11 units of heat being evolved by the contraction of the original mixed gas into two-thirds its volume of steam, and by the further condensation of the resulting steam into 18 cubic centimetres of water.

While the quantity of heat evolved by the combination of a given quantity of oxygen and hydrogen is invariable, the intensity of the heat may vary from a scarcely recognisable rise of temperature up to the highest temperature of the oxy-hydrogen blowpipe flame, capable of fusing platinum and silica.

A most remarkable effect of the intense temperature resulting from the combination of oxygen and hydrogen into water, is the partial decomposition of water into oxygen and hydrogen, discovered by Mr. Grove in 1846.

At this high temperature, hydrochloric acid and carbonic anhydride gases also undergo partial decomposition, into hydrogen and chlorine, and into carbonous oxide and oxygen respectively.

Upon what do these singular decompositions by heat, of bodies formed with great evolution of heat, depend; or with what class of chemical phenomena may they be associated?

III.

Under certain familiar conditions, chemical action seemingly takes place to its utmost possible extent in a single direction only, with production of a maximum amount of the substance that is formed with maximum evolution of heat.

For example, taking atomic proportions in grammes, the heat of formation of chloride of zinc, $ZnCl_2$, is 101 units, and the heat of formation of chloride of copper, $CuCl_2$, is 60·5 units. Hence, with chlorine in solution and excess of both copper and zinc, there is finally produced the maximum possible amount of chloride of zinc and no chloride of copper.

Again, an addition of sufficient zinc to solution of chloride of copper, there is complete combination of chlorine with zinc and complete separation of chlorine from copper, *i. e.* complete burning of the one metal and complete unburning of the other.

IV.

But under simpler though less familiar conditions, chemical action habitually takes place in more than one direction simultaneously, with production of correlative products in varying proportions.

Thus, with hydrogen and excess of both chlorine and oxygen, although the heat of formation of oxide of hydrogen H_2O is 57 units, and the heat of formation of chloride of hydrogen $2HCl$ is only 47·5 units, yet, in this case, the hydrogen does not combine with the oxygen to the exclusion of the chlorine, but divides itself between the oxygen and the chlorine in proportions which vary with the conditions of the experiment.

In accordance with this result it is found that, at the same red heat, excess of chlorine will effect the partial decomposition of water with extrusion of oxygen; and, conversely, that excess of oxygen will effect the partial decomposition of hydrochloric acid with extrusion of chlorine.

So that, beginning with the two chemical substances, water and chlorine, or beginning with the two chemical substances, hydrochloric and oxygen, or beginning with the three chemical substances, hydrogen, chlorine, and oxygen, there exist, at a full red heat, the four chemical substances, water, hydrochloric acid, chlorine, and oxygen; the proportions of the four substances depending certainly upon the relative quantities present of the elements concerned, and most probably also upon the temperature of the experiment.

Similarly, beginning with the one chemical substance, water (Grove), or beginning with the two chemical substances, oxygen and hydrogen (Bunsen), there always exist, at a sufficiently high temperature, the three chemical substances, water, oxygen, and hydrogen.

Although, by exposure to a red heat, the electrolytic mixture of oxygen and hydrogen gases becomes completely combined, or transformed into water, yet, as recently shown by Bunsen, at the high temperature of 2024 degrees, only one-half, and at the still higher temperature of 2844 degrees, only one-third of the mixture undergoes combination, the other one-half or two-thirds remaining in the state of mixed gas.

V.

Chemists are acquainted with many reciprocal actions comparable with those of chlorine upon water, and of oxygen upon hydrochloric acid, the most familiar instance being probably the decomposition of ignited oxide of iron by hydrogen with extrusion of iron, and the converse decomposition of oxide of hydrogen by ignited iron with extrusion of hydrogen.

Similarly, sodium will decompose the oxides of carbon, while carbon will decompose oxide of sodium; and just as a sufficient excess of chlorine may be made to effect the almost complete decomposition of a given quantity of water, so may a sufficient excess of carbon (or carbonous oxide) be made to effect the almost complete decomposition of a given quantity of sodium-oxide or zinc-oxide, as in the ordinary processes for obtaining the two metals; notwithstanding that, for an equal consumption of oxygen, the respective combination heats of sodium and zinc exceed by far the combination heat of carbon or carbonous oxide.

Again, although the combination heat of oxygen and carbonous oxide is 68 units, while that of oxygen and hydrogen is only 57 units, yet, as was shown by Bunsen many years ago, upon exploding a mixture of oxygen with a joint excess of carbonous oxide and hydrogen, the oxygen does not attach itself exclusively to the carbonous oxide, but divides itself between the carbonous oxide and hydrogen in a ratio determined by their relative proportions.

[W. O.]

WEEKLY EVENING MEETING,

Friday, May 29, 1868.

SIR HENRY HOLLAND, Bart. M.D. D.C.L. F.R.S. President,
in the Chair.

W. E. H. LECKY, Esq.

On the Influence of the Imagination on History.

THE imagination may be regarded in two ways, as a realizing faculty or as a creative faculty. Considered in the former light, it is the power by which men realize the unseen; considered in the latter, it is the power by which they create fictions or idealize and embellish facts. If we regard it in the first aspect, it is manifest that all historical writing as well as all historical reading is a continuous effort of the imagination. It is the business of the historian to resuscitate a society that has passed away, to cause men and manners, habits of thought and habits of feeling that no longer exist to pass once more before his reader, and the charm of his history will depend chiefly on the vividness, while its philosophic value will depend chiefly upon the fidelity, of the picture. If we regard the imagination in the second aspect, the part which it plays in history is also very great. In all histories we may discover a large mythological element produced by the undisciplined imagination of an early society, reflecting and also in a great degree determining the character of the people and comprising its first literature, its first philosophy, and its first religion. We also find many historical facts and characters that have been transfigured by the imagination. Men have so fascinated the imaginations of their fellows, that they have become the ideals of many generations, and when thus idealized, have exercised a vast and enduring influence upon the world. The same facts have often produced a history and a romance, and the romance has sometimes proved more important than the history.

After a few preliminary remarks on the effects of the imagination in magnifying men, qualities, and incidents in proportion to their dramatic interest rather than to their importance, and also upon the nature of myths, the speaker proceeded to discuss his main subject, the part played by ideals in history. He observed, that in order that a character or incident should be idealized, two things are necessary. There must be something in the object itself striking and conspicuous enough to fascinate the imagination. There must also be a pre-existing tendency which the ideal represents. Thus, when the course of events has been long tending towards military enterprise, then, and then only, a great warrior is likely to be idealized; when an ascetic tendency has been long in existence, then only an ascetic is likely to be idealized; when the direction of affairs has been to exalt monarchical institutions, then only a king becomes the ideal. But this ideal, which is thus the result of the pre-existent tendency, becomes in turn an efficient cause. For men are governed much more by their imaginations than by their reason. In order to affect them, it is necessary not only to teach them what is right, but also to show them what is right, and precepts are impotent till they are connected with examples.

Thus the early Greek philosophy and the early Greek types were essentially local and municipal. The sympathies of the people were restricted to the interests of their own petty states. The national heroes, who represented the supreme ideal of greatness, were all local heroes, and the notion of a cosmopolitan sympathy was unknown. It arose in the philosophy of Socrates and his followers; but it only became general when the career of Alexander fascinated the minds of the people, eclipsed by its splendour the glories of Pericles, Leonidas, and Miltiades, and familiarized the minds of men with the conception of a universal empire. And this was probably the most important result of the career of Alexander, for the cosmopolitanism of the Greek mind, blending with the cosmopolitanism effected in the Roman mind by the conditions of universal empire, alone rendered possible the conversion of Europe to Christianity.

We may take an illustration of the force of ideals from the history of Roman suicides. In the early days of Rome, suicide (except in the form of self-immolation as a religious rite, as in the cases of Decius and Curtius) was unknown. In the later days it was regarded as almost the normal close of a great career. Many circumstances conspired to this change, but one of the most important was the displacement of Regulus by Cato as the ideal type. Regulus is believed to be an unhistorical figure. His courage, his high honour, his patient endurance of suffering, represented the ideal of the simple and virtuous republicans. He was proposed to the young as the type or model of what a good man should be, and he exercised this fascination until the age of Cato, who reflected most perfectly the ideal of the Stoical philosophy which had just arisen, and whose tragic death, synchronizing as it did with the defeat of a glorious cause and the destruction of a great republic, exercised an extreme but very natural fascination over the Roman mind. The life and death of Cato formed the supreme model of the Stoical period of Roman history. He was deemed (as Velleius Paterculus said) of all men the one "most like to virtue;" and the ideal retained its power till the softening influences of the Greek spirit permeating Roman life rendered the Stoical type too hard and unsympathising for the spiritual wants of men when new beliefs were formed and new ideals arose to command the world.

It would be impossible within the limits of a lecture to give anything like a complete review of the successive ideals that have governed mankind. A few examples must suffice.

Thus, in the fifth century, we find Christendom completely under the fascination of the hermit life. Several causes, some of them religious and some of them arising from the social and political condition of the empire, had driven multitudes to this life, and the imaginations of men seizing on the many poetical aspects it presented, constructed an early legendary literature of which the hermit was the central figure, and which was very widely diffused throughout Europe. The ascetic ideal was, in the first place, the extreme antithesis to the old Greek exaltation of the civic virtues. Among the Greeks the first idea of a good man had been a man who devoted himself unremittingly to the service of his country. In the fifth century, the first condition of supreme virtue had become the complete abandonment of secular duties and cares. That such a character as the hermit should have been idealized in the days of Greek or Roman republicanism was impossible; but the long-continued imperial despotism and the extreme corruption of all parts of Roman administration had gradually withdrawn all the best minds from political life, had attached a certain taint or stigma to public employment, and had therefore predisposed men to accept, as their supreme type of virtue, a life entirely unconnected with the duties of a citizen. The ascetic ideal also represented the complete subjugation of the body to the mind. The virtue of Paganism had for a long period consisted solely in action. A movement, however, had long been in progress raising the contemplative virtues to the supreme place; and this movement had already in the neoplatonic period reached the point of asceticism. In the hermit life, it attained its extreme limits. Greek art had been the glorification of the body. The mediæval Mosaics represented its extreme maceration. Dio Chrysostom, in a very whimsical passage of one of his orations, had observed that a great moral lesson was to be derived from the fact that Homer, though he places his heroes by the banks of what he always expressly calls the "fishy Hellespont," never makes them eat fish, but only roast ox; for this food being "strength sufficing," fitted to make men strong, and manly, and courageous, was best suited for virtuous men. If we contrast this judgment with the protracted and, indeed, incredible fasts that were attributed to the saints of the desert, we shall have a vivid picture of the change that had come over the ideal.

The ascetic ideal, represented in innumerable legends, painted in every church, extolled from every pulpit, exercised for many centuries an absolute rule over Europe. When men thought of virtue or of greatness, the supreme type that naturally rose before their imagination was the hermit. This ideal was at last replaced by that of chivalry, which represented chiefly three innovations, the growth of the spirit of gallantry, the increased reverence for secular rank, and the military tendencies of the times.

1. The amatory character of chivalry, which was especially reflected in the romances and poems of the troubadours, was a natural rebound against the extreme misogynism of the ascetic period. The first duty of the saint was to fly from all contact with women; and a principal measure of his excellence was the number of years he had avoided seeing them. As the Puritanism of the English Commonwealth produced the licentiousness of the Restoration, so the ascetic austerities produced at length that passionate glorification of the fair sex, of which the romance of Le Petit Jehan de Saintré is perhaps the most curious example. The tide flowed in this direction with especial vehemence after the cessation of the panic which had arisen at the close of the tenth century about the approaching end of the world. The Council of Clermont, which originated the Crusades, in some degree recognized the new tendency, by imposing on the knight the religious obligation of defending all widows and virgins.

2. The growing sentiment of admiration for secular rank was a product of the feudal system. The few peasantry had almost all disappeared in Europe, most of them voluntarily enrolling themselves as serfs under some powerful lord, for the sake of the protection he could afford. The chiefs gave to their leading warriors portions of conquered land. Gradually the obligation of military service was attached to these donations, and still later they became hereditary. In this manner an hereditary aristocracy was constituted. A great hierarchy of rank arose, of which the serf was the basis and the emperor the apex. All except the highest personage were in a condition of continual subordination, and a feeling of reverence for rank was universally diffused. Men came to associate their ideal of supreme greatness with regal or noble authority, and their minds were therefore prepared to idealize any great sovereign who might arise. This

sovereign appeared in Charlemagne, whose genius exercised upon Europe a fascination not less powerful than that which Alexander had once exercised upon Greece; and Charlemagne became accordingly the centre of a whole literature of romance.

3. The growth of the military spirit had at the same time been rapidly advancing. The strong hostility the Church had at first manifested towards it, had been qualified when many tribes of warlike barbarians embraced the faith; and the military obligation, which was an essential element of feudalism, acted in the same direction. But, above all, the invasions and conquests of Mahommedanism awoke the military energies of Christendom, and at the same time determined the direction it should take.

If we now proceed to examine the Karlovingian romances, which arose under the influences that have been described, we shall find in them some significant and important characteristics.

In the first place, the ideal was changed. For many centuries the imagination of Europe had represented as its supreme and most perfect type the hermit in the desert. The central figure was now a king, a knight, a warrior. The romances of Charlemagne and the romances of Arthur form one of the most important epochs in the history of the human mind, for they mark the period when the ideal of Christendom was changed.

In the next place, we may observe how curiously the moral atmosphere reigning in Europe when the romances were composed coloured the portrait of Charlemagne. That sovereign, who, if not the greatest, was probably the most many-sided, man who ever sat upon a throne, whose vast and capacious genius radiated through almost all the forms of thought and action existing in his time, pervading all and renovating all, did no doubt combine the talents of a great conqueror with those of a great legislator and administrator; but his military expeditions were directed almost exclusively against the Germans. He made thirty-one or thirty-two expeditions against them. He conquered, he converted, he partly civilized them; and his victories had a lasting influence upon Europe. With the Spanish Mahommedans he came but very slightly in contact. He made in person only a single expedition against them; and that expedition, which was of no great magnitude and importance, and which forms altogether an insignificant episode in his reign, was unsuccessful in its issue. Such was the career of the historic Charlemagne; but the writers of the romances being imbued with the Crusading spirit which was universal at their time, totally misrepresented it. The German wars are scarcely noticed. Charlemagne is represented as having passed his entire life in a continual, heroic, and triumphant struggle with the Mahommedans of Europe, and is even gravely credited with a triumphant expedition to Jerusalem itself. The first three of the romances of the Crusades, which were all written by monks, make Charlemagne their hero.

We might have supposed, however, that the spirit of romance would have attached itself in a Crusading age rather to the grandfather of Charlemagne, than to that emperor himself. Charles Martel, by his victory near Poitiers, by freeing Provence from Mohammedanism, had in a great measure been the saviour of Europe. Yet he never was made an ideal, and he occupies only the most subordinate positions in romantic literature. For this fact three reasons may, I think, be assigned. He appeared somewhat too early, before the moral tendencies that have been indicated had acquired a sufficient force to break the spell of the ascetic ideal which governed the world. He did not possess the same dazzling genius as his grandson, and was therefore not able to fill and entrance the popular imagination. He was also regarded with much animosity by the clergy, whose property he had severely taxed; while Charlemagne was the first sovereign who rendered compulsory the payment of tithes. The figure of Charles Martel has therefore scarcely any place in the romances of chivalry, and nearly the only legend of any importance concerning him is that after his death a hermit saw him carried by demons down the crater of a volcano, on account of his sin in secularizing church property. The war of Charles Martel, however, against the Mahommedans was not forgotten, but the glory of it was transferred to the popular hero, and one of the great triumphs of the Charlemagne of romance was an expedition to rescue Nimes and Carcassone from the Mahommedans.

But it was only in the Crusades that these new tendencies acquired a decisive ascendency. In few other periods of history has there been so great a difference between the ideals created by the imagination and the realities that are recognized by history. Of all recorded wars, the Crusades are probably those which were at once most disinterested in their origin, most immoral in their execution, and most disastrous in their immediate effects. Before the Crusaders started on their expeditions they signalized their zeal by atrocious massacres of the Jews in France and Germany. At Constantinople, on their passage to the Holy Land, they perpetrated every kind of outrage; and many of the works of ancient art, and perhaps some of the works of ancient literature, perished in the conflagrations they caused. In Palestine they were guilty of atrocities which no tongue can adequately describe, no imagination conceive. All the ghastly cruelty which the fiercest intolerance could engender blended in a strange and hideous amalgam with all the licentiousness of the most unbridled lust. After the capture of Jerusalem, besides the multitudes who were slain during the siege, they deliberately massacred in cold blood the old, the women, and the children. At Ptolemais they murdered on a single occasion 2700 defenceless prisoners. The Archbishop of Tyre emphatically declared that wherever the Crusading armies had passed there was "not a chaste woman to be found." And with all this we find every description of fraud and treachery and falsehood and intrigue, the dissensions of kings, of generals, of religious orders, disintegrating the invading hosts; so that when at last, after the endurance and the infliction of countless sufferings, after two centuries of nearly unremitting war, and the destruction, it is said, of not less than two millions of lives, the armies of the Cross were finally rolled back upon Europe,—it may be truly said that they were not so much baffled by the arms of the Saracen as blasted by their own multiform, degrading, and appalling vice.

Such was the historical aspect of the Crusades, but far otherwise were they conceived by the imagination. There had been, as we have seen, a strong religious feeling and also a strong military spirit prevailing in Europe, and these two streams met at the Crusades, and the popular imagination, under their influence, conceived an ideal which is assuredly one of the most beautiful that has ever been enshrined in the poetry or in the hearts of mankind. The red cross knight of Tasso and of Spenser, combining all the courage and fire of the ancient hero with all the gentleness of the Christian saint —the virtue of humility refracted and diluted into the graces of courtesy and of modesty—the brave soldier, animated by no feeling of personal ambition, but drawing his sword only in the cause of the feeble and the oppressed, to defend his persecuted faith and raise once more the prostrate altars of his God—this was the ideal of chivalry which acquired during the Crusades its supreme authority, and its influence is felt to the present day. When we apply the epithet chivalrous to a modern gentleman, this is no unmeaning term. There is a certain aroma of refinement in the character of a modern gentleman, there is something in that fine combination of the self-assertion of honour with the self-abnegation of courtesy which was not known in the ancient world, and which may be distinctly traced to that ideal of chivalry which the Crusades made dominant in Europe.

The romances of the Crusades arose at the very beginning of the movement, and we find the popular imagination not only inventing innumerable prodigies, but also, after its usual fashion, transfiguring the simplest facts. Thus the Council of Clermont, which was in reality held in the dark days of November, was arbitrarily transferred by the romance writers to the month of May, the cheerful opening of summer appearing more suitable for an event which introduced so bright a period in the history of the world. Geography itself was sometimes recast by the imagination, and maps are in existence in which Jerusalem is represented as the centre of the earth,—doubtless because the geographer believed that its local position must correspond to its moral position.

The last example I shall give of the manner in which ideals detach themselves from realities and of the enduring influence they exercise is taken from the history of Scotland. The distinctive beauty and the great philosophic interest of the Scotch character arises from the very singular combination it displays of a romantic and chivalrous with a practical and industrial spirit. No other nation exhibits in equal perfection the union of these two elements, and the fact is in the first instance due to geographical and political causes. The seat of the first is the Highlands, the seat of the other the Lowlands. The first spirit has grown out of the old clan system; the history of the second spirit is that of the towns, of the rise of the middle classes, and of Puritanism. The enthusiasm of the first is loyalty; the enthusiasm of the second is liberty. The first spirit makes men reverent, retrospective, and imaginative; the second makes them enlightened, progressive, and practical. In no other nation do we find a vein of poetic sensibility and romantic feeling qualifying and tempering in so singular a manner a character that is essentially industrial, practical, and almost plodding. The long endurance of this romantic element may chiefly be ascribed to the existence of certain traditional and patriotic romances, which elicit the first enthusiasm of most Scotchmen, and which leave an impression upon their characters that is seldom wholly effaced. Among these romances, perhaps the most prominent is that of Mary Stuart. Her beauty, her misfortunes, and the many noble qualities she undoubtedly displayed, made her peculiarly fitted for a centre of romance; and the vein of sentiment the clan spirit had formed in Scotland speedily surrounded her with a halo of romance. How early that romance arose has been strikingly illustrated by Dean Stanley, in his work on Westminster Abbey. "Mary's tomb," he tells us, "was revered by devout Scots as the shrine of a canonized saint. 'I hear,' says Dempster thirteen years after the removal of the remains from Peterborough, 'that her bones, lately translated to the burial place of the kings of England, are resplendent with miracles.' This probably is the last instance of a miracle-working tomb in England." The same romance has continued, though in other forms, to the present day, and there are few Scotch men or women in whose character there may not be detected something that may be distinctly traced to that unhappy queen. How far the real Mary was removed from the Mary of romance, a great living historian has conclusively shown; but the ideal Mary has probably done more in the world than the real one. The latter played rather a noisy than a great part, and all trace of her influence has long since past; the former has had an abiding influence on the imaginations and the characters of her people.

It will appear from these considerations how very important the fabulous element has often proved in history. The facts of history have been in a great degree governed by its fictions. The part of the historian, in dealing with a fable, is not simply to prove its fabulous character, or even to explain the circumstances that generated it: he must also trace the influence that fable has exercised upon the world. Men as they were supposed to have been have often been more influential than men as they were, and the realities of history can never be exhaustively explained without reference to its illusions.

And in this respect historical facts resemble physical phenomena. We hear much in this place of the universe of which our world is but an infinitesimal fraction, of the strange and interchanging forces that govern its phenomena, of the all-pervading law discovered alike in its mightiest masses and in its minutest particles; but there is a universe widely different from that which modern science reveals. There is a universe of which our globe is the centre and the sun and moon are inconsiderable lights decorating its firmament, and all its phenomena are isolated and detached. It is the world of the appearances of things. As truly as the sun causes the planets to revolve in their orbits around it, as truly as the moon makes the tides of the ocean to ebb and flow beneath her power, so truly does the aspect of external nature as it is seen by the untutored eye of man form the first associations of the imagination, mould by its plastic energy the belief of succeeding generations, and imprint its characters almost indelibly on the inmost texture of the mind of man.

It is the duty of the historian not simply to describe, but also to explain, the conduct of men. He must penetrate below actions, and must reveal the characters from which actions spring. There exist in every individual mind and in every society countless tendencies, moral and intellectual, infinitely varying in their combinations, in the degrees of their intensity, and in the directions of their operations. These are the true materials of history, for these are the secret springs that determine the actions of an individual or of a society. It is the task of the historian to trace their genesis, to analyze their nature, to estimate their forces, to forecast their probable issue. Some of them may be traced to the facts of history, and some of them to its fables. Facts never acquire their greatest influence over the human mind until they have past through the medium of the imagination. Ideals ultimately govern the world, and each, before it loses its ascendency, bequeaths some moral truth as an abiding legacy to the human race. From the days of the Pagan romances, from the days of the ascetic life, from the days of Charlemagne and the Crusaders—from these, and many other periods, moral sentiments have descended to ourselves which have all their place among the many and various elements that compose the characters and determine the actions of civilized men.

[W. E. H. L.]

GENERAL MONTHLY MEETING,

Monday, June 1, 1868.

William Pole, Esq. M.A. F.R.S. in the Chair.

Mrs. Alfred Morrison, and
Rev. J. George Wrench,

were *elected* Members of the Royal Institution.

John Edward Taylor, Esq. and
Charles H. Lardner Woodd, Esq.

were *admitted* Members of the Royal Institution.

The special thanks of the Members were returned to Sir **Henry Holland**, Bart. the President, for his Tenth Annual Donation of 40*l.* to the Fund for the Promotion of Experimental Researches.

The special thanks of the Members were returned to **Felix R. Garden**, Esq. the son of the late Alexander Garden, Esq. M.R.I. for his present of a Balance, accompanied by the following memorandum:—

"Balance (of rude exterior, but singular perfection); was made by Harrison according to the plan and by order of Henry Cavendish, Esq. and passed at his death to his cousin and heir, Lord George Cavendish. By him it was presented to Sir Humphry Davy, together with the greater part of Mr. Cavendish's philosophical apparatus. Sir Humphry Davy gave it to J. G. Children, who now presents it in token of his sincere regard, and in acknowledgment of innumerable friendly offices, to Alexander Garden.—January 1st, 1830."

The **Presents** received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

Agricultural Society of England, Royal—Journal. Vol. II. No. 1. 8vo. 1868.
American Academy of Natural Sciences, Philadelphia—Proceedings. 8vo. 1867.
Davis, Alfred, Esq. M.R.I.—Textile Manufactures and Costumes of India. By J. F. Watson. 4to. 1867.
Editors—Artizan for May, 1868. 4to.
  Athenæum for May, 1868. 4to.
  British Journal of Photography for May, 1868. 4to.
  Chemical News for May, 1868. 4to.
  Engineer for May, 1868. fol.
  Geological and Natural History Repository. May, 1868. 8vo.
  Horological Journal for May, 1868. 8vo.
  Journal of Gas-Lighting for May, 1868. 4to.
  Mechanics' Magazine for May, 1868. 8vo.
  Pharmaceutical Journal for May, 1868.
  Photographic News for May, 1868. 4to.
  Practical Mechanics' Journal for May, 1868. 4to.
  Revue des Cours Scientifiques et Littéraires. May, 1868. 4to.
Franklin Institute—Journal, Nos. 507, 508. 8vo. 1868.
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  Verhandlungen, Jahrgang, 1867, Nos. 13–18. 1867.
  Hörnes, Fossilen Mollusken des Tertiær-beckens von Wien. Band II. Nos. 7, 8. fol. 1867.
Geological Society—Quarterly Journal, No 94. 8vo. 1868.
Linnean Society—Journal, Nos. 44, 45. 8vo. 1868.
Macilwain, George, Esq. M.R.I. (the Author)—Surgical Commentaries. 16to. 1868.
Mailly, M. E. (the Author)—L'Espagne Scientifique. 16to. 1868.
Manning, Frederick, Esq. M.R.I.—Catalogue of the Shakespeare Museum, Stratford-upon-Avon. 8vo. 1868.
Meteorological Society—Proceedings, No. 37. 8vo. 1868.
Oettingen. Dr. A. Von (the Author)—Meteorologische Beobachtungen in Dorpat, 1867. 8vo. 1868.
Photographic Society—Journal, No. 193. 8vo. 1868.
Society of Literature, Royal—Transactions, Vol. IX. Part 1. 8vo. 1868.
Society of Arts—Journal for May, 1868. 8vo.
Symons, G. J. (the Author)—Symons' Monthly Meteorological Magazine, May, 1868. 8vo.
Teylerian Institution, Haarlem—Archives du Musée Teyler. Vol. I. Fasc. 3. 8vo. 1868.

WEEKLY EVENING MEETING,

Friday, June 5, 1868.

Sir Henry Holland, Bart. M.D. D.C.L. F.R.S. President, in the Chair.

Sir Samuel White Baker, M.A.

On Abyssinia, or Ethiopia.

There are countries of small repute that only rise from their obscurity when certain unexpected events call them before the world: thus Abyssinia was simply known to exist, but commanded no attention, until the detention of British subjects by its king forced that wild country before the eyes of Europe.

Nevertheless, we are bound to admit that, although Abyssinia was ignored by the civilized world, there is much of interest attached to that peculiar land, inhabited by various races, whose origin is at once doubtful and mysterious, but who have for a period of nearly 1600 years maintained inviolate their belief in Christ, although surrounded upon all sides by the enemies of their faith. It is the only spot in the vast continent of Africa where Christianity absolutely took root.

In the short space of a discourse I can only endeavour to condense the past and present of Abyssinia, leaving the uncertain future to the imagination of those who contemplate its annexation to a Mahommedan empire.

To be concise, I propose to arrange the description of Abyssinia under separate heads, commencing with Geographical Position, and following in order with:

1. Geological Formation;
2. Natural Productions and Animals;
3. Ancient History;

terminating with the "Abyssinian difficulty" and the death of Theodore.

Geographical Position.—The frontiers of Abyssinia, like those of most wild countries, are exceedingly ill defined, but they may be roughly stated as comprised between 9° and 15° N. lat. and 35° to 42° E. longitude.

Although this country contains the principal streams that cause the annual inundation of the Nile, there is not one navigable river throughout the entire empire; thus Abyssinia must always struggle against the difficulties of transport.

With a considerable extent of sea-coast, Abyssinia has never hoisted a flag upon the Red Sea.

This apparent apathy is the result of geographical difficulties. The formation of the country is adverse to development, so long as it remains in savage hands.

The low sandy shore that at one time formed the bottom of the Red Sea, burns during a portion of the year with intense heat, and is devoid of water. From this level plain, the mountains of Abyssinia abruptly rise, and form an extraordinary mass of plateaux, peaks, and flat-topped rocks, to altitudes varying from 6 to 14,000 feet. This extreme variation of altitude affords a wide range of temperature: thus the thermometer which stands at 120° in the shade on the sea-coast, falls much below freezing-point in higher localities, and the unhealthy valleys are walled in by plateaux heights that may rank among the finest climates of the world.

The variation of temperature and of soil produce the most interesting botanical changes; in the lowlands are found all the well-known tropical productions, while every successive altitude exhibits the beauties of different latitudes; thus we have the wonderful stride in vegetation from the millet and the palms of the valley, to the wheat and the fir-tree of the plateaux and mountain-peaks.

The natural effect of so large a mountain mass as that presented by Abyssinia, is the attraction of rain; thus, in addition to the blessing of a favourable climate, it has the advantage of moisture. Heavy dews refresh the soil during the dry season, while numerous mountain rivulets facilitate irrigation. In the month of April the weather begins to vary, and heavy thunder-storms break upon the mountains. In May these become more frequent; the trickling beds of insignificant streams change to roaring torrents, following every storm, until the month of June ushers in the rainy season in all its grandeur of purple clouds and tremendous thunder. The mountains that in the dry season were in silence, now tremble and roar with the floods that burst from their rugged sides, and in their impetuous course from the high ground they cut the deep channels that form the only roads from the lowlands to Upper Abyssinia. These innumerable torrents, radiating from a mountain centre, at length concentrate in two great rivers flowing north-west—the Blue Nile and the Atbara. It was on 22nd June that I witnessed the dry, sandy bed of the latter suddenly invaded by the tremendous flood, and although not a cloud dimmed the parching sky, I knew that the rains were pouring in Abyssinia. It is now well known that this flood causes the periodical inundation of Lower Egypt.

Having given an outline of the effect of the mountain range, we will now seek for the cause, by glancing at the principal geological features.

The greater portion of the rocks are plutonic, and a general upheaval of the mass has been occasioned by volcanic action. We thus observe, at an altitude of several thousand feet above the ocean, masses of coral and fossil shells that at one time formed the bottom of the sea. In another district we find beds of sandstone, not only upheaved, but broken through by masses of basalt that have formed lofty mountains; while in the southern portion of the country we find that the same action that caused an upheaval on the one hand, has also caused depression on the other, as shown by the Salt Lake Assal (first seen by Harris in his mission to Shoa), which is 570 feet below the level of the sea. There can be little doubt that at the time of the great volcanic disturbance a portion of the sea must have been enclosed and caught by the upheaval of the shore and the depression of the interior; the evaporation of which has given rise to the plains of salt in that portion of Abyssinia. This salt is cut by the natives into small pieces, like hone-stones, and in certain districts it is used in lieu of coin.

Minerals.—The mineralogy of the country has never been scientifically examined, but it is known to be rich in iron and copper, and to possess in smaller quantities gold, antimony, and lead. In one district coal has long since been discovered cropping out from the mountain's side in exceedingly thick veins, said to be from 10 to 15 feet. It is with this fuel that Theodore smelted his iron, and cast his monster mortar that was to terrify the English. On the confines of Abyssinia, at Fazoclé, the Egyptians have worked gold mines for many years, and there can be little doubt that a scientific investigation of the country would lead to important mineral discoveries. I myself discovered, in the portion of Abyssinia occupied by Mek Nimmur, the presence of copper in large quantities, that was entirely unknown to the natives; also bloodstone and cornelian in extraordinary masses. So rich is the copper-ore in the district to which I allude, that it poisons the water of the stream that flows through the cliffs, and it has the effect of impoverishing all animals until they become mere living skeletons.

As the general formation of the mountain-range is basaltic, which undergoes rapid decomposition when exposed to the atmospherical changes of violent rains and intense heat, the shapes of the extreme heights are most fantastic; in some instances the sides have fallen bodily away, and have left a dice-shaped table thousands of feet above the plain; in others the natural demolition is more complete, and the top having crumbled, has widened the base with its fallen ruin, leaving a keen-edged pyramid as an unmistakable landmark among the chaos of mountain-peaks. Wherever a broad flat-topped summit is found towering above the rest, with precipitous and inaccessible sides, it is occupied as an Amba, or fortress, by some refractory chief, into which eagle's-nest he can retire with his followers when hard pressed by the enemy.

Fertility.—Although during the dry season the aspect of the mountain-range is parched and barren, the soil of the valleys and plateaux is exceedingly fertile, formed by the disintegrated portions of volcanic rocks that have been washed from the mountain sides. Even the steep slopes of these mountains give birth during the rains to a rich vegetation, which supports vast herds of cattle in the populated districts, and in those less inhabited localities attracts the numerous wild animals that constitute the "fauna" of Abyssinia—the elephant, rhinoceros, buffalo, antelopes of many varieties, giraffes, lions, leopards, hyenas, hippopotami, baboons, monkeys, &c. Throughout the country are innumerable birds, including the great and lesser bustard, guinea-fowl, francolin, and other varieties of partridges, sand-grouse, with geese, ducks, and a long list of rare species to delight the ornithologist.

Of the insect tribes the most important are the cattle-fly, or "seroot," that drives all domestic animals from the lowlands during the rainy season, and the bee.

I believe that this tormentor (the Seroot) was the instrument employed in the plague of flies inflicted upon the Egyptians by Moses. This fly is alluded to in the passage in Isaiah vii. 18:—"And it shall come to pass in that day that the Lord shall hiss for the fly that is in the uttermost part of the land of Egypt." This insect scourge appears at the commencement of the rains in June, being hatched from the egg deposited in the soil in the preceding year, and its arrival is the signal for an exodus of the people and flocks from the low rich soils to drier or more elevated localities.

The bee is the blessing of Abyssinia; the rocks and hollow trees teem with wild honey, during the months from December to April. Beeswax is one of the principal exports of the country, and mead, or "tetch," is the general beverage. This is a mixture of five parts of water to one of honey, flavoured with herbs, and fermented; it is rendered intoxicating by the addition of the leaves of a plant called by the Abyssinians "Jershooa."

The chief article of export from Abyssinia is coffee. This important production is indigenous to the soil, and flourishes with small attempts at cultivation. There can be no doubt that Abyssinia, if in the possession of a European power, would become one of the first coffee-producing countries, as the quality of soil, climate, and vast extent of mountain slopes are peculiarly adapted for that branch of agriculture. At present the great market is at Gellabāt, commonly known as Matemma; there the coffee is brought by Abyssinian merchants and sold in exchange for cotton, which is produced of excellent quality, by the Tokrooris who inhabit that district. Although the mountainous portion of Abyssinia is ill adapted for cotton cultivation, there is a large district partly uninhabited, owing to the insecurity of life near the Egyptian frontier, that is eminently suited for cotton plantations. This beautiful country is about 2500 feet above the sea-level, at the base of precipitous range of mountains, from which it slopes to the north-west, and drains into the Atbara. It is through this fertile but neglected land that the great Nile tributaries of Abyssinia cut their impetuous channels. The Takazzie, or Settite, the Angrab, Salaam, and others of less importance burst from the dark gorges of the mountain sides in imposing grandeur, and at length escaping from the narrow walls of rocks and precipices, they have cut deep valleys in the rich soil some two miles in width, at the bottom of which—200 feet below the general level of the country—they flow in superb streams, enriched with the mud that will be deposited in the Nile delta.

Although that portion of the country offers every advantage for cotton cultivation, it is almost uninhabited, neither can it become of value until the Egyptians shall have absolutely defined their frontier, and Abyssinia shall have some more civilized and stable form of government.

In a summary of the geographical features of Abyssinia, we find that it possesses nearly every variety of climate and soil; that it is rich in minerals; it possesses coal, but means of transport are difficult in the absence of roads or navigable rivers. It produces an exceedingly hardy race of horses and mules, together with an excellent breed of cattle.

With such natural advantages, it appears surprising that Abyssinia should not have progressed in the scale of civilization. This inertia must be attributed to the difficulties of its position: it is a mountainous country, surrounded by enemies differing in religion, and regarding the population of Abyssinia as Christian dogs who should be driven out or converted to Mahommedanism. Without the possession of a sea-coast, which is claimed by Egypt, and depending for security upon the difficulty of approach presented by the mountains, Abyssinia has maintained its independence simply through its isolation; it has of late years been excluded from the world, therefore it remains some centuries behind the world.

Races of mankind that have thus lived long in seclusion cling most pertinaciously to their ancient customs and traditions; thus, hemmed in among their own mountains, the Abyssinians are strictly conservative, and hold to their faith with extreme tenacity.

According to the traditions of the people, the origin of the Abyssinian race was a settlement of the Jews. Although this assertion is difficult to prove, there is no reason to doubt that the Israelites may have settled in considerable numbers at remote periods. Certain it is, that in the reign of Solomon, the Red Sea was navigated by the Jewish fleet, and the coasts were searched for every production that could add to the treasury of the king. It is written in I. Kings ix. 26:—"And King Solomon made a navy of ships in Ezion-geber, which is beside Eloth on the shore of the Red Sea, in the land of Edom." Again, we find in chap. x. 22:—"For the king had at sea a navy of Tharshish with the navy of Hiram: once in three years came the navy of Tharshish, bringing gold and silver, ivory, apes, and peacocks." Thus there were two distinct fleets in the Red Sea, the "navy of Hiram" and the "navy of Tharshish:" the latter only returned once in three years from some distant voyage, probably from Ceylon and India; while that of Hiram traded with Ophir and along the east coast of Africa.

It is therefore highly probable that Jewish settlements were established according to the traditions of the Abyssinians, which intermarried with the inhabitants; these I believe to have been originally pure Gallas, at that time either idolaters or devoid of all religion. Even at the present day the Gallas are so closely allied to the Abyssinians that they are with difficulty distinguished, both appearing to belong to the Caucasian race, and entirely distinct from all other races of the African continent. The men are well-featured, of a dark-brown colour, with good hair, while the women (especially the Gallas) are remarkable for a delicacy of feature that is at least equal to that of Europeans.

A further proof of the Israelitish extraction of the Abyssinians is rigid adherence to the ancient Jewish rites: circumcision is invariably practised, polygamy is allowed, and the harsh and cruel Jewish laws of retribution are sternly enacted.

Although the name Ethiopia was used in a broad sense by the ancients without any strict geographical limits, there can be no doubt that it is specially applied to Abyssinia in many parts of the Old Testament. At that time Abyssinia included the entire course of the Blue Nile and the Atbara, which is now possessed by Egypt. We find in Isaiah repeated allusions to the country that cannot be mistaken (18th chapter, 1, 2 verses):—"Woe to the land shadowing with wings, which is beyond the river of Ethiopia: that sendeth ambassadors by the sea, even *in vessels of bulrushes upon the waters.*"

Both in the Hebrew and in Arabic the same word denotes either the *sea* or a large river, or sheet of water; thus, in the verse quoted, the "sea" is the Nile, and even to this day the Abyssinians use no other vessels (upon the great lake Tana) than bundles of rushes lashed together in the form of canoes, which are the "vessels of bulrushes upon the waters."

Again, in Isaiah xix.: "And the waters shall fail from the sea, and the river shall be wasted and dried up. And they shall turn the rivers far away; and the brooks of defence shall be emptied and dried up; the reeds and flags shall wither."

The word "sea" is here used in the same sense, signifying the lake—most probably the lake Tana in Ethiopia (Abyssinia) which supplies water to the Blue Nile. The prophecy, "They shall turn the rivers far away," is explained by the Abyssinian traditions that, in the ancient wars between that country and Egypt, they turned the course of several large rivers, and by means of prodigious dams they directed their waters away from the Nile, thus impoverishing Egypt by preventing the annual inundation. It may be a question whether in the time of Joseph the seven years of famine in Egypt were not caused by such a disturbance of the Nile tributaries of Abyssinia.

An equally important question may be suggested, whether the low Nile that afflicted Egypt as a plague in the time of Moses was not equally the result of hostile Ethiopian engineering at the Abyssinian sources?

At that period Ethiopia was a power that had actually overrun and vanquished Egypt, and although we receive merely an outline of certain events in the Old Testament, we are indebted to Josephus for a detailed account of the life of Moses, which he gathered from books that were most probably lost at the destruction of the Alexandrian Library. He tells us that after Egypt had been vanquished by the Ethiopians, Moses was chosen to lead the Egyptian forces against the enemy. Successful in every battle, he gained great renown, and at the head of the Egyptian army he not only drove the Ethiopians out of Egypt, but he followed them into their own country until he arrived at their capital, Saaba or Soba, on the banks of the Blue Nile; not a dozen miles from the spot upon which Khartoum now stands in N. lat., about 15° 26′. This great town of Ethiopia, protected by the Blue Nile on one side and by the White Nile a few miles in the rear, was besieged by Moses, and would hardly have been taken, had not the daughter of the Ethiopian king become enraptured with the accounts she had received of Moses' renown, and grand personal appearance; her passion became so great that she sent a messenger, declaring that she would betray her father's citadel in return for the love of the Egyptian general. She did so; the gates were opened, the city of Soba was taken, and Moses returned to Egypt victorious with his Ethiopian bride. All that we hear of this lady in the Bible is contained in Numbers xii. 1:—"And Miriam and Aaron spake against Moses because of the Ethiopian woman whom he had married; for he had married an Ethiopian woman."

I have dwelt at some length upon this incident to prove that, even in the reign of the Pharaohs, Abyssinia, or Ethiopia, was a most powerful country; therefore although the present race claims a Jewish descent, at that remote period the Abyssinians could have had no admixture of Israelitish blood.

We shall now turn to a more recent, although still ancient, period of great interest in Abyssinian history, bearing in mind that Saaba, or Soba, was the capital of the country situated on the Blue Nile due south of Egypt.

In the reign of Solomon we are told that he was visited by the great Queen of the South—the Queen of Sheba. It is written in I. Kings:—"But King Solomon loved many strange women, together with the daughter of Pharaoh, women of the Moabites, Ammonites, Edomites, Zidonians, and Hittites" . . . "and he had seven hundred wives princesses, and three hundred concubines: and his wives turned away his heart."

As the daughter of the King of Saaba had become enamoured of Moses through his reputation, so it appears by Abyssinian tradition that the Queen of Sheba, perhaps of that *same Ethiopian capital Saaba*, had almost more than a feminine curiosity in making her long journey to Solomon. It is the present boast of Abyssinia that the long line of her emperors descends direct from Menilek, the son of Solomon and the Queen of Sheba.

There are several cogent reasons for the suggestion that the capital of Ethiopia (Saaba) was the country of this celebrated but mysterious queen. In the II. Chronicles ix. her offerings are thus described: "And she gave the king an hundred and twenty talents of gold, and of spices great abundance, and precious stones: neither was there any such spice as the Queen of Sheba gave King Solomon."

When speculating upon the country from which she came, we must observe the character of the presents delivered by the queen. 1st, the Gold. This is not only produced in Abyssinia generally, but even at the present day gold mines are worked by the Egyptian Government at Fazoclé, on the Blue Nile, which is the river upon which her supposed capital, Saaba, was situated.

2nd, the Spices. I imagine that the finest gum arabic is alluded to under the general name of spice, as that presented by the Queen was evidently a great rarity; it is specially remarked, "neither was there any such spice as the Queen of Sheba gave King Solomon." It so happens that the district of Kordofan, a few days' journey from the city of Saaba, is renowned for the most beautiful quality of gum arabic that is produced from its acacias.

3rd, the Precious Stones. The ancients attached great value to the topaz; and we find an allusion to this stone in Job that is curiously suggestive in the theory that the Queen of Sheba was Queen of Saaba, or Ethiopia: in chap. xxviii. 19, "The topaz of Ethiopia shall not equal it." Thus, all those presents offered to Solomon by the queen were celebrated products of Ethiopia, *i. e.* Abyssinia—gold, spices, and precious stones.

It is natural to suppose that so important a country as Ethiopia, which had at one time conquered Egypt, would be in regular communication with that power, *viâ* the Nile and the usual caravan routes; therefore the great queen would have speedily heard the reports of so renowned a king as Solomon.

Accepting the Abyssinian tradition that Menilek, the son of Solomon and Queen of Sheba, was the first of that royal line which has since governed the country, let us turn our attention to the last farewell, when Queen Sheba thus took leave of Solomon, to return into Ethiopia. She said:—"Blessed be the Lord thy God, which delighted in thee to set thee on his throne, to be king for the Lord thy God: because thy God loved Israel, to establish them for ever, therefore He made thee king over them, to do judgment and justice."—II. Chron. ix. 8.

Thus, about 986 B.C. was a belief in the true God introduced to the pagans of Ethiopia, upon the return of their queen, who had heard and been converted by the wisdom of Solomon; and thus were Jewish laws engrafted upon that land, of which David had already prophesied (Psalm lxviii. 31): "Princes shall come out of Egypt; Ethiopia shall soon stretch out her hands unto God."

We have every reason to suppose that from this time the Jewish settlements in Abyssinia commenced; but we will at once leave that ancient epoch, and turn to that most interesting time when Christianity first dawned upon the land.

In Acts viii. we have the following graphic account of the conversion to Christianity of the treasurer of Ethiopia, who is supposed to have introduced that creed upon his return to the country. It is written:—

(Ver. 26.) "And the angel of the Lord spake unto Philip, saying, Arise, and go toward the south unto the way that goeth down from Jerusalem unto Gaza, which is desert.

"And he arose and went: and, behold, a man of Ethiopia, an eunuch of great authority under Candace queen of the Ethiopians, who had the charge of all her treasure, and had come to Jerusalem for to worship, was returning, and sitting in his chariot, read Esaias the prophet.

"Then the Spirit said unto Philip, Go near, and join thyself to this chariot.

"And Philip ran thither to him, and heard him read the prophet Esaias, and said, Understandest thou what thou readest?

"And he said, How can I, except some man should guide me? And he desired Philip that he would come up and sit with him.

"The place of the scripture which he read was this, He was led as a sheep to the slaughter; and like a lamb dumb before his shearer, so opened he not his mouth: in his humiliation his judgment was taken away: and who shall declare his generation? for his life is taken from the earth.

"And the eunuch answered Philip, and said, I pray thee, of whom speaketh the prophet this? of himself, or of some other man?

"Then Philip opened his mouth, and began at the same scripture, and preached unto him Jesus.

"And as they went on their way, they came unto a certain water: and the eunuch said, See, here is water; what doth hinder me to be baptized?

"And Philip said, If thou believest with all thine heart, thou mayest. And he answered and said, I believe that Jesus Christ is the Son of God.

"And he commanded the chariot to stand still: and they went down both into the water, both Philip and the eunuch; and he baptized him."

Even to this day the Abyssinians baptize by total immersion: thus the first seeds of Christianity were sown by the eunuch, upon his return. We next hear of the general conversion of Abyssinia to Christianity, by Frumentius, in about A.D. 340, who introduced the Coptic Church from Alexandria, from which date unto the present day it has remained as an offset of that Church, under the direction of the Coptic Patriarch.

In the short space of a discourse it is impossible to give more than an outline of the principal events that have occurred in the long period of Ethiopian history: it will be sufficient to have traced the origin of the conversion to Christianity, and to pass over the less important events of the Greek settlements and the founding of Axum by the Ptolemies; the mission of the Portuguese Jesuits in the fifteenth century, who, after creating anarchy throughout the country, were expelled in 1632; and the constant encroachments of the Mahommedan Gallas. Every attempt to alter the original faith of Abyssinia has failed, even down to our own time, when in 1829 Bishops Gobat and Kugler were despatched by our Church Missionary Society. We now approach those events which are more specially interesting to us as the forerunners of the Abyssinian difficulty. Like the Jewish history, that of Abyssinia has been marked with continual blood-shedding; the descent from Solomon has been the cause of perpetual confusion, one usurper succeeding another under the pretext of legitimate birth, while the civil wars have been conducted with revolting atrocities. At the same time, the mutual hostility that marked the ancient histories of Egypt and Ethiopia has burnt with a steady flame, fresh fuel having been added to the fire by the fanatical opinions of opposing creeds, the ancient Christianity of Abyssinia clashing with the Mohammedanism of the Egyptians.

Within the last twenty-five years the Egyptians have extended their southern conquests until they have absorbed territories that originally formed portions of Ethiopia, and having no regard for boundaries, there has been a constant border warfare upon their elastic frontier. Perpetual raids have been made upon Abyssinian ground, villages plundered, and the young girls as slaves captured to fill the hareems of the wealthy. Wide and fertile tracts of land that I have already described are uninhabited, owing to the insecurity of life and property, and the Abyssinians have withdrawn into their wall of mountains. In addition to this difficulty, the hostility between the two countries has rendered it next to impossible for the Abyssinians to continue their pilgrimages to Jerusalem, to which they have attached extreme importance from very remote periods. At the same time they have lost their sea-coast, which has been annexed by Egypt.

Under these adverse circumstances, Abyssinia sought the protection of England as a Christian and powerful friend.

In the year 1848, when Ras Ali was King of Abyssinia, Mr. Plowden, who had for some years been resident as a traveller in that country, was appointed British consul by Lord Palmerston; and the first grand error was committed, by the appointment of a consul in the interior of a savage country, where he could not possibly be supported.

In the year 1853 a claimant to the throne appeared in the person of one Kasai, of Kwora, on the Senaar frontier, who declared himself to be the rightful heir to the throne, as the direct descendant of Menilek, the son of Solomon. This man combined high qualities of courage and ability that shortly procured him extraordinary influence; and his success was so unvarying and rapid, that a superstitious awe seized upon his enemies: to meet Theodore Kasai in battle was to be defeated. Ras Ali was dethroned, and in 1855 Theodore was crowned by the Aboona as "King of kings of Ethiopia."

At that time there was in Abyssinia an English gentleman adventurer, named Bell: it was this traveller who first induced our country-man Mr. Plowden to visit Abyssinia, in which country both these gentlemen eminently distinguished themselves; and after gaining an extraordinary influence with the King Theodorus, they died by the hands of his enemies. Mr. Bell served as his commander in-chief, and, after great exploits, fell in battle in the service of the king; but Mr. Plowden, who was appointed British consul for Abyssinia in 1848, was attacked upon the road during a journey, and was killed by a party of the king's enemy, Negoussee. This sad event occurred in February, 1860; and Theodore avenged the death of his friend by the execution of Negoussee and a number of his people.

At that time Theodore was a firm friend to the English, who were supposed to reciprocate the feeling, as Her Majesty's Government had already, in 1849, entered into a treaty with Abyssinia, and had also interfered for the protection of the Abyssinians from the attacks of the Egyptians; *vide* the following despatch from Lord Clarendon, then Secretary of State for Foreign Affairs, to the Consul-general of Egypt, dated 30th June, 1854:—

"You will state to the Pasha distinctly that Her Majesty's Government will not acquiesce in any assumption, either on the part of the Porte or that of himself, of any authority over the independent territories of Abyssinia; and that Her Majesty's Government will watch over the interests of the Christians in that country, and not allow them to be maltreated or oppressed by their Mussulman neighbours."*

Believing in the friendship of England, and devoted in his admiration of those good specimens of Englishmen, Plowden and Bell, who had won so great an influence, that extraordinary man Theodore, with a mind of no common capacity, organized a plan for the regeneration of his country, and determined that Ethiopia should resume her high position among the empires of the east.

There is an extreme interest in tracing the grand views of this remarkable man at that early stage of his career, before his plans had been thwarted, and he had been rendered cruel and desperate by constant rebellions, ingratitude, and treachery; before he had discovered that the material that he governed was inadequate to the great task he contemplated.

In an interesting work published by the deceased Consul Plowden's brother, we have the advantage of his manuscript written in Abyssinia at a time when he was the king's most intimate friend, and shared all his projects. He writes, in page 455:—

"King Theodorus is young in years, vigorous in all manly exercises, of a striking countenance, peculiarly polite and engaging when pleased, and mostly displaying great tact and delicacy.

"He is persuaded that he is destined to restore the glories of the Ethiopian empire and to achieve great conquests; of untiring energy, both mental and bodily, his personal and moral daring are boundless.

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* 'Abyssinian Blue Book,' p. 89.

"When aroused, his wrath is terrible, and all tremble, but at all moments he possesses a perfect self-command. Indefatigable in business, he takes little repose night or day; his ideas and language are clear and precise, hesitation is not known to him, and he has neither counsellors ncr go-betweens.

"He is fond of splendour, and receives in state, even on a campaign. He is unsparing in punishment (very necessary to restrain disorder and to restore order in such a wilderness as Abyssinia). He salutes his meanest subject with courtesy, is sincerely, though often mistakenly, religious, and will acknowledge a fault committed towards his poorest follower in a moment of passion, with sincerity and grace.

"He is generous to excess, and free from all cupidity, regarding nothing with pleasure or desire but munitions of war for his soldiers. He has hitherto exercised the utmost clemency towards the vanquished, treating them rather as his friends than his enemies. His faith is signal. 'Without Christ,' he says, 'I am nothing; if he has destined me to purify and reform this distracted kingdom, with his aid, who shall stay me?'

"The worst points in his character are his violent anger at times, his unyielding pride, as regards his kingly and divine right, and his fanatical religious zeal.

"Married himself at the altar, and strictly continent, he has ordered or persuaded all who love him to follow his example, and he exacts the greatest decency of manners and conversation; this system he hopes to extend to all classes.

"He has begun to substitute letters for verbal messages. After perusing the 'History of the Jesuits in Abyssinia,' he has decided that no Roman Catholic priests shall teach in his dominions. To foreigners, however, he permits the free exercise of their religion, but prohibits all preaching contrary to the doctrine of the Coptic church.

"He is particularly jealous of his sovereign rights and of anything that appears to trench on them; he wishes in a short time to send embassies to the great European powers, to treat with them on equal terms.

"Some of his ideas may be imperfect, others impracticable; but a man who, rising from the clouds of Abyssinian ignorance, without assistance and without advice, has done so much, and contemplates such large designs, cannot be regarded as of an ordinary stamp."

In his private conversations with Mr. Plowden the character of Theodore is strikingly displayed.

On one occasion he said, "You and Bell only love me. Abyssinians are governed by God's will, and I have yet much to do. What if I died to-night (cholera was then raging), or turned monk, but that God wills me for this work?

"If you gave me this room full of gold, of what use would it be to me? I wish for knowledge to avoid the necessity of severe punishment, and to put my country in order. As God has given this throne to me, a beggar, so let Him give me knowledge."

On another occasion, previous to his departure on a campaign, when Mr. Plowden refused to accept his gifts, he said, "Our country requires us to be hospitable; we must wash the feet of strangers. By whose power are we now in a house, and not in a wilderness? I do not give you pay or raiment, I only give you bread and water, you *must* receive them. I know you are richer than I am, but now it is only a loan, and you will repay me hereafter in deeds." I replied, "I am nothing; my Queen only can be of use to you." He said, "Listen! without God's permission all the kings of the earth could not prevail against me, and I fear them not; but your Queen, a Christian, has sent you to me, and faith unites us: Christ wills our friendship. God may design me good in this."

Mr. Plowden adds, "He then called me apart and offered me 1000 dollars to enable me to live in comfort in his absence, which I refused. He then took my hand, and said, 'All men are mortal; if anything happens to me, befriend my son. Write to your country, say you had a friend who loved you all, and who intended to send an embassy to you for your friendship, and beg them to support my son.' I assented. He said, 'I love and trust you, good-bye.'"

This is a selection from similar conversations with Mr. Plowden, which exhibit the intensity of feeling of the king; at the same time it is a record of the judgment and discretion that were displayed by the consul. He wisely declined the king's gifts, thereby upholding the dignity of the Queen, and retaining his complete independence; at the same time his relations with the king were those of the warmest friendship.

We will now change the scene. Theodore's enemies had killed his bosom friends, the two Englishmen Bell and Plowden; he was thus robbed of those two sincere advisers to whom he could always turn for an opinion, and in whom he had placed implicit trust. From that date the character of Theodore changed with succeeding events. The rebels tormented him on all sides; the European missionaries were an annoyance; the Egyptians made raids upon his frontier; he had lost those friends to whom he could turn for sympathy—they were dead. A new consul, Mr. Cameron, was appointed from England.

Theodore had been assured of the friendship of England, and he had wished to send an embassy to the Queen; this was declined on our side. He requested Mr. Cameron to forward an urgent letter to the Queen; this request was complied with; the letter was sent, and he anxiously awaited a reply.

In the meantime the Egyptians continued their attacks upon his frontier. England had, as we have already seen, interfered to protect Abyssinia from aggression, but no *active* assistance was given. The British consul, Mr. Cameron, passed over into Egypt, and remained with the enemies of Abyssinia. The king became suspicious; where were the dear friends of old in whom he had always trusted—Plowden and Bell?

Months passed away. Mr. Cameron returned to Abyssinia, but no reply arrived from the Queen to his urgent letter. Fresh insurrections broke out at home. Constantly worried by the Egyptians on one side, by the Gallas on the other, and his empire racked with internal discord, the heart of the king grew hard; all those sanguine hopes of regenerating Ethiopia, the day-dreams of his youth, had faded. Disappointed, deceived, cajoled, the naked truth lay bare before him—England had deserted him; the country in which he had trusted had not even condescended to notice his letter to the Queen. Was it possible? Perhaps that letter was never forwarded, and he had been deceived by Cameron. This suspicion was maddening. Cameron had visited the Egyptians, his enemies, and therefore he might be *his* enemy.

Years passed away—nearly three years; the King of kings of Ethiopia, Theodorus, was no longer the Theodore of former days, when he leant on his faithful counsellors, Plowden and Bell; soured and despairing, the softer portions of his nature grew callous, and all that remained were the indomitable courage, the pride, and chivalry of the king.

In the meanwhile that important letter upon which his hopes had centred, lay utterly ignored and unanswered, in some dusty pigeon-hole of the Foreign Office, while our suspected consul was lying in chains in Abyssinia!

It is painful to follow the successive phases of the Abyssinian difficulty.

Mr. Rassam was sent as British envoy. In the meantime the king had increased in tyranny and suspicion; he had placed all the Europeans in chains.

Mr. Rassam's mission failed; the Oriental etiquette of exchange of presents was frightfully transgressed. The members of Mr. Rassam's mission were added to the captives.

England was insulted. The report had spread through Egypt and India that a British consul and envoy were in chains. We determined upon war.

The events of this war are too well known to be repeated. From the days of Hannibal, who marched his army and elephants across the Alps, no such mountain-march has been performed as that by Sir Robert Napier, who with a skill and foresight unequalled in modern times has cut a road for 400 miles through the Abyssinian Alps, beaten the enemy, rescued the prisoners, and we trust has by this time returned with the army to the Red Sea.

Thus closes the Abyssinian drama; and thus all the grand visions of the proud Theodore for the regeneration of Ethiopia have dissolved. With all his faults, Theodore was a remarkable character; faithful to the memory of his first friends, the Englishmen, Plowden and Bell, he has never spilt one drop of English blood—he has never hurt a European head; he delivered the captives into our hands safe and in robust health. His army deserted him, and delivered up the key of his position. Too brave to yield, too proud to swell the triumph of his conqueror, he determined to die as he had lived, as "King of kings of Ethiopia." With a few devoted followers he faced the storm of the British assault. Crushed by the overwhelming odds, his brave adherents were swept down; and as Saul, when the battle went against him, fell upon his own sword, so the great Theodore ended his career with a pistol-shot through his brain. He had formerly received that pistol as a present from Her Majesty the Queen of England.

Thus passed away this extraordinary man. Of his cruelties we will not speak; they were not more than a repetition of those bloody events that have ever stained the history of Abyssinia. However horrible, they were inferior to those wholesale massacres committed by Moses, Joshua, and those eminent characters in Jewish history which are looked upon in Abyssinia as models of virtue: hardly were they more terrible than acts in the reign of Mary committed in our own country.

The rock of Magdala is the everlasting tombstone that covers the remains of Theodore, King of Ethiopia; at the same time it is an imperishable monument not only of British bravery and enterprise, but of British justice and moderation: unlike those savage wars that have reddened their paths with blood, and desolated the land by rapine, no single act of injustice has been reported throughout our long and difficult march, and England retires from Abyssinia respected by barbarians and honoured by the civilized world.

Thus the curtain falls upon the last act in the Abyssinian tragedy. In conclusion, I cannot help recalling those pathetic words in the first act addressed by the king to his friend Plowden at the moment of parting: he said, "Write to your country, say you had a friend who loved you all, and who intended to send an embassy to you for your friendship, and beg them to support my son;" the consul assented, and Theodore's last words were, "I love and trust you, good-bye."

All Englishmen must be gratified that this confidence was not misplaced. At that time Theodore was our friend, but the trust has been deemed sacred although he died as our enemy. Only yesterday a telegram was received from Sir Robert Napier, reporting the death of Theodore's queen, and craving permission that the orphan son, a child, might be taken to Bombay for protection and education. Thus a descendant of Solomon and the Queen of Sheba remains beneath the British flag, who, civilized by our laws and educated in the mercies of Christianity, may on some future day sit upon his late father's throne, and carry out his grand but futile plans for the regeneration of Abyssinia.

[S. W. B.]

WEEKLY EVENING MEETING,

Friday, June 12, 1868.

Sir Henry Holland, Bart. M.D. D.C.L. F.R.S. President, in the Chair.

Edward Frankland, F.R.S.

PROFESSOR OF CHEMISTRY R.I.

On the Source of Light in Luminous Flames.

The most prolific source of error amongst mankind is the unquestioning acceptation of authoritative opinion. However much we may pride ourselves upon the sifting of the explanations of things by our own enlightened judgments, it cannot be denied that the *ipse dixit* mode of settlement is still wonderfully frequent amongst us. Not only is this the case with the public in general, but even the cultivators of science are not entirely innocent of the same weakness.

The essential difference between a fact and a theory is not always appreciated with sufficient vividness. The statement that "16 parts by weight of oxygen unite with 2 parts of hydrogen to form water," is considered by many, for instance, as perfectly synonymous with the assertion that "1 atom of oxygen unites with 2 atoms of hydrogen to form water."

The existence of an imponderable ethereal medium filling all space is often regarded as equally certain with the presence of a gaseous envelope surrounding our globe.

The atomic theory and the hypothesis of an ethereal medium are, at present, absolutely necessary, the one to the progress of chemistry, the other to the further development of physics; but neither this circumstance nor the splendid discoveries made by their aid can establish their truth. A mathematician starting from false data is sure to arrive at a false result; but it is far otherwise with theory, for false theories can, and constantly do, conduct to true facts. Thus Columbus's counterpoise theory of the earth led to the discovery of America, although that theory was nevertheless essentially false.

The most sober worker in science cannot progress without the assistance of theory to co-ordinate his facts, and to lead him on to further research. It is here that even a false theory is invaluable, and it is only when the theory continues to be held after it has become opposed to facts, that it exercises a prejudicial influence upon the progress of science. Then it hinders rather than expedites the advance of the experimenter, and ought to be at once abandoned.

In pursuing the investigation forming the subject of this discourse, the speaker had been compelled thus to abandon a theory of the source of light in luminous flames, which he, in common with others, had derived from Davy's classical researches on flame.

Our text-books answer the question, *What is the source of light in a luminous gas or candle flame?* in the most positive and unanimous manner.

Selecting from some of the most celebrated, the following quotations may be made:—

"All our artificial lights depend upon the ignition of solid matter, in the intense heat developed by the chemical changes attendant on combustion."—*W. A. Miller.*

"Whenever hydrocarbons are imperfectly burnt, there is a deposition of carbon, and this temporary deposition of carbon is an *essential* condition for the production of the white light required in an ordinary flame."—*Williamson.*

"The illuminating power of the gas flame is therefore due to these *carbon particles*, which are afterwards burned nearer the border of the flame."—*Balfour Stewart.*

"The brightness or illuminating power of flame depends not only on the degree of heat, but likewise on the presence or absence of solid particles which may act as radiant points. A flame containing no such particles emits but a feeble light, even if its temperature is the highest possible."—*Watts.*

The speaker then proceeded to investigate a number of different flames: he showed that there are many flames possessing a high degree of luminosity, which cannot possibly contain solid particles. Thus the flame of metallic arsenic burning in oxygen emits a remarkably intense white light; and as metallic arsenic volatilizes at 180° C., and its product of combustion, arsenious anhydride, at 218° C., whilst the temperature of incandescence in solids is at least 500° C., it is obviously impossible here to assume the presence of ignited solid particles in the flame. Again, if carbonic disulphide vapour be made to burn in oxygen, or oxygen in carbonic disulphide vapour, an almost insupportably brilliant light is the result; now fuliginous matter is never present in any part of this flame, and the boiling point of sulphur (440° C.) is below the temperature of incandescence, so that the assumption of solid particles in the flame is here also inadmissible. If the last experiment be varied by the substitution of nitric oxide gas for oxygen, the result is still the same; and the dazzling light produced by the combustion of these compounds is also so rich in the more refrangible rays, that it has been employed in taking instantaneous photographs, and for exhibiting the phenomena of fluorescence. Lastly, amongst the chemical reactions celebrated for the production of dazzling light, there are few which surpass the active combustion of phosphorus in oxygen. Now phosphoric anhydride, the product of this combustion, is volatile at a red heat,* and it is therefore manifestly impossible that this substance should exist in the solid form at the temperature of the phosphorus flame, which far transcends the melting point of platinum.

For these reasons, and for others which the speaker had stated in a course of lectures on Coal-Gas, delivered in March, 1867, and printed in the 'Journal of Gas Lighting,' he considered that incandescent particles of carbon are not the source of light in gas and candle flames, but that the luminosity of these flames is due to radiations from dense, but transparent hydrocarbon vapours. As a further generalization from the above-mentioned experiments, he was led to the conclusion that dense gases and vapours become luminous at much lower temperatures than aeriform fluids of comparatively low specific gravity; and that this result is to a great extent, if not altogether, independent of the nature of the gas or vapour, inasmuch as he found that gases of low density, which are not luminous at a given temperature when burnt under common atmospheric pressure, become so when they are simultaneously compressed. Thus mixtures of hydrogen and carbonic oxide with oxygen emit but little light when they are burnt or exploded in free air; but exhibit intense luminosity when exploded in closed glass vessels, so as to prevent their expansion at the moment of combustion.

In a communication just made to the Royal Society the speaker had described the extension of these experiments to the combustion of jets of hydrogen and carbonic oxide in oxygen under a pressure gradually increasing to twenty atmospheres. These experiments, which were conducted in the laboratory of the Royal Institution, were made in a strong wrought-iron vessel furnished with a thick glass plate of sufficient size to permit of the optical examination of the flame. The appearance of a jet of hydrogen burning in oxygen under the ordinary atmospheric pressure was exhibited. On increasing the pressure to two atmospheres, the previously feeble luminosity was shown to be very markedly augmented, whilst at ten atmospheres' pressure, the light emitted by a jet about one inch long was amply sufficient to enable the observer to read a newspaper at a distance of two feet from the flame, and this without any reflecting surface behind the flame. Examined by the spectroscope, *the spectrum of this flame is bright and perfectly continuous from red to violet.*

With a higher initial luminosity, the flame of carbonic oxide in oxygen becomes much more luminous at a pressure of ten atmospheres than a flame of hydrogen of the same size and burning under the same pressure. The spectrum of carbonic oxide burning in oxygen under a pressure of fourteen atmospheres is very brilliant and perfectly continuous.

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* Davy mentions this fact in connection with his view of the source of luminosity in flames, and endeavours to explain the, to him, anomalous phenomenon. He says:—"Since this paper has been written, I have found that phosphoric acid volatilizes slowly at a strong red heat, but under moderate pressure it bears a white heat; and in a flame so intense as that of phosphorus, the elastic force must produce the effect of compression."—*Davy's Works*, vol. vi., p. 48.

If it be true that dense gases emit more light than rare ones when ignited, the passage of the electric spark through different gases ought to produce an amount of light varying with the density of the gas; and the speaker showed that electric sparks passed as nearly as possible, under similar conditions, through hydrogen, oxygen, chlorine, and sulphurous anhydride, emit light, the intensity of which is very slight in the case of hydrogen, considerable in that of oxygen, and very great in the case of chlorine and sulphurous anhydride. On passing a stream of induction sparks through the gas standing over liquefied sulphurous anhydride in a strong tube at the ordinary temperature, when a pressure of about three atmospheres was exerted by the gas, a very brilliant light was obtained. A stream of induction sparks was passed through air confined in a glass tube connected with a condensing syringe, and the pressure of the air being then augmented to two or three atmospheres, a very marked increase in the luminosity of the sparks was observed, whilst on allowing the condensed air to escape, the same phenomena were observed in the reverse order.

Way's mercurial light was also exhibited as an instance of intense light produced by the ignition of the heavy vapour of mercury.

The gases and vapours just mentioned have the following relative densities:—

Hydrogen	1
Air	14·5
Oxygen	16
Sulphurous anhydride	32
Chlorine	35·5
Mercury	100
Phosphoric anhydride	71 or 142

The feeble light emitted by phosphorus when burning in chlorine seems, at first sight, to be an exception to the law just indicated, for the density of the product of combustion (phosphorous trichloride) 68·7 would lead us to anticipate the evolution of considerable light. But it must be borne in mind that the luminosity of a flame depends also upon its temperature, and it can be shown that the temperature in this case is probably greatly inferior to that produced by the combustion of phosphorus in oxygen. We have not all the necessary data for calculating the temperature of these flames, but, according to Andrews, phosphorus burnt in oxygen gives 5747 heat units, which, divided by the weight of the product from one grain of phosphorus, gives 2500 units. When phosphorus burns in chlorine, it gives only, according to the same authority, 2085 heat units, which, divided as before by the weight of the product, gives 470 units. It is therefore evident that the temperature in the latter case must be greatly below that produced in the former, unless the specific heat of phosphoric anhydride be enormously higher than that of phosphorous trichloride. The speaker had, in fact, found that if the temperature of the flame of phosphorus, burning in chlorine, be raised about 500° C. by previously heating both elements to that extent, the flame emitted a brilliant white light.

To return to ordinary luminous flames, the argument of the *necessity* of solid particles to explain their luminosity obviously falls to the ground; and a closer examination into the evidence of the existence of these particles reveals its extreme weakness. Soot from a gas flame is not elementary carbon, it always contains hydrogen. The perfect transparency of the luminous portion of flame also tends to negative the idea of the presence in it of solid particles. The continuous spectrum of gas and candle flames does not require, as is commonly supposed, the assumption of solid particles. The spectra of the flames of carbonic oxide in air, of carbonic disulphide, arsenic, and phosphorus in oxygen, are continuous, and so, as we have seen, is that of hydrogen burning in oxygen under a pressure of ten atmospheres. It is to the behaviour of hydrocarbons under the influence of heat that we must look for the source of luminosity in a gas flame. These gradually lose hydrogen, whilst their carbon atoms coalesce to form compounds of greater complexity, and consequently of greater vapour density. Thus marsh-gas ($C\,H_4$) becomes acetylene ($C_2\,H_2$), and the density increases from 8 to 13. Again, olefiant gas ($C_2\,H_4$) forms naphthaline ($C_{10}\,H_8$), when the vapour density augments from 14 to 64. These are some of the dense hydrocarbons which are known to exist in a gas flame, but there are doubtless others still more dense; pitch, for instance, must consist of the condensed vapours of such heavy hydrocarbons, for it distils over from the retorts in the process of gas-making. Candle flames are similarly constituted. The direct dependence of the luminosity of gas and candle flames upon atmospheric pressure, also strongly confirms the view that the light of these flames is due to incandescent dense vapours.

This inquiry cannot be confined to terrestrial objects. Science seeks alike for law in the meanest and grandest objects of creation. From questioning a candle she addresses herself to suns, stars, nebulæ, and comets; the same considerations which have just been applied to gas and candle flames are equally pertinent to these great cosmical sources of light.

[E. F.]

GENERAL MONTHLY MEETING,

Monday, July 6, 1868.

Sir Henry Holland, Bart. M.D. D.C.L. F.R.S. President, in the Chair.

John Glas Sandeman, Esq.

was *elected* a Member of the Royal Institution.

The Managers announced, That, in conformity with the Deed of Endowment, they had appointed William Odling, Esq. M.B. F.R.S. Fullerian Professor of Chemistry, in the room of the late Professor Faraday.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

Governor-General of India—Geological Survey of India:—
  Annual Report. 1867.
  Memoirs. Vol. VI. Parts 1, 2. 8vo. 1868.
  Catalogue of Meteorites. 4to. 1867.
  Palæontologia Indica. V. 1-4. 4to. 1867.
British Museum, Trustees—Catalogue of Specimens of Blatariæ. 8vo. 1868.
Commissioners in Lunacy—Twenty-second Report. 8vo. 1868.
Asiatic Society, Royal—Journal. New Series. Vol. III. Part 1. 8vo. 1868.
Astronomical Society, Royal—Monthly Notices, Vol. XXVIII. No. 7. 8vo. 1868.
Bavarian Academy of Science—Sitzungsberichte, 1867. II. Heft 4. 1868. I. Heft 1, 2. 8vo.
Bremen Naturwissenschaftliche Gesellschaft—Abhandlungen, Band I. Heft. 3. 8vo. 1868.
British Architects, Royal Institute of—Sessional Papers. 4to. 1867–8.
Chemical Society—Journal for May, June, 1868. 8vo.
Churchill, Messrs.—Quarterly Journal of Science for July, 1868. 8vo.
  London Student. No. 4. 8vo. 1868.
Dickson, Dr.—Dr. G. Naranzi sur l'Epidémie do Hindie (K 95) 8vo. 1868.
Editors—American Journal of Science and Arts, No. 135. 8vo. 1868.
  Artizan for June, 1868. 4to.
  Athenæum for June, 1868. 4to.
  British Journal of Photography for June, 1868. 4to.
  Chemical News for June, 1868. 4to.
  Engineer for June, 1868. fol.
  Geological and Natural History Repository, June, 1868. 8vo.
  Horological Journal for June, 1868. 8vo.
  Journal of Gas-Lighting for June, 1868. 4to.
  Mechanics' Magazine for June, 1868. 8vo.
  Pharmaceutical Journal for June, 1868.
  Photographic News for June, 1868. 4to.
  Practical Mechanics' Journal for June, 1868. 4to.
  Revue des Cours Scientifiques et Littéraires. June, 1868. 4to.
Geological Institute, Imperial, Vienna—Jahrbuch, Band XVIII. No. 1.
  Verhandlungen: Jahrgang, 1868. No. 1.
Hope, Alexander J. B. Beresford, Esq. M.P.—Speech on Metric Weights and Measures. (K 95) 8vo. 1868.
Horticultural Society, Royal—Proceedings, No. 11. 8vo. 1868.
Linnean Society—Journal, Nos. 41, 46, 47. 8vo. 1868.
Linton, Rev. H. M.A. (*the Author*) –The Scriptures arranged as written in the Order of Time. 2nd ed. 1866.
Montpellier Académie des Sciences—Mémoires. Tomes 5 et 6. Fasc 1. 4to. 1863–4.
  Procès Verbaux, &c. 1863. 4to. 1864.
Photographic Society—Journal, No. 194. 8vo. 1868.
Society of Arts—Journal for June, 1868. 8vo.
Symons, G. J. (*the Author*)—Meteorological Magazine, June, 1868. 8vo.
United Service Institution, Royal—Journal, April, 1868. Appendix. 8vo.
Wechniakoff, M. Theodore (*the Author*)—Recherches sur les Conditions de la Production Scientifique et Esthétique. 8vo. 1865–6.
Zoological Society of London—Transactions, Vol. VI. Part 5. 1868.
  Proceedings, 1867. Part 3. 8vo.

Royal Institution of Great Britain.

1868.

GENERAL MONTHLY MEETING,

Monday, November 2, 1868.

Wm. Pole, Esq. M.A. F.R.S. in the Chair.

Musgrave Brisco, Esq.
was *elected* a Member of the Royal Institution.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

Accademia Pontificia de' Nuovi Lincei, Roma—Atti: Anno II. 1849. 4to. 1867. Anno XX. 4to. 1866–8.
Actuaries, Institute of—Journal. No. 73. 8vo. 1868.
Agricultural Society of England, Royal—Journal. New Series. No. 8. 8vo. 1868.
Antiquaries, Society of—Proceedings. Vol. III. No. 7. Vol. IV. Nos. 1, 2. 8vo. 1867–8.
Archæologia. Vol. XLI. Part 2. 4to. 1868.
Asiatic Society of Bengal—Proceedings, 1868. Nos. 1–5. 8vo.
Journal. No. 143. 8vo. 1868.
Astronomical Society, Royal—Monthly Notices. Vol. XXVIII. Nos. 8, 9. 8vo. 1868.
Barrett, W. F. Esq. (the Author)—On Combination of Rectangular Vibrations. (Phil. Mag. Sept. 1868.)
On Musical and Sensitive Flames. 8vo. 1868.
Bavarian Academy of Science, Royal—Sitzungsberichte, 1868. Band I. Heft 2, 3. Band II. Heft 1. 8vo.
Bigsby, John J. M.D. F.G.S. (the Author)—Thesaurus Siluricus: The Fauna and Flora of the Silurian Period. 4to. 1868.
Bombay Branch of the Royal Asiatic Society—Journal. Vol. VIII. No. 24. 8vo. 1868.
British Association for the Advancement of Science—Report of the 37th Meeting; at Dundee. 8vo. 1868.
British Museum, Trustees—Catalogue of the Diurnal Lepidoptera. 8vo. 1868.
Guide to the Christy Collection. 12mo. 1868.
Catlow, Joseph Peel, Esq. (the Author), by his Executors—Principles of Æsthetic Medicine. 8vo. 1867.
Chambers, George F. Esq. F.R.A.S. M.R.I. (the Author)—Handbook for Visitors to Eastbourne. 16to. 1868.
Chemical Society—Journal for July to Oct. 1868. 8vo.
Clinical Society of London—Transactions. Vol. I. 8vo. 1868.
Corporation of London—Catalogue of Sculpture, Paintings, and Engravings. Part II. 8vo. 1868.
Catalogue of Library. 8th Supplement. 8vo. 1868.

Editors—American Journal of Science and Arts. Nos. 136, 137. 8vo. 1868.
  Artizan for July to Oct. 1868. 4to.
  Athenæum for July to Oct. 1868. 4to.
  British Journal of Photography for July to Oct. 1868. 4to.
  Chemical News for July to Oct. 1868. 4to.
  Engineer for July to Oct. 1868. Fol.
  Geological and Natural History Repertory. Aug.-Oct. 1868. 8vo.
  Horological Journal for July to Oct. 1868. 8vo.
  Journal of Gas-Lighting for July to Oct. 1868. 4to.
  Mechanics' Magazine for July to Oct. 1868. 8vo.
  Pharmaceutical Journal for July to Oct. 1868.
  Photographic News for July to Oct. 1868. 4to.
  Practical Mechanics' Journal for July to Oct. 1868. 4to.
  Revue des Cours Scientifiques et Littéraires. Juillet-Oct. 1868.
Ellis, Alexander J. Esq. F.R.S. M.R.I. (the Author)—The only English Proclamation of Henry III. 8vo. 1868.
Faraday, Mrs.—Eloge Historique de Michel Faraday. Par M. Dumas. 4to. 1868.
Franklin Institute—Journal. Nos. 510–513. 8vo. 1868.
Geographical Society, Royal—Proceedings. Vol. XII. Nos. 2–5. 8vo. 1868.
  Journal. Vol. XXXVII. 8vo. 1868.
Geological Institute, Imperial, Vienna—Jahrbuch. Band XVIII. No. 2. 8vo. 1868.
Geological Society—Quarterly Journal. No. 95. 8vo. 1868.
Greenwich, Royal Observatory (through the Royal Society)—Greenwich Observations for 1866. 4to. 1868.
Heugh, John, Esq. M.R.I.—Report on the Mexican and United States Boundary Survey. 3 vols. 4to. 1857–8.
  Reports on Explorations and Surveys for a Railway from the Mississippi to the Pacific Ocean. 13 vols. fol. 1855–60.
  Report on the Colorado River from the West. 4to. 1861.
  Reports on Naval Astronomical Expedition to the Southern Hemisphere. 1849–50. 4 vols. fol. 1855–6.
Linnean Society—Transactions. Vol. XXVI. Part 1. 4to. 1868.
  Journal. Nos. 42, 43. 8vo. 1868.
Lockyer, J. Norman, Esq. M.R.I. (the Author)—Elementary Lessons in Astronomy. 16to. 1868.
Madrid, Royal Academy of Sciences—Libros de Saber de Astronomia del Rey Alfonso X. Tomo V. Parte I. fol. 1867.
Mechanical Engineers' Institution, Birmingham—Proceedings, June, 1867. Part 3. 8vo.
Medical and Chirurgical Society, Royal—Proceedings. Vol. VI. No. 2. 8vo. 1868.
Melde, Dr. F. (the Author)—Experimentaluntersuchungen über Blasenbildung. Marburg. 8vo. 1868.
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Victoria Institute—Journal of Transactions. Vol. II. No. 7. 8vo. 1868. Vol. III. No. 9. 8vo. 1868.
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Wolowski, M. (the Author)—Quelques Notes sur la Question Monétaire. (K 95) 8vo. 1868.
Zoological Society of London—Transactions. Vol. VI. Parts 6, 7. 4to. 1867–8. Proceedings, 1868. Part 1. 8vo.

MUSEUM.

Tuke, Dr. T. Harrington, M.R.I.—Lachrymatory from a Tomb at Cyrene.

GENERAL MONTHLY MEETING,

Monday, December 7, 1868.

SIR HENRY HOLLAND, Bart. M.D. D.C.L. F.R.S. President, in the Chair.

Robert Douglas Hale, M.D.
Frederick Gutteres Henriques, Esq.
Edward Frankland, Esq. Ph. D. F.R.S. Corresponding Member of the Academy of Sciences, Paris, and Professor of Chemistry at the Royal School of Mines.
George James Shaw, M.D.
Captain the Hon. William Le Poer Trench, and
John Peter Wilson, Esq.

were *elected* Members of the Royal Institution.

The following Lecture Arrangements for 1868–9 were announced:—

Professor ODLING, F.R.S.—Six Lectures (*adapted to a Juvenile Auditory*), 'On the Chemical Changes of Carbon.' On December 29th, 31st, 1868; January 2nd, 5th, 7th, 9th, 1869.

Before Easter.

RICHARD WESTMACOTT, Esq. R.A. F.R.S.—Six Lectures, 'On Subjects connected with Fine Art.' On Tuesdays, January 12th to February 16th, 1869.

Rev. FREDERIC W. FARRAR, M.A. F.R.S.—Four Lectures, 'On the History and Results of Comparative Philology.' On Tuesdays, February 23rd to March 16th

Professor T. RUPERT JONES.—Three Lectures, 'On the Protozoa, or Simplest Animal Forms, and their Distribution in Time and Space, and the Results of their Agency on the Earth's Surface.' On Thursdays, January 14th, 21st, 28th.

Dr. Michael Foster.—Three Lectures, 'On the Involuntary Movements of Animals.' On Thursdays, February 4th, 11th, 18th.

Dr. John Harley.—Two Lectures, 'On Respiration and its Influence on the Heart.' On Thursdays, February 25th and March 4th.

Dr. Henry Power.—Two Lectures, 'On the Eye in Animals and Man.' On Thursdays, March 11th and 18th.

Professor Odling, F.R.S.—Ten Lectures, 'On Hydrogen and its Analogues.' On Saturdays, January 16th to March 20th.

After Easter.

Professor Robert Grant, LL.D. F.R.S.—Nine Lectures, 'On Stellar Astronomy.' On Tuesdays, April 6th to June 1st.

Professor Tyndall, LL.D. F.R.S.—Nine Lectures, 'On Light.' On Thursdays, April 8th to June 3rd.

Archibald Geikie, Esq. F.R.S.—Three Lectures, 'On the Origin of Land-surfaces.' On Saturdays, April 10th, 17th, 24th.

Professor Seeley.—Three Lectures, 'On Roman History.' On Saturdays, May 8th, 15th, 22nd.

Emanuel Deutsch, Esq.—Three Lectures, 'On Semitic Culture.' On Saturdays, May 29th to June 12th.

The following Presents were laid on the table, and the thanks of the Members returned for the same:—

American Academy of Arts and Sciences—Proceedings. Vol. VII. Nos. 24–43. 8vo. 1866–7.
Memoirs. Vol. IX. Part 1. 4to. 1867.
American Philosophical Society—Proceedings, No. 77. 8vo. 1868.
Asiatic Society of Bengal—Proceedings, 1868, Nos. 6–8. 8vo.
Journal, Nos. 144–146. 8vo. 1868.
Boston Society of Natural History, U.S.—Memoirs. Vol. I. Part 3. 4to. 1868.
Proceedings. Vol. XI. Nos. 7–30. 8vo. 1867–8.
Condition and Doings, 1867, 1868. 8vo.
Annual, 1868–9. 12mo.
British Museum Trustees—Catalogue of Additions to MSS. 1848–53. 8vo. 1868.
British Pharmaceutical Conference—Proceedings at the Norwich Meeting, 1868. 8vo.
Chemical Society—Journal for Nov. 1868. 8vo.
Editors—Artizan for Nov. 1868. 4to.
Athenæum for July to Nov. 1868. 4to.
British Journal of Photography for Nov. 1868. 4to.
Chemical News for Nov. 1868. 4to.
Engineer for Nov. 1868. fol.
Geological and Natural History Repository. Nov. 1868. 8vo.
Horological Journal for Nov. 1868. 8vo.
Journal of Gas-Lighting for Nov. 1868. 4to.
Mechanics' Magazine for Nov. 1868. 8vo.
Pharmaceutical Journal for Nov. 1868
Photographic News for Nov. 1868. 4to.
Practical Mechanics' Journal for Nov. 1868. 4to.
Revue des Cours Scientifiques et Littéraires. Nov. 1868.
Essex Institute, U.S.—Proceedings. Vol. V. Nos. 5, 6. 1868.
Franklin Institute—Journal, No. 514. 8vo. 1868.
Geological Society—Quarterly Journal, No. 96. 8vo. 1868.
Lords of the Admiralty—Nautical Almanac for 1872. 8vo. 1868.
Linnean Society—Journal, No. 44. 8vo. 1868.

Mechanical Engineers' Institution, Birmingham—Proceedings, Oct. 1867. 8vo.
Mensbrugghe, M. G. Vander (*the Author*)—Sur la Tension des Lames Liquides (K 95) 8vo. 1866–7.
Photographic Society—Journal, No. 199. 8vo. 1868.
Plateau, M J. Hon. M.R.I (*the Author*)—Recherches sur les Figures d'Equilibre d'une Masse liquide sans Pesanteur. 8e Série. (Mém. de l'Acad. de Belgique. Tome XXXVII.) 4to. 1868.
Rosetti, Dr. F. (*the Author*)—Sul Maximum di Densità e sulla Dilatazione dell' Acqua dell' Adriatico. (K 95) 8vo. 1868.
Royal Medical and Chirurgical Society—Transactions. Vol. LI. 8vo. 1868.
Smithsonian Institution, U.S.—Annual Report, 1866. 8vo. 1867.
Smithsonian Contributions. Vol. XV. 4to. 1867.
Upsala, Société Royale des Sciences—Nova Acta. Serie 3. Vol. VI. Fasc. 2. 4to. 1868.
Upsala Universitets Arsskrift, 1866, 1867. 8vo.
White, Rev. James, M.A. (*the Author*)—On the Curved Rack in Moncrieff's Protected Barbette Gun-Carriage. (L 15) 8vo. 1868.
Zoological Society of London—Proceedings, 1868. Part 2.

1869.

WEEKLY EVENING MEETING,

Friday, January 15, 1869.

SIR HENRY HOLLAND, Bart. M.D. D.C.L. F.R.S. President, in the Chair.

PROFESSOR TYNDALL, LL.D. F.R.S.

On Chemical Rays, and the Light of the Sky.

THE first physical investigation of any importance in which, jointly with my friend Professor Knoblauch, I took part, bore the title, "The Magneto-optic Properties of Crystals, and the Relation of Magnetism and Diamagnetism to Molecular Arrangement."* This investigation compelled me to reflect upon the structure of crystals, on their optical properties in relation to that structure, and more particularly on the striking phenomena exhibited by many of them in the field of a sufficiently powerful magnet. These were evidently due to the manner in which the molecules of the crystals were built together by the force of crystallization: and it was natural, if not necessary for me, to employ such strength of imagination as I possessed in obtaining a mental picture of this molecular architecture. The inquiry gave a tinge and bias to my subsequent scientific thought, rendering, as it did, the conceptions and pursuit of molecular physics pleasant to me. Its influence is to be traced in most of my scientific work. The first lecture, for example, which I ever delivered in this theatre, was "On the Influence of Material Aggregation on the Manifestations of Force;" by "material aggregation" being meant the way in which, by nature or by art, the particles of matter are arranged together. In 1853 I also published a paper "On Molecular Influences," in which common heat was made the explorer of organic structure. In the "Bakerian Lecture," given before the Royal Society in 1855, the same idea and phraseology crop out. The Bakerian Lecture for 1864 bears the title "Contributions to Molecular Physics." And all through the investigations which have occupied me during the last ten years, my wish and aim have been to make radiant heat an instrument by which to lay hold of the ultimate particles of matter.

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* 'Philosophical Magazine,' July, 1850.

The labours now to be considered lie in the same direction. In the researches just referred to, I employed tubes of glass and brass, called, for the sake of distinction, "experimental tubes," in which radiant heat was acted upon by the gases and vapours subjected to examination. Wishing, two or three months ago, to render visible what occurred within these tubes on the entrance of the gases or vapours, I found it necessary to intensely illuminate their interiors. The source of illumination chosen was the electric light; the beam of which, converged by a suitable lens, was sent along the axis of the tube. The dirt and filth in which we habitually live were strikingly revealed by this method of illumination. For, wash our tube as we might with water, alcohol, acid, or alkali, until its appearance in ordinary daylight was that of absolute purity, the delusive character of this appearance was in most cases revealed by the electric beam. In fact, in air so dirty as that which supplies our lungs—and I will not say that we could get on healthily without the "dirt"*—it is not possible to be more than approximately cleanly.

Vapours of various kinds were sent into a glass experimental tube a yard in length, and about three inches in diameter. As a general rule, the vapours were perfectly transparent; the tube when they were present appearing as empty as when they were absent. In two or three cases, however, a faint cloudiness showed itself within the tube. This caused me a momentary anxiety, for I did not know how far, in describing my previous experiments, actions might have been ascribed to pure cloudless vapour which were really due to those newly-observed nebulæ. Intermittent discomfort, however, is the normal feeling of the investigator; for it drives him to closer scrutiny, to greater accuracy, and often, as a consequence, to new discovery. It was soon found that the nebulæ revealed by the beam were also *generated* by the beam, and the observation opened a new door into that region inaccessible to sense, which embraces so much of the intellectual life of the physical investigator.

What *are* those vapours of which we have been speaking? They are aggregates of *molecules*, or small masses of matter, and every molecule is itself an aggregate of smaller parts called *atoms*. A molecule of aqueous vapour, for example, consists of two atoms of hydrogen and one of oxygen. A molecule of ammonia consists of three atoms of hydrogen and one of nitrogen, and so of other substances. Thus the molecules themselves inconceivably small, are made up of distinct parts still smaller. When, therefore, a compound vapour is spoken of, the corresponding mental image is an aggregate of molecules separated from each other, though still exceedingly near, each of these being composed of a group of atoms still nearer to each other. So much for the *matter* which enters into our conception of a vapour.* To this must now be added the idea of *motion*. The molecules have motions of their own *as wholes;* their constituent atoms have also motions of their own, which are executed independently of those of the molecules; just as the various movements on the earth's surface are executed independently of the orbital revolution of our planet.

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* This "dirt" consists in great part of organic germs, of the functions of which in the animal economy we are as yet ignorant.

The vapour molecules are kept asunder by forces which, virtually or actually, are forces of repulsion. Between these elastic forces and the atmospheric pressure under which the vapour exists, equilibrium is established as soon as the proper distances between the molecules have been assumed. If, after this, the molecules be urged nearer to each other by a momentary force, they recoil as soon as the force is expended. If by the exercise of a similar force they be separated more widely, when the force ceases to act they again approach each other. The case is different as regards the constituent atoms.

And here let me remark that we are now upon the very outmost verge of molecular physics; and that I am attempting to familiarize your minds with conceptions which have not yet obtained universal currency even among chemists; which many chemists, moreover, might deem untenable. But, tenable or untenable, it is of the highest scientific importance to discuss them. Let us, then, look mentally at our atoms grouped together to form a molecule. Every atom is held apart from its neighbours by a force of repulsion; why, then, do not the mutually repellent members of this group part company? The molecules *do* separate from each other when the external pressure is lessened or removed, but the atoms do not. The reason of this stability is that *two* forces, the one attractive and the other repulsive, are in operation between every two atoms; and the position of every atom—its distance from its fellows—is determined by the equilibration of these two forces. If the atoms come too near, repulsion predominates and drives them apart; if too distant, attraction predominates and draws them together. The point at which attraction and repulsion are equal to each other is the atom's *position of equilibrium.*

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* Newton seemed to consider that the molecules might be rendered visible by microscopes; but of the atoms he appears to have entertained a different opinion. He finely remarks:—"It seems impossible to see the more secret and noble works of nature within the corpuscles, by reason of their transparency." (Herschel, "On Light," Art. 1145.)

If not absolutely cold—and there is no such thing as absolute coldness in our corner of nature—the atoms are always in a state of vibration, their vibrations being executed to and fro *across their positions of equilibrium.*

Into a vapour thus constituted, we have now to pour a beam of light. But what, in the first instance, *is* a beam of light? It is a train of innumerable waves, excited in, and propagated through, an almost infinitely attenuated and elastic medium, which fills all space, and which we name the Æther. These waves of light are not all of the same size: some of them are much longer and higher than others. Now the short waves and the long ones move with the same rapidity through space, just as short and long waves of sound travel with the same rapidity through air. Hence the shorter waves must follow each other in quicker succession than the longer ones. The different rapidities with which the waves of light impinge upon the retina, or optic nerve, give rise in consciousness to differences of *colour.* There are however, numberless waves emitted by the sun and other luminous bodies which reach the retina, but which are incompetent to excite the sensation of light. If the lengths of the waves exceed a certain limit, or if they fall short of a certain other limit, they cannot generate vision. And it is to be particularly borne in mind that the capacity to produce *light* does not depend so much on the *strength* of the waves, as on their *periods of recurrence.* I have often permitted waves to enter my own eye, of a power which, if differently distributed, would have instantly and utterly ruined the optic nerve, but which failed to produce any impression whatever upon consciousness, because their periods were not those demanded by the retina.

The elements of all the conceptions with which we shall have subsequently to deal are now in your possession. And you will observe that though we are speaking of things which lie entirely beyond the range of the senses, the conceptions are as truly *mechanical* as they would be if we were dealing with ordinary masses of matter, and with waves of sensible magnitude. I do not think that any really scientific mind at the present day will be disposed to draw a substantial distinction between chemical and mechanical phenomena. They differ from each other as regards the magnitude of the masses involved; but in this sense the phenomena of astronomy differ, also, from those of ordinary mechanics. The main bent of the natural philosophy of a future age will probably be to chasten into order, by subjecting it to mechanical laws, the existing chaos of chemical phenomena.

Whether we see rightly or wrongly—whether our intellection be real or imaginary—it is of the utmost importance in science to aim at perfect clearness in the description of all that comes, or seems to come, within the range of the intellect. For if we are right, clearness of utterance forwards the cause of right; while if we are wrong, it ensures the speedy correction of error. In this spirit, and with the determination at all events to speak plainly, let us deal with our conceptions of æther waves and molecules. Supposing a wave, or a train of waves, to impinge upon a molecule so as to urge all its parts with the same motion, the molecule would move bodily as a whole, but because they are animated by a *common motion* there would be no tendency of its constituent atoms to separate from each other. *Differential motions* among the atoms themselves would be necessary to effect a separation, and if such motions be not introduced by the shock of the waves, there is no mechanical ground for the decomposition of the molecule.

It is, however, difficult to conceive the shock of a wave, or a train of waves, so distributed among the atoms as to cause no strain amongst them. For atoms are of different weights, probably of different sizes; at all events it is almost certain that the ratio of the mass of the atom to the surface it presents to the action of the waves is different in different cases. If this be so, and I think the probabilities are immensely in favour of its being so, then every wave which passes over a molecule tends to decompose it—tends to carry away from their weightier and more sluggish companions those atoms which, in relation to their mass, present the largest resisting surfaces to the motion of the waves. The case may be illustrated by reference to a man standing on the deck of a ship. As long as both of them share equally the motions of the wind or of the sea, there is no tendency to separation. In chemical language, they are in a state of combination. But a wave passing over it finds the ship less rapid in yielding to its motion than the man; the man is consequently carried away, and we have what may be regarded as decomposition.

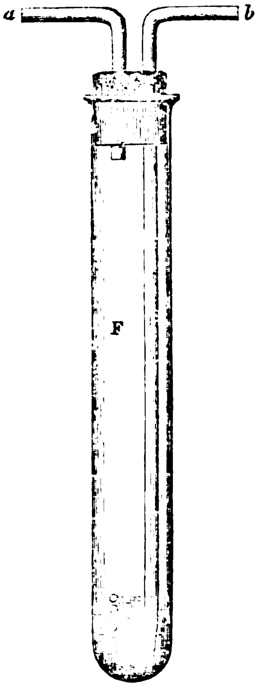
Thus the conception of the decomposition of compound molecules by the waves of æther comes to us recommended by *à priori* probability. But a closer examination of the question compels us to supplement, if not materially to qualify, this conception. It is a most remarkable fact, that the waves which have thus far been found most effectual in shaking asunder the atoms of compound molecules are those of least mechanical power. *Billows*, to use a strong comparison, are incompetent to produce effects which are readily produced by *ripples*. It is, for example, the violet and ultra-violet rays of the sun that are most effectual in producing these chemical decompositions; and, compared with the red and ultra-red solar rays, the energy of these "chemical rays" is infinitesimal. This energy would probably in some cases have to be multiplied by millions to bring it up to that of the ultra-red rays; and still the latter are powerless where the smaller waves are potent. We here observe a remarkable similarity between the behaviour of chemical molecules and that of the human retina. The energy transmitted to the eye from a candle-flame half-a-mile distant is more than sufficient to inform consciousness; while waves of a different period, possessing twenty thousand million times this energy, have been suffered to impinge upon my own retina, with an absolute unconsciousness of any effect whatever—mechanical, physiological, chemical, or thermal.

Whence, then, the power of these smaller waves to unlock the bonds of chemical union? If it be not a result of their strength, it must be, as in the case of vision, a result of their periods of recurrence. But how are we to figure this action? I should say thus: the shock of a single wave produces no more than an infinitesimal effect upon an atom or a molecule. To produce a larger effect, the motion must *accumulate*, and for wave-impulses to accumulate, they must arrive in periods identical with the periods of vibration of the atoms on which they impinge. In this case each successive wave finds the atom in a position which enables that wave to add its shock to the sum of the shocks of its predecessors. The effect is mechanically the same as that due to the timed impulses of a boy upon a swing. The single tick of a clock has no appreciable effect upon the unvibrating and equally long pendulum of a distant clock; but a succession of ticks, each of which adds, at the proper moment, its infinitesimal push to the sum of the pushes preceding it, will, as a matter of fact, set the second clock going. So likewise a single puff of air against the prong of a heavy tuning-fork produces no sensible motion, and, consequently, no audible sound; but a succession of puffs, which follow each other in periods identical with the tuning-fork's period of vibration, will render the fork sonorous. I think the chemical action of light is to be regarded in this way. Fact and reason point to the conclusion that it is the heaping up of motion on the atoms, in consequence of their synchronism with the shorter waves, that causes them to part company. This I take to be the mechanical cause of these decompositions which are effected by the waves of æther.

And now let us return to that faint cloudiness, already mentioned, from which, as from a germ, these considerations and speculations have sprung. It has been long known that light effected the decomposition of a certain number of bodies. The transparent iodide of ethyl, or of methyl, for example, becomes brown and opaque on exposure to light, through the discharge of its iodine. The art of photography is founded on the chemical actions of light; so that it is well known that the effects for which the foregoing theoretic considerations would have prepared us, are not only probable, but actual.

But the method employed in the experiments in which the cloudiness above referred to was observed, and which consists simply in offering the *vapours* of volatile substances to the action of light, enables us not only to give such experiments a beautiful form, but also to give a vast extension to the operations of light, or rather of radiant force, as a chemical agent. It also enables us to illustrate in our laboratories actions which have been hitherto performed only in the laboratory of nature. A few of these actions of a representative character I have now to bring before you; and, in doing so, I will take advantage of the fact that, in a great number of cases, one or more of the substances into which the waves of light break up compound molecules are comparatively *involatile*. These products of decomposition require a greater heat than is required by the vapours from which they are derived to keep them in the gaseous form; and hence, if the space in which these new bodies are liberated be of the proper temperature, they will not remain in the vaporous condition, but will precipitate themselves as liquid particles, thus forming visible clouds upon the beam to the action of which they owe their existence.

We will now commence our illustrative experiments. I hold in my hand a little flask, F, which is stopped by a cork, pierced in two places. Through one orifice passes a narrow glass tube, *a*, which terminates immediately under the cork; through the other orifice passes a similar tube, *b*, descending to the bottom of the little flask, which is filled to a height of about an inch with a transparent liquid. The name of this liquid is *nitrate of amyl*, in every molecule of which we have 5 atoms of carbon, 11 of hydrogen, 1 of nitrogen, and 2 of oxygen. Upon this group the waves of our electric light will be immediately let loose. The large horizontal tube that you see before you is what I have called an "experimental tube;" it is connected with our small flask, a stop-cock, however, intervening between them, by means of which the passage between the flask and the experimental tube can be opened or closed at pleasure. The other tube, passing through the cork of the flask and descending into the liquid, is connected with a U-shaped vessel, filled with fragments of clean glass, covered with sulphuric acid. In front of the U-shaped vessel is a narrow tube stuffed with cotton-wool. At one end of the experimental tube is our electric lamp; and here, finally, is an air-pump, by by means of which the tube has been exhausted. We are now ready for experiment.



Opening the cock cautiously, the air of the room passes, in the first place, through the cotton-wool, which holds back the numberless organic germs and inorganic dust-particles floating in the atmosphere. The air, thus cleansed, passes into the U-shaped vessel, where it is *dried* by the sulphuric acid. It then descends through the narrow tube to the bottom of the little flask, and escapes there through a small orifice into the liquid. Through this it bubbles, loading itself to some extent with the nitrite of amyl vapour, and then the air and vapour enter the experimental tube together.

The closest scrutiny would now fail to discover anything within this tube; it is, to all appearance, absolutely empty. The air and the vapour are both invisible. We will permit the electric beam to play upon this vapour. The lens of the lamp is so situated as to render the beam slightly convergent, the focus being formed in the vapour at about the middle of the tube. You will notice that the tube remains dark for a moment after the turning on of the beam; but the chemical action will be so rapid that attention is requisite to mark this interval of darkness. I ignite the lamp; the tube for a moment seems empty; but suddenly the beam darts through a luminous white cloud, which has banished the preceding darkness. It has, in fact, shaken asunder the molecules of the nitrite of amyl, and brought down upon itself a shower of liquid particles which cause it to flash forth in your presence like a solid luminous spear. It is worth while to mark how this experiment illustrates the fact, that however intense a luminous beam may be, it remains invisible unless it has something to shine upon. *Space*, though traversed by the rays from all suns and all stars, is itself unseen. Not even the æther which fills space, and whose motions are the light of the universe, is itself visible.

You notice that the end of the experimental tube most distant from the lamp is free from cloud. Now the nitrite of amyl vapour is there also, but it is unaffected by the powerful beam passing through it. Let us make the transmitted beam more concentrated by receiving it on a concave silver mirror, and causing it to return by reflection into the tube. It is still powerless. Though a cone of light of extraordinary intensity now traverses the vapour, no precipitation occurs, no trace of cloud is formed. Why? Because the very small portion of the beam competent to decompose the vapour is quite exhausted by its work in the frontal portions of the tube. The great body of the light which remains, after this sifting out of the few effectual rays, has no power over the molecules of nitrite of amyl. We have here, strikingly illustrated, what has been already stated regarding the influence of *period*, as contrasted with that of *strength*. For the portion of the beam which is here ineffectual has probably more than a million times the absolute energy of the effectual portion. It is energy specially related to the atoms that we here need, which specially related energy being possessed by the feeble waves, invests them with their extraordinary power. When the experimental tube is reversed so as to bring the undecomposed vapours under the action of the *unsifted* beam, you have instantly this fine luminous cloud precipitated.

The light of the sun also effects the decomposition of the nitrite of amyl vapour. A small room in the Royal Institution, into which the sun shone, was partially darkened, the light being permitted to enter through an open portion of the window-shutter. In the track of the beam was placed a large plano-convex lens, which formed a fine convergent cone in the dust of the room behind it. The experimental tube was filled in the laboratory, covered with a black cloth, and carried into the partially darkened room. On thrusting one end of the tube into the cone of rays behind the lens, precipitation within the cone was copious and immediate. The vapour at the distant end of the tube was shielded by that in front; but on reversing the tube, a second and similar splendid cone was precipitated.

Now let us pause for a moment and glance at the ground over which we have passed. We have defined a vapour as an aggregate of molecules mutually repellent, but hindered from indefinitely retreating from each other by an external pressure. We have defined a molecule as an aggregate of atoms maintained in positions of equilibrium by the equalized action of two opposing forces, and always oscillitating to and fro across those positions. We have defined a beam of light as a train of innumerable waves, and have illustrated their chemical action. We have learned that it is not the magnitude or power of the waves, so much as their periods of recurrence, that renders them effectual as chemical agents. We have also seen how the luminous beam is sifted by the vapour which it decomposes, and deprived of those rays which are competent to effect the decomposition. The effects, moreover, obtained with the electric beam are also produced by the beams of the sun.

And here I would ask you to make familiar to your minds the idea that no chemical action can be produced by a ray that does not involve the destruction of the ray. But the term "ray" is unsatisfactory to us at present, when our desire is to abolish all vagueness, and to affix a definite physical significance to each of our terms. Abandoning the term ray as loose and indefinite, we have to fix our thoughts upon the *waves* of light; and to render clear to our minds that those waves which produce chemical action do so by delivering up their own motion to the molecules which they decompose. We have here forestalled to some extent a question of great importance in molecular physics, which, however, is worthy of being fixed more definitely in your mind; it is this: When the waves of æther are intercepted by a compound vapour, is the motion of the waves transferred to the molecules of the vapour, or to the atoms of the molecules? We have thus far leaned to the conclusion that the motion is communicated to the atoms; for if not to these individually, why should they be shaken asunder? The question, however, is capable of, and is worthy of, another test, the bearing and significance of which you will immediately appreciate.

As already explained, the molecules are held in their positions of equilibrium by their mutual repulsion on the one side, and by an external pressure on the other. Their rate of vibration, if they vibrate at all, must depend upon the elastic force which they mutually exert. If this force be changed, the rate of vibration must change along with it; and after the change the molecules could no longer absorb the waves which they absorbed prior to the change. Now the elastic force between molecule and molecule is utterly altered when a vapour passes to the liquid state. Hence, if the liquid absorbs waves of the same period as its vapour, it is a proof that the absorption is not effected by the molecules. Let us be perfectly clear on this important point.

Those waves are absorbed whose vibrations synchronize with those of the molecules or atoms on which they impinge; a principle which is sometimes expressed by saying that bodies radiate and absorb the same rays. This great law, as you know, is the foundation of spectrum-analysis; it enabled Kirchhoff to explain the lines of Frauenhofer, and to determine the chemical composition of the atmosphere of the sun. If then, after such a change as that involved in the passage of a vapour to the liquid state, the same waves are absorbed as were absorbed prior to the passage, it is a proof that the molecules, which must have utterly changed *their* periods, cannot be the seat of the absorption; and we are driven to conclude that it is to the *atoms*, whose rates of vibration are unchanged by the change of aggregation, that the wave-motion is transferred. If experiment should prove this identity of action on the part of a vapour and its liquid, it would establish in a new and striking manner the conclusion to which we have previously leaned.

We will now resort to the experimental test. In front of this experimental tube, which contains a quantity of the nitrite of amyl vapour, is placed a glass cell a quarter of an inch in thickness, filled with the liquid nitrite of amyl. I send the electric beam first through the liquid and then through its vapour. The luminous power of this beam is very great but it can make no impression upon the vapour. The liquid has robbed it completely of its effective waves. I remove the liquid; chemical action immediately commences, and in a moment we have the apparently empty tube filled with this bright cloud, precipitated by one portion of the beam, and illuminated by another. I re-introduce the liquid: the chemical action instantly ceases. I again remove the liquid, and the action commences once more. Thus we uncover in part the secrets of this world of molecules and atoms.

Instead of employing air as the vehicle by which the vapour is carried into the experimental tube, we may employ oxygen, hydrogen, or nitrogen. With hydrogen curious effects are observed, due to the sinking of the clouds through the extremely light gas in which they float. They illustrate, without proving, the argument of those who say that the clouds of our own atmosphere could not float if the cloud particles were not little bladders, instead of full spheres. Before you is a tube filled with the nitrite of amyl vapour, which has been carried into the tube by hydrogen gas. On sending the beam through the tube a delicate bluish-white cloud is precipitated. A few strokes of the pump clear the tube of this cloud, but leave a residue of vapour behind. Again turning in the beam we have a second cloud, more delicate than the first, precipitated. This may be done half-a-dozen times in succession. A residue of vapour will still linger in the tube sufficient to yield a cloud of exquisite delicacy, both as regards colour and texture.

Besides the nitrite of amyl a great number of other substances might be employed, which, like the nitrite, have been hitherto not known to be chemically susceptible to light. But I confine myself at present to this representative case. One point, however, in addition I wish to illustrate, chiefly because the effect is the same in kind as one of great importance in nature. In our atmosphere you know floats carbonic acid gas, which furnishes food to the vegetable world. But this food could not be consumed by plants and vegetables without the intervention of the sun's rays. And yet, as far as we know, these rays are powerless upon the free carbonic acid of our atmosphere. The sun can only decompose the gas when it is drunk in by the leaves of plants. In the leaves it is in close proximity with substances ready to take advantage of the loosening of the molecules of the carbonic acid by the waves of light. Incipient disunion being introduced by the solar rays, the carbon of the gas is seized upon by the leaf and appropriated, while the oxygen is discharged into the atmosphere.

The experimental tube now before you contains a quantity of a different vapour from that which we have hitherto employed. The liquid from which this vapour is derived is called the nitrite of butyl. On sending the electric beam through the vapour, which has been carried in by air, the chemical action is scarcely sensible. I add to the vapour a quantity of air which has been permitted to bubble through hydrochloric acid. When the beam is now turned on, so rapid is the action and so dense the clouds precipitated, that you could hardly by an effort of attention observe the dark interval which preceded the precipitation of the cloud. This enormous augmentation of the action is due to the presence of the hydrochloric acid. Like the chlorophyl in the leaves of plants, it takes advantage of the loosening of the molecules of nitrite of butyl by the waves of the electric light.

In these experiments we have employed a luminous beam for two different purposes. A small portion of it has been devoted to the decomposition of our vapours, while the great body of the light has served to render luminous the clouds resulting from the decomposition. It is possible to impart to these clouds any required degree of tenuity, for it is in our power to limit at pleasure the amount of vapour in our experimental tube. When the quantity is duly limited, the precipitated particles are at first inconceivably small, defying the highest microscopic power to bring them within the range of vision. Probably their diameters might then be expressed in millionths of an inch. They grow gradually, and as they augment in size, throw from them, by reflexion, a continually increasing quantity of wave-motion, until, finally, the cloud which they form becomes so luminous as to fill this theatre with light. During the growth of the particles the most splendid iridescences are often exhibited. Such I have sometimes seen with delight and wonder in the atmosphere of the Alps, but never anything so gorgeous as those which our laboratory experiments reveal. It is not, however, with the iridescences, however beautiful they may be, that we have now to occupy our thoughts, but with other effects which bear upon the two great standing enigmas of meteorology—the colour of the sky and the polarization of its light.

And here let me briefly say that, were it not for the stimulus imparted to me by the private correspondence of a celebrated man, I should not have entered upon the investigation of these subjects so soon. In reference to the effects of light which you have just witnessed, Sir John Herschel wrote to me thus:—"It is a class of relations eminently calculated to set one thinking, and it seems to have had that effect upon you to excellent purpose. I am glad it has brought you into contact with the blue colour of the sky; still more so if it should lead you to any satisfactory explanation of the polarization of sky-light." The letter went on to treat of "this mysterious and beautiful phenomenon" in a manner which excited in me the strong desire to throw, if possible, some certain light upon a question regarding which the most divergent opinions and speculations were afloat among our most eminent scientific men.

First, then, with regard to the colour of the sky; how is it produced, and can we not reproduce it? This colour has not the same origin as that of ordinary colouring matter, in which certain portions of the white solar light are extinguished, the colour of the substances being that of the portion which remains. A violet is blue because its molecular texture enables it to quench the green, yellow, and red constituents of white light, and to allow the blue free transmission. A geranium is red because its molecular texture is such as quenches all rays except the red. Such colours are called colours of absorption; but the hue of the sky is not of this character. The blue light of the sky is all *reflected* light, and were there nothing in our atmosphere competent to reflect the solar rays we should see no blue firmament, but should look into the darkness of infinite space. The reflection of the blue is effected by perfectly colourless particles. Smallness of size alone is requisite to ensure the selection and reflexion of this colour. Of all the visual waves emitted by the sun, the shortest and smallest are those which correspond to the colour blue. On such waves small particles have more power than upon large ones, hence the predominance of blue colour in all light reflected from exceedingly small particles. The crimson glow of the Alps in the evening and in the morning is due, on the other hand, to *transmitted* light; that is to say, to light which in its passage through great atmospheric distances has its blue constituents sifted out of it by repeated reflexion.

It is possible, as stated, by duly regulating the quantity of vapour, to make our precipitated particles grow from an infinitesimal and altogether ultra-microscopic size to masses of sensible magnitude; and by means of these particles, in a certain stage of their growth, we can produce a blue which shall rival, if it does not transcend, that of the deepest and purest Italian sky. Let this point be in the first place established. Associated with our experimental tube is a barometer, the mercurial column of which now indicates that the tube is exhausted. Into the tube I introduce a quantity of the mixed air and nitrite of butyl vapour sufficient to depress the mercurial column one-twentieth of an inch; that is to say, the air and vapour together exert a pressure of one six-hundredth of an atmosphere. I now add a quantity of air and hydrochloric acid sufficient to depress the mercury half-an-inch further, and into this compound and highly attenuated atmosphere I discharge the beam of the electric light. The effect is slow; but gradually within the tube arises this splendid azure, which strengthens for a time, reaches a maximum of depth and purity, and then, as the particles grow larger, passes into whitish blue. This experiment is representative, and it illustrates a general principle. Various other colourless substances of the most diverse properties, optical and chemical, might be employed for this experiment. The *incipient cloud* in every case would exhibit this superb blue; thus proving to demonstration that particles of infinitesimal size, without any colour of their own, and irrespective of those optical properties exhibited by the substance in a massive state, are competent to produce the colour of the sky.

But there is another subject connected with our firmament, of a more subtle and recondite character than even its colour. I mean that "mysterious and beautiful phenomenon,"* the polarization of the light of the sky. The polarity of a magnet consists in its *two-endedness*, both ends, or poles, acting in opposite ways. Polar forces, as most of you know, are those in which the duality of attraction and repulsion is manifested. And a kind of *two-sidedness*—noticed by Huygens, commented on by Newton, and discovered by a French philosopher, named Malus, in a beam of light which had been reflected from one of the windows of the Luxembourg Palace in Paris—receives the name of *polarization*. We must now, however, attach a distinctness to the idea of a polarized beam, which its discoverers were not able to attach to it. For in their day men's thoughts were not sufficiently ripe, nor optical theory sufficiently advanced, to seize upon or express the physical meaning of polarization. When a gun is fired, the explosion is propagated as a wave through the air, The shells of air, if I may use the term, surrounding the centre of concussion, are successively thrown into motion, each shell yielding up its motion to that in advance of it, and returning to its position of equilibrium. Thus, while the *wave* travels through long distances, each individual particle of air concerned in its transmission performs merely a small excursion to and fro.† In the case of sound, the vibration of the air-particles are executed *in* the direction in which the sound travels. They are therefore called *longitudinal* vibrations. In the case of light, on the contrary, the vibrations are *transversal;* that is to say, the individual particles of æther move to and fro *across* the direction in which the light is propagated. In this respect waves of light resemble ordinary water-waves, more than waves of sound. In the case of an *ordinary* beam of light, the vibrations of the æther particles are executed *in every direction* perpendicular to it; but let the beam impinge obliquely, upon a plane glass surface, as in the case of Malus, the portion reflected will no longer have its particles vibrating in all directions round it. By the act of reflexion, *if it occur at the proper angle*, the vibrations are all confined to a single plane, and light thus circumstanced is called *plane polarized light.*

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* Herschel's 'Meteorology,' Art. 233.

† 'Lectures on Sound,' p. 3. (Longmans.)

A beam of light passing through ordinary glass executes its vibrations within the substance exactly as it would do in air, or in æther-filled space. Not so when it passes through many transparent crystals. For these have also their two-sidedness, the arrangement of their particles being such as to tolerate vibrations only in certain definite directions. There is the well-known crystal tourmaline, which shows a marked hostility to all vibrations executed at right angles to the axis of the crystal. It speedily extinguishes such vibrations, while those executed parallel to the axis are freely propagated. The consequence is, that a beam of light, after it has passed through any thickness of this crystal, emerges from it polarized. So also as regards the beautiful crystal known as Iceland spar, or as double doubly refracting spar. In one direction, but in one only, it shows the neutrality of glass; in all other directions it splits the beam of light passing through it into two distinct halves, both of which are perfectly polarized, their vibrations being executed in two planes, at right angles to each other.

It is possible by a suitable contrivance to get rid of one of the two polarized beams into which Iceland spar divides an ordinary beam of light. This was done so ingeniously and effectively by a man named Nicol, that the Iceland spar, cut in his fashion, is now universally known as Nicol's prism. Such a prism can polarize a beam of light; and if the beam, before it impinges on the prism, be already polarized, in one position of the prism it is stopped, while in another position it is transmitted. Our way is now, to some extent, cleared towards an examination of the light of the sky. Looking at various points of the blue firmament through a Nicol's prism, and turning the prism round its axis, we soon notice variations of the brightness of the sky. In certain positions of the spar, and from certain points of the firmament, the light appears to be wholly transmitted; while, looking at the same points, it is only necessary to turn the prism round its axis through an angle of ninety degrees to materially diminish the intensity of the light. On close scrutiny it is found that the difference produced by the rotation of the prism is greatest when the sky is regarded in a direction at right angles to that of the solar rays through the air. Let me describe a few actual observations made some days ago on Primrose Hill. The sun was near setting, and a few scattered neutral-tint clouds, which failed to catch the dying light, were floating in the air. When these were looked at across the track of the solar beams, it was possible by turning the Nicol round, to see them either as white clouds on a dark ground, or as dark clouds on a bright ground.* In some of its positions the sky-light was in great part quenched by the Nicol, and then the clouds, projected against the darkness of space, appeared white. Turning the Nicol ninety degrees round its axis, the brightness of the sky was restored, and then the clouds became dark through contrast with this brightness.

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* I was not aware when these words were written that this observation was made by the indefatigable Brewster.

Experiments of this kind prove that the blue light sent to us by the firmament is polarized, and that the direction of most perfect polarization is perpendicular to the solar rays. Were the heavenly azure like the ordinary light of the sun, the turning of the prism would have no effect upon it; it would be transmitted equally during the entire rotation of the prism. The light of the sky is in great part quenched, because it is in great part polarized.

When a luminous beam impinges at the proper angle on a plane glass surface it is polarized by reflexion. It is polarized, *in part*, by all oblique reflexions; but at one particular angle, the reflected light is *perfectly polarized.* An exceedingly beautiful and simple law, discovered by Sir David Brewster, enables us readily to find *the polarizing angle* of any substance whose refractive index is known. This law was discovered experimentally by Brewster; but the Wave Theory of light renders a complete reason for the law. A geometrical image of it is thus given. When a beam of light impinges obliquely upon a plate of glass it is in part reflected and in part refracted. At one particular incidence the reflected and the refracted portions of the beam are at right angles to each other. The angle of incidence is *then* the polarizing angle. It varies with the refractive index of the substance; being for water 52½, for glass 57½, and for diamond 68 degrees.

And now we are prepared to comprehend the difficulties which have beset the question before us. It has been already stated that in order to obtain the most perfect polarization of the firmamental light, the sky must be regarded in a direction at right angles to the solar beams. This is sometimes expressed by saying that the place of maximum polarization is at an angular distance of 90° from the sun. This angle, enclosed as it is between the direct and reflected rays, comprises both the angles of incidence and reflexion. Hence the angle of incidence, which corresponds to the maximum polarization of the sky, is half of 90°, or 45°. This is the atmospheric polarizing angle, and the question is, what known substance possesses an index of refraction to correspond with this polarizing angle? If we knew this substance, we might be tempted to conclude that particles of it, scattered in the atmosphere, produce the polarization of the sky. "Were the angle of maximum polarization," says Sir John Herschel, "76° (instead of 90°), we should look to *water*, or ice, as the reflecting body, however inconceivable the existence in a cloudless atmosphere, and a hot summer day, of unevaporated particles of water." But a polarizing angle of 45° corresponds to a refractive index of 1; this means that there is no refraction at all, in which case we ought to have no reflexion. Brewster and others came to the conclusion that the reflexion was from the particles of air themselves. Dr. Rubenson, of Upsala, made the angle enclosed between the direct and reflected beams 90° 2′; "the half of which," says Mr. Buchan, in his excellent little 'Handy Book of Meteorology,' "is so near the polarizing angle of air, as to leave no doubt that the light of the sky, as first stated by Brewster, is polarized by reflexion from the particles of air." It is difficult to affix a physical meaning to this conclusion. If light be reflected, it must be at the common limiting surface of two media of different refrangibility. But to satisfy the law of Brewster, as Sir John Herschel remarks, "the reflexion would have to be made *in* air *upon* air!" "The more the subject is considered," adds the celebrated philosopher last named, "the more it will be found beset with difficulties, and its explanation, when arrived at, will probably be found to carry with it that of the blue colour of the sky itself."

If you doubt the wisdom, acknowledge, at all events, the faith in your capacity which has caused me to bring a subject so entangled before you. I believe, however, that even the intellect which draws its strength and its associations from a totally different source, may have its interest excited in subjects like the present, dark and difficult though they be. I do not expect that you will all grasp the details of this discussion; but I think that everybody present will see the extremely important part hitherto played by the law of Brewster in speculations as to the colour and polarization of the sky. This law leads to the extraordinary conclusion already announced, that the reflexion takes place at the limiting surface of two media of the same refrangibility, where reflexion could no more occur than it could occur in the very heart of an optically homogeneous medium.* I shall now seek to demonstrate in your presence, *firstly*, and in conformation of our former experiments, that sky-blue may be produced by exceedingly minute particles of any kind of matter; *secondly*, that polarization identical with that of the sky is produced by such particles; and *thirdly*, that matter in this fine state of division, where its particles are probably small in comparison with the height and span of a wave of light, releases itself completely from the law of Brewster; the direction of maximum polarization being absolutely independent of the polarizing angle as hitherto defined. Why this should be the case, the wave theory of light, to make itself complete, will have subsequently to explain.

Into this experimental tube, in the manner already described, I introduce a vapour which is decomposable by the waves of light. The mixed air and vapour are sufficient to depress the mercurial column one inch. I add to this mixture air, which has been permitted to bubble through dilute hydrochloric acid, until the column is depressed thirty inches: in other words, until the tube is full. And now I permit the electric beam to play upon the mixture. For some time nothing is seen. The chemical action is doubtless progressing, and condensation going on; but the condensing molecules have not yet coalesced to particles sufficiently large to reflect sensibly the waves of light. As before stated—and the statement rests upon an experimental basis—the particles here generated are at first so small that their diameters would probably have to be expressed in millionths of an inch; while to form each of these *particles* whole crowds of *molecules* are probably aggregated. Helped by such considerations, the intellectual vision plunges more profoundly into atomic nature, and shows us, among other things, how far we are from the realization of Newton's hope that the molecules might one day be seen by microscopes. While I am speaking, you observe this delicate blue colour forming and strengthening within the experimental tube. No sky-blue could exceed it in richness and purity; but the particles which produce this colour lie wholly beyond our microscopic range. A uniform colour is here developed, which has as little breach of continuity—which yields as little evidence of the particles concerned in its production—as that yielded by a body whose colour is due to true molecular absorption. This blue is at first as deep and dark as the sky seen from the highest Alpine peaks, and for the same reason. But it grows gradually brighter, still maintaining its blueness, until at length a whitish tinge mingles with the pure azure; announcing that the particles are now no longer of that infinitesimal size which reflects the shortest waves alone.*

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* I am here taking for granted that the polarizing angle of 45° established by observation is rigidly correct. With regard to the reflexion which accompanies atmospheric refraction, inasmuch as the rays are incident upon a convex surface, or upon a series of concentric convex surfaces, the reflected light is dispersed in space instead of reaching the eye of the observer. Such reflexion, moreover, even to an eye in space, would not account for the colour of the sky, nor probably for the quantity of its light.

The liquid here employed is the iodide of allyl,† but I might choose any one of a dozen substances here before me to produce the effect. You have seen what may be done with the nitrite of butyl. With nitrite of amyl, bisulphide of carbon, benzol, benzoic æther, &c. the same blue colour may be produced. In all cases where matter slowly passes from the molecular to the massive state, the transition is marked by the production of the blue. More than this:—you have seen me looking at the blue colour (I hardly like to call it a blue "cloud," its texture and properties are so different from ordinary clouds) through this bit of spar. This is a Nicol's prism, and I could wish one of them to be placed in the hands of each of you. Well, this blue that I have been regarding turns out to be, if I may use the expression, a bit of more perfect sky than the sky itself. When I look across the illuminating beam exactly as we look across the solar rays in the atmosphere, I obtain not only partial polarization, but *perfect* polarization. In one position of the Nicol the blue light seems to pass unimpeded to the eye; in the other it is absolutely cut off, the experimental tube being reduced to optical *emptiness*. Behind the experimental tube it is well to place a black surface, in order to prevent foreign light from troubling the eye. In one position of the Nicol this black surface is seen without softening or qualification; for the particles within the tube are themselves invisible, and the light which they reflect is quenched. If the light of the sky were polarized with the same perfection, on looking properly towards it through a Nicol we should meet, not the mild radiance of the firmament, but the unillumined blackness of space.

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* Possibly a photographic impression might be taken long before the blue becomes visible, for the ultra-blue rays are first reflected.

† For which I have to thank the obliging kindness of Dr. Maxwell Simpson, F.R.S.

The construction of the Nicol is such that it permits to pass through it vibrations which are executed in a certain determinate direction, and these only. All vibrations executed at right angles to this direction are completely stopped: while components only of those executed obliquely to it are transmitted. It is easy, therefore, to see that from the position in which the Nicol must be held to transmit or to quench the light of our incipient cloud, we can infer the direction of the vibrations of that light. You will be able to picture those vibrations without difficulty. Suppose a line drawn from any point of the "cloud" perpendicular to the illuminating beam. The particles of æther along that line, which carry the light from the cloud to the eye, vibrate in a direction perpendicular both to the line and to the beam. And if any number of lines be drawn in the same way from the cloud, like the spokes of a wheel, the particles of æther along all of them oscillate in the same manner. Wherefore, if a *plane surface* be imagined cutting the incipient cloud at right angles to its length, the perfectly polarized vibrations discharged laterally will all be parallel to this surface. This, in fact, is the plane of vibration of the polarized light. Or you may suppose a circle drawn round the experimental tube, and a series of strings attached to various points of this circle. If all the cords be stretched as perpendiculars to the experimental tube, and caused to wriggle by a series of jerks imparted at right angles both to them and to the tube, the motion of the particles of the strings will then represent those of the particles of æther. A distinct image of those vibrations is now, I hope, within the reach of every person here present.

Our incipient blue cloud is a virtual Nicol's prism, and, between it and the real Nicol, we can produce all the effects obtainable between the polarizer and analyzer of a polariscope. When, for example, a thin plate of selenite, which is crystallized sulphate of lime, is placed between the Nicol and the incipient cloud, we obtain the splendid chromatic phenomena of polarized light. The colour of the gypsum-plate, as many of you know, depends upon its thickness. If this be uniform, the colour is uniform. If, on the contrary, the plate be wedge-shaped, thickening gradually and uniformly from edge to back, we have brilliant bands of colour produced parallel to the edge of the wedge. Perhaps the best form of plate for experiments of this character is that now in my hand, which was prepared for me some years ago by a man of genius in his way, the late Mr. Darker of Lambeth. It consists of a plate of selenite thin at the centre, and gradually thickening towards the circumference. Placing this film between the Nicol and the cloud, we obtain, instead of a series of parallel bands, a system of splendidly coloured rings. The colours are most vivid when the incipient cloud is looked at perpendicularly. Precisely the same phenomena are observed when we look at the blue firmament in a direction perpendicular to the solar rays.

We have thus far illuminated our incipient cloud with ordinary light, and found the portion of this light reflected laterally from the cloud in all directions round it to be perfectly polarized. We will now examine the effects produced when the light which illuminates the cloud is itself polarized. In front of the electric lamp, and between it and the experimental tube, is placed this fine Nicol's prism, which is sufficiently large to embrace and to polarize the entire beam. The prism is now placed so that the plane of vibration of the light emergent from it, and falling upon the cloud, is vertical. How does the cloud behave towards this light? This formless aggregate of infinitesimal particles, without definite structure, shows the two-sidedness of the light in the most striking manner. It is absolutely incompetent to reflect upwards or downwards, while it freely discharges the light horizontally, right and left. I turn the polarizing Nicol so as to render the plane of vibration horizontal; the cloud now freely reflects the light vertically upwards and downwards, but it is absolutely incompetent to shed a ray horizontally to the right or left.

Fix your attention upon one of those reflecting particles. Figure it as a little sphere with the beam of the electric light impinging upon it. Let us call that diameter which coincides with the direction of the beam, the *axis* of the sphere; one of its *poles* would then be turned towards the light, and the other in the opposite direction. The equator of the little sphere would of course be midway between its poles. Now, conceive a parallel of latitude drawn upon the sphere at an angular distance of 45 degrees from the pole; that is to say, midway between the pole and the equator. Then what occurs with ordinary light is this: all the vibrations tangent to the little circle, which I have called a parallel of latitude, are reflected perfectly polarized; but all vibrations executed at right angles to the circle go unreflected through the little sphere. If, instead of ordinary light, we use polarized light, it is clear that at two opposite points of the little circle the vibrations are executed along the tangents, while at two other opposite points they are executed at right angles to the tangents. In the former case the particle *reflects* the light, in the latter it *transmits* the light unreflected. What is true of a single particle is true of all, and hence the inability of the incipient cloud formed of such particles to reflect light in two directions, while it freely reflects it in two others. The entire facts are now placed before you. The reflecting particle and the waves of æther are of course both beyond the range of the senses, but to the intellect the conceptions here introduced are just as easy as if, in illustration, I had pointed to the poles, equator, and parallel of latitude of an ordinary terrestrial globe.

Suppose the atmosphere of our planet to be surrounded by an envelope impervious to light, with an aperture on the sunward side, through which a solar beam could enter and cross our atmosphere. Surrounded on all sides by air not directly illuminated, the track of the sunlight would resemble that of the electric beam in a dark space filled with our incipient cloud. The course of the sunbeam would be *blue*, and it would discharge laterally, in all directions round it, light in precisely the same polarized condition as that discharged from the incipient cloud. In fact, the azure revealed by the sunbeam would be the azure of such a cloud. And if, instead of permitting the ordinary light of the sun to enter the aperture, a Nicol's prism were placed there, which should polarize the sunlight on its entrance into our atmosphere, the particles producing the colour of the sky would act precisely like those of our incipient cloud. In two directions we should have the solar light reflected; in two others unreflected. In fact, out of such a solitary beam, traversing the unilluminated air, we should be able to extract every effect shown by our incipient cloud. In the production of such clouds we virtually carry bits of the sky into our laboratories, and obtain with them all the effects obtainable in the open firmament of heaven.

And here, had not a sufficient strain been already imposed upon your minds, I might enter upon the description of a series of extraordinary effects observed when the particles of our incipient clouds are allowed to augment in size, so as to approach the condition of true cloudy matter. The selenite ring-system, already referred to, is a most delicate reagent for the detection of polarized light. When we look *normally*, or perpendicularly, at an incipient cloud, the colours of the rings are most vividly developed, a diminution of the colour being immediately apparent when the incipient cloud is regarded *obliquely*. But let us continue to look through the Nicol and selenite normally at the cloud: the particles augment in size, the cloud becomes coarser and whiter, the strength of the selenite colours becoming gradually feebler. At length the cloud ceases to discharge polarized light along the normal, and then the selenite colours entirely disappear. If *now* the cloud be regarded *obliquely*, the colours are restored, very vividly, if not with their first vividness and clearness. Thus the cloud that has ceased to discharge polarized light at right angles to the illuminating beam, pours out such light copiously in oblique directions. The direction of maximum polarization changes with the texture of the cloud.

But this is not all; and to understand, even partially, what remains, a word must be said regarding the appearance of the colours of our plate of selenite. If, as before stated, the plate be of uniform thickness, its hue in polarized light is uniform. Suppose, then, that by arranging the Nicol the colour of the plate is raised to its maximum brilliancy, and suppose the colour produced to be *green;* on turning the Nicol round its axis the green becomes fainter. When the angle of rotation amounts to 45 degrees the colour disappears; we then pass what may be called a neutral point, where the selenite behaves, not as a crystal, but as a bit of amorphous glass. Continuing the rotation, a colour reappears, but it is no longer green, but *red*. This attains its maximum at a distance of 45 degrees from the neutral point, or, in other words, at a distance of 90 degrees from the position which showed the green at its maximum. At a further distance of 45 degrees from the position of maximum red, the colour disappears a second time. We have there a second neutral point, beyond which the green comes again into view, attaining its maximum brilliancy at the end of a rotation of 180 degrees. By the rotation of the Nicol, therefore, through an angle of 90 degrees, we produce a colour *complementary* to that with which we started.

As may be inferred from this result, the selenite ring-system changes its character when the Nicol is turned. It is possible to have the centre of the circle dark, the surrounding rings being vividly coloured. The turning of the Nicol through an angle of 90 degrees renders the centre bright, while every point occupied by a certain colour in the first instance is occupied by the *complement* of that colour in the second. But what am I aiming at in these long preliminary statements? I want to be able to say, with full assurance of being understood by everybody present, that a cloud may so alter its texture as to produce upon light an effect equivalent to the rotation of the Nicol through 90 degrees. By curious internal actions, not here to be described, the cloud in our experimental tube sometimes divides itself into sections of different textures. Some sections are coarser than others, while it often happens that some are iridescent to the naked eye, and others not. Looking normally at such a cloud through the selenite and Nicol, it often happens that in passing from section to section the whole character of the ring-system is changed. You start with a section producing a *dark* centre and a corresponding system of rings; you pass to another section through a neutral point, and find in that section the centre *bright*, and at the same radial distances find each of the first rings displaced by one of the complementary colour. Sometimes as many as four such reversions occur in the cloud of an experimental tube a yard long. Now, the changes here indicated mean that in passing from section to section of the cloud the plane of vibration of the polarized light turns suddenly through an angle of 90 degrees; this change being entirely due to the different texture of the two parts of the cloud.

You will now be able to understand, as far as it is capable of being understood, a very beautiful effect which, under favourable circumstances, might be observed in our atmosphere. This experimental tube contains an inch of the iodide of allyl vapour, the remaining 29 inches necessary to fill the tube being air, which has bubbled through aqueous hydrochloric acid. Besides, therefore, the vapour of iodide of allyl, we have those of water and of acid within the tube.

The light has been acting on the mixture for some time, a beautiful incipient blue cloud being formed. As before stated, the "incipient cloud" is wholly different in texture and optical properties from an ordinary cloud; but it is possible to precipitate the aqueous vapour within this tube so as to cause it to form a cloud similar to the clouds of our atmosphere. This new and real cloud will be precipitated in the midst of the azure of the incipient cloud. An exhausted vessel of about one-third of the capacity of the experimental tube is now connected with the tube, the passage uniting both being closed by a stopcock. On opening this cock the mixed air and vapour will rush from the experimental tube into the empty vessel; and, in consequence of the chilling due to rarefaction, the vapour in the experimental tube will fall together as a true cloud. You are now prepared for the experiment. I first look at this blue colour, so as to obtain a vivid ring-system with a dark centre. Turning on the cock, the air is rarefied and the cloud precipitated. What is the result? Instantly the centre of the system of coloured rings becomes bright, and the whole series of colours corresponding to definite radial distances, complementary. While I continue to look at the cloud, it gradually melts away as an atmospheric cloud might do in the azure of heaven. And *there* is our azure also remaining behind. The coarser cloud seems drawn aside like a veil, the blue reappears, the first ring-system, with its dark centre and correspondingly coloured circles, being restored.

Thus patiently and bravely you have accompanied me over a piece of exceedingly difficult ground; and I think as a prudent guide, we ought to halt upon the eminence we have now attained. We might go higher, but the boulders begin here to be very rough. At a future day we shall, I doubt not, be able to overcome this difficulty, and to reach together a greater elevation.

[J. T.]

WEEKLY EVENING MEETINGS,

Friday, January 22, 29, 1869.

SIR HENRY HOLLAND, Bart. M.D. D.C.L. F.R.S. President, in the Chair.

January 22, PROFESSOR ALEXANDER HERSCHEL.

On the latest Eclipse of the Sun.

January 29, JOHN RUSKIN, Esq. M.R.I.

On the Flamboyant Architecture of the Valley of the Somme.

[No Abstracts received.]

GENERAL MONTHLY MEETING,

Monday, Feb. 1, 1869.

W. R. **Grove**, Esq. M.A. Q.C. F.R.S. Vice-President, in the Chair.

Edward Armitage, Esq. A.R.A.
Geoffrey Bevington, Esq.
Frederick Leighton, Esq.
The Master of Lindsay.
Frederick Nettlefold, Esq.
Frederick J. Toulmin, Esq.
R. O. White, Esq.
William Edward Wilson, Esq. and
Philip Wright, Esq.

were *elected* Members of the Royal Institution.

The special thanks of the Members were returned to Sir **Henry Holland**, Bart. the President, for his present of an original Spirit Thermometer of the Accademia del Cimento of Florence, 17th century, which he had received from M. G. Libri.

The special thanks of the Members were also returned for the following additions to "the Donation Fund for the Promotion of Experimental Researches":—

Alfred Davis, Esq. (3rd Donation)	£21	0
W. D. (3rd Donation)	5	5

The **Presents** received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

Actuaries, Institute of—Journal, No. 74. 4to. 1868.

Asiatic Society of Bengal—Journals, Nos. 147, 148. 8vo. 1868.

Asiatic Society, Royal—Journal. New Series. Vol. III. No. 2. 8vo. 1868.

Astronomical Society, Royal—Proceedings. Vol. XXIX. Nos. 1, 2. 8vo. 1868–9.
  Monthly Notices. Vol. XXVIII. Nos. 8, 9. 8vo. 1868.

Basel Natural Philosophy Society—Verhandlungen. Theil V. Heft I. 8vo. 1868.

Bavarian Academy of Science, Royal.—Sitzungsberichte, 1868. Band II. Heft 2. 8vo.

Beke, Dr. C. T. (the Author)—Confutation of Mr. Layard's Calumnies in the House of Commons. (K 95) 8vo. 1868.

British Museum, Trustees—Inscriptions in the Hieratic and Demotic Character. fol. 1868.
  Catalogue of Hemiptera Heteroptera. Part 3. 8vo. 1868.

Chemical Society—Journals for Dec. 1868; Jan. 1869.

Christiania University, Norway—M. Sars, Mémoires sur les Crinoïdes Vivants. 4to. 1868.
  Norse Meteorologisk Aarbog for 1867. 4to. 1868.
  Morkinskinna: (History of the Kings of Norway, 1035–1177.) Ed. C. R. Unger. 8vo. 1867.

Davis, Alfred, Esq. M.R.I.—Manasseh Ben Israel: The Conciliator, a Reconcilement of the apparent Contradictions in Holy Scripture. With Notes by E. H. Lindo. 2 vols. 8vo. 1842.

Editors—American Journal of Science and Arts. Nov. 1868. 8vo.
  Artizan for Dec. 1868; Jan. 1869. 4to.
  Athenæum for Dec. 1868; Jan. 1869. 4to.
  British Journal of Photography for Dec. 1868; Jan. 1869. 4to.
  Chemical News for Dec. 1868; Jan. 1869. 4to.
  Engineer for Dec. 1868; Jan. 1869. fol.
  Geological and Natural History Repertory. Dec. 1868; Jan. 1869. 8vo.
  Horological Journal for Dec. 1868; Jan. 1869. 8vo.
  Journal of Gas-Lighting for Dec. 1868; Jan. 1869. 4to.
  Mechanics' Magazine for Dec. 1868; Jan. 1869. 8vo.
  Pharmaceutical Journal for Dec. 1868; Jan. 1869. 8vo.
  Photographic News for Dec. 1868; Jan. 1869. 4to.
  Practical Mechanics' Journal for Dec. 1868; Jan. 1869. 4to.
  Revue des Cours Scientifiques et Littéraires. Dec. 1868; Jan. 1869. 4to.

Franklin Institute—Journal, No. 515. 8vo. 1868.

Genève, Société de Physique—Mémoires. Tome XIX. Partie 2. 4to. 1868.

Glasgow Philosophical Society—Proceedings. Vol. VI. No. 4. 8vo. 1867–8.

Jablonowski'sche Society, Leipsic—Preisschriften. XIII. 8vo. 1868.

Jones, H. Bence, M.D. F.R.S. Hon. Sec. R.I. (the Author)—Croonian Lectures on Matter and Force. 16to. 1868.

Latham, Alfred, Esq. M.R.I.—J. De Barros e Cunha "To-Day" (on State of Portugal). (K 96) 8vo. 1868.

Linnean Society—Transactions. Vol. XXVI. Part 2. 4to. 1868.
  Journal, Nos. 45, 48. 8vo. 1868.

Liverpool Literary and Philosophical Society—Proceedings. Nos. 20, 21, 22. 8vo. 1866–7.

Lords of the Committee of Council—Catalogue of Third Exhibition of National Portraits. 4to. 1868.

Mechanical Engineers' Institution, Birmingham—Proceedings, Jan.-April, 1868. 8vo.

Meteorological Society—Proceedings, No. 39. 8vo. 1868.

Moore, C. H. Esq. M.R.I.—On going to Sleep. 12mo. 1868.

Photographic Society—Journal, Nos. 200, 201. 8vo. 1868.

Royal Society of London—Proceedings, Nos. 106, 107. 8vo. 1868.
  Catalogue of Scientific Papers. 1800–63. Vol. II. 4to. 1868.

Saxon Society of Sciences, Royal—Abhandlungen. Band V. Nos. 4, 5. 8vo. 1868.
  Berichte: Philol. Hist. Classe. 1867, No. 2; 1868, No. 1. 8vo.

Statistical Society of London—Journal. Vol. XXXI. Part 4. 1868.

Symons, G. J. Esq. (the Author)—Symons' Monthly Meteorological Magazine, Dec. 1868; Jan. 1869. 8vo.

Vereins zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, Marz–Aug. 1868. 4to.

Victoria Institute—Transactions, No. 10. 8vo. 1866.

Winn, J. M. M.D. (the Author)—On the Nature and Treatment of Hereditary Disease. (K 96) 8vo. 1869.

WEEKLY EVENING MEETING,

Friday, February 5, 1869.

SIR HENRY HOLLAND, Bart. M.D. D.C.L. F.R.S. President, in the Chair.

JAMES FERGUSSON, Esq. F.R.S.

On Tree and Serpent Worship, as exemplified by some recently discovered Indian Monuments.

THE speaker introduced the subject by explaining the difficulties which arose in treating of it, partly in consequence of the reckless manner in which a certain class of antiquaries had theorized regarding Serpent-worship, but more because, as a result of this, all the better class of critics had been deterred from meddling with what had become the laughing-stock of sober-minded persons, in consequence of the absurdities which had been engrafted upon it. Except one work, by Böttiger, on the 'Baumkultus der Hellenen,' no serious work had been published in Germany, bearing on the subject; while in France nothing had appeared in elucidation of the worship of either the Serpent or of Trees.

The case was different in this country: a whole literature had sprung up, dating from the visit of King James I. to Stonehenge in company with his architect, Inigo Jones; and from their time, Dr. Stukeley, Colt Hoare, Geoffrey Higgins, Bathurst Deane, and many others, had published volume after volume on the subject. Almost all these works had, however, been based on a passage in the 29th book of Pliny's 'Natural History,' in which he related the formation of an "*Anguinum*" or serpent-egg, by an assembly of snakes on a certain day, adding that the egg was considered an important charm by the Druids. On this slender basis, Stonehenge, Avebury, and all the megalithic temples of Britain, were called Druidic, and Serpent-worship admitted as the established faith of our forefathers. It was in vain to hope to attack successfully such a castle in the air, unless some new and tangible evidence could be brought to bear on the subject. This, however, has now fortunately reached us from India, and the object of this evening's discourse is to explain its form and relevance.

The first monument bearing on the subject was the Temple of Nakhon Vat, in the centre of the now desolate country of Cambodia, which was discovered about ten years ago, almost accidentally, by a French traveller, M. Muhot. It is probably not too much to say, that, taken altogether, it is probably the most remarkable temple in Asia, being one of the largest, and is unsurpassed by any in the extent and the beauty of its form, and the marvellous elaboration of its sculptural details. On examination it was found that this temple was erected, by an Indian colony from Taxila, as late as the 13th century of the Christian era, and was dedicated wholly to the worship of the Serpent.

The next piece of evidence was brought to light even more accidentally. While looking for objects to cast for the Paris Exhibition of 1867, a large collection of sculptures in white marble were discovered buried under rubbish of all sorts in the stables of Fife House, then occupied as a temporary museum attached to the India Office. On examination, it was found that these had been sent home some twelve years ago, by Sir Walter Elliot, having been principally excavated by him from the Amravati Tope, a building of the 4th century, situated about 60 miles from the mouth of the Kistnah river in the Zillah Guntoor.

The building to which these marbles belonged was originally enclosed by a circular screen 195 feet in diameter, or exactly double the dimensions of the corresponding screen at Stonehenge, the height of the two circles being very nearly the same. Within this was a procession-path, 12 feet in width, and then an inner screen only 6 feet in height, but even more elaborately ornamented with carvings than the outer enclosure. The interior of the Tope inside these two circles was occupied by a number of buildings, all of which have been destroyed, and their materials used by a local Rajah in building the town of Amravati, at the end of the last century.

On examination, it was found that the Tope had been erected in the 4th century, and was in all essentials a Buddhist monument; but its sculptures proved that the worship of the seven-headed Naga, or Serpent-god, was nearly as important and as prevalent when it was erected as that of Buddha himself. Another circumstance, nearly as unexpected, was that the worship of the Tree was equal in dignity to that of the Serpent—the three forming a trinity for which we were by no means prepared.

The next piece of evidence which came to light was in the form of a series of photographs of the Sanchi Tope, near Bhopal, in Central India, made by Lieut. Waterhouse, and a still more interesting series of drawings of the sculptures of the same monument by Lieut.-Col. Maisey.

The sculptures of this monument are earlier than those of the Amravati Tope, and date from the first century of our era. In them Buddha himself never appears as an object of worship, though the monument is essentially Buddhist. The Serpent is worshipped, but only occasionally; but the Tree is the prevailing and prominent object of adoration.

The light thrown on the subject by the examination of these three typical examples was so distinct and clear, that many minor indications which had hitherto been overlooked were now found to bear directly on the subject; and the general result was to prove what had only before been suspected,* which was, that before the preaching of Gautama Buddha, or Sakya Muni, who died 543 B.C., the prevailing worship of the aboriginal tribes of India was Tree and Serpent worship; that the former was tolerated by Buddha—the latter abolished; but in later times, when the prophet's influence became weaker, that the two had cropped up again, and had, in later times, so obscured as nearly to obliterate the reforms he had introduced.

Mr. Fergusson then proceeded to point out what he believed to be the key to half the problems of Indian mythology or art: this was, that the country was now, and had in all historical times been, inhabited by two perfectly distinct and separate races of men. One aboriginal, so far as known, and of distinctly Turanian race; the other, Aryans, who migrated into India some 2000, or it may be 3000 years before the Christian era, and who, down at least to the 7th century B.C. completely dominated the aboriginal races.

The language of the Aryans was Sanscrit—their religion that of the Vedas; and it may be asserted, almost without limitation, that all the literature of India belongs to this great family of mankind; but like Aryans all over the world, they had no great feeling for art, and erected no permanent buildings.

The aboriginal Turanians, on the other hand, had no literature, but an innate love of art, and built as instinctively as bees. Their religion, like that of all similar races, was ancestral. They had no distinct idea of a future state, but supplied its place by metempsychosis; and, as before stated, their principal outward symbols of worship were Serpents and Trees.

The religion which Buddha taught was not a reform of the Vedic faith of the Aryans, but a refinement of the less intellectual religion of the Turanians. Serpent-worship was abolished, and with it human sacrifices, to be replaced by the utmost tenderness towards all living things; but Tree-worship was not only tolerated, but encouraged; the ancestral tumulus became a relic shrine; ascetics were formed into monastic communities; and, what is even more important for our present purposes, simultaneously with this upraising of a Turanian race, men began to erect permanent buildings in India. There does not, so far as we now know, exist in all India a single building or any carved stone that dates from the days of Aryan supremacy; but 300 years after the death of Sakya Muni, Asoka, then emperor of India, did for Buddhism what Constantine did for Christianity 600 years afterwards. He made it the religion of the state; and with him begins also the history of lithic architecture in that country. The old caves that belong to this age, and all those down at least to the Christian era, are literal copies of wooden forms; and it is not till after the time of the Sanchi's gateways, which were erected in the first century after Christ, that the architecture ceases to be mere imitative carpentry, and becomes appropriate to masonic forms.

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* 'History of Architecture,' by the Author. Vol. ii., p. 448.

These propositions were illustrated by diagrams on the walls, taken principally from the Sanchi and Amravati Topes, to which the speaker frequently referred as illustrating this branch of his subject.

Having established these points in so far as India was concerned, the speaker then turned to the forms which this worship had assumed among the Turanian races in other parts of the world.

The earliest written notice of the worship of Trees and Serpents is that contained in the 2nd and 3rd chapters of Genesis. With the knowledge we now possess on this subject, it appears reasonable to assume that the curse therein recorded on the Serpent was not against the reptile as such, but the expression by a Semitic people of their abhorrence of what they considered a degrading superstition, which it was necessary should be anathematized and swept away in order to make way for the purer and higher worship of Jehovah, which it was the great object of the writers of the Pentateuch to introduce. In so far as the Jews were concerned the abolition seems to have been successful; but when they come in contact with the Canaanites it again crops up occasionally. As, for instance, when the Lord is said to have appeared to Moses in a flame, issuing from a sacred tree, on which occasion the prophet's rod was turned into a Serpent. A still more remarkable instance was that of the brazen Serpent, which Moses erected in the desert to cure the Israelites from the bites they were suffering from. Though we lose sight of this image for a while, it appears that the Jews burnt incense and made offerings to it down to the time of Hezekiah, and that it was during these 600 years kept in the temple with the Asherahs or Groves, which were the symbolical trees of this form of worship. It reappeared after the time of Christ in the form of the sects of Ophites; and, in so far as we can trust coins, prevailed in all the cities of Asia Minor in which the seven churches were first established.

Both forms apparently prevailed in Babylon, but only Tree-worship has been found in Assyria; while in ancient Egypt the adoration of the Serpent apparently only formed one item in that wonderful pantheon of animal worship which formed so singular and so marked a part of their mythology.

In Greece we find a history and mythology precisely analogous to what we find in India. An old Turanian race of Pelasgi, with ancestral, and Tree and Serpent worship, superseded by an Aryan race symbolized by the return of the Heracleidæ, and all whose earlier myths represent either the prevalence of this form of worship or the struggles of the immigrant Aryan races to suppress it. When once they had attained the political supremacy, however, the Hellenes seem to have become more tolerant.

The Pythonic oracle at Delphi was adopted conjointly with the Druidic oracle of Dodona, as the principal sanctuary of the country. The oldest temple of the Acropolis at Athens was erected to enshrine the tree of Minerva, which was given in charge to the serpent Erecthonios. But still more remarkable than these was the worship of

Esculapius in the form of a serpent in the grove at Epidaurus, which prevailed till after the Christian era. Among the demigods and heroes the Serpent association was as frequent as with the greater deities, as is exemplified by the stories of Cecrops, Jason, Theseus, Hercules, Agamemnon, and generally with the Homeric fables.

Rome borrowed her Esculapian serpent-worship apparently from Epidaurus, though Italy had a centre of that faith at Lanuvium, and it afterwards became so favourite a form under the Empire that the number of tame Serpents became a positive nuisance.

The Germans apparently worshipped Trees, but never Serpents; but in Scandinavia, the Finns and Lapps and other Turanian tribes brought with them both Tree and Serpent worship to such an extent, that notwithstanding the long supremacy of Northmen of a different race, both Trees and Serpents were worshipped in Esthonia as in Scandinavia in the last century, and the faith as exhibited in the Edda is as near a counterpart of what is found further East, as could well be expected considering the distances of the places and the very different channels through which the description reaches us.

From Scandinavia the faith seems to have reached the north-east coast of Scotland, but not to have penetrated south of the Forth in that direction. Its traces are very few and indistinct south of the Tweed, and what are found seem to have come by a more southern route from some other source. Both the Welsh and the Irish, however, have many traditions of Serpent-worship, which, if treated reasonably, might throw much light on the subject; but except the legend of the Virgin Keyna, at Stanton Drew, they are at present all of the vaguest form.

Leaving these indistinct traces to fade into the western ocean, the speaker next pointed to Africa as the great centre of Tree and Serpent worship of the present day. The faith of the kingdom of Dahomey, on the Gold Coast, is essentially the adoration of Trees and Serpents, accompanied by ancestral worship and human sacrifices, and female soldiers. In fact, Africa preserves in full vigour and perfection at the present day all those characteristics which we see only dimly reflected in the myths of other nations.

In the new world, too, the worship of the Serpent—apparently there connected with that of the sun—certainly prevailed extensively before that continent was discovered by Columbus; and with forms so like many of those found in Asia that frequent attempts have been made to prove that what we find there is a form of Buddhism. This cannot, however, be sustained; but it certainly appears to be a form of that primæval faith on which Buddhism was based and out of which it arose in India.

In conclusion, the speaker pointed to certain forms of Dolmens, stone circles, menhirs, and such like rude stone monuments, found in India, identical in form and purpose with those found in Africa, in Brittany, and nearly all over the world wherever a Turanian people can be traced. These are not necessarily old, though some of them

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In conclusion, the speaker pointed to certain forms of Dolmens, stone circles, menhirs, and such like rude stone monuments, found in India, identical in form and purpose with those found in Africa, in Brittany, and nearly all over the world wherever a Turanian people can be traced. These are not necessarily old, though some of them may be of any age: others were certainly erected in India within the limits of this century, and are undistinguishable from the older examples; showing how persistent certain forms of faith are when once adopted by certain races of mankind. Among these the Turanians are certainly the most instinctive and least progressive of any.

It is this last fact which gives unity while it adds interest to the whole subject. In Tree and Serpent worship we have the oldest known form of faith and belonging to the most ancient people of whose existence we have any knowledge. It is now found generally in a nearly fossil state underlying the Semitic and Aryan strata which have been superimposed upon it. Occasionally, however, it crops up in out-of-the-way corners of the world, fresh and vigorous, and tells a strange tale of the persistent unchangeableness of certain races of mankind and still more strange irradicability of certain forms of superstitious faith.

[J. F.]

WEEKLY EVENING MEETING,

Friday, February 12, 1869.

William Robert Grove, Esq. M.A. Q.C. F.R.S. Vice-President, in the Chair.

Col. W. F. D. Jervois, R.E. C.B.

On the Coast Defences of England.

In bringing to your notice the subject for our consideration this evening, I will first briefly refer to the coast defences of former times, and then endeavour to explain the principles of defence which are being adopted in this country at the present day.

In the earlier periods of English history our Anglo-Saxon forefathers were exposed to the frequent predatory incursions of the Northmen and the Danes, who, starting from the shores of Norway and Denmark, sailed round the coasts of England, ascended the navigable rivers, ravaged and laid waste the surrounding country, and then returned with their booty to their ships; and during the frequent wars waged by our Norman and Plantagenet kings with the sovereigns of France, the shores of England were subject to attacks by the French, which differed but little in character from the predatory incursions of the Northmen.

The precautions taken by our ancestors to guard against such attacks are thus described by Holinshed:—"The custome of the countries adioining neere to the sea is, especiallie in time of warre, on euerie hill or high place to erect a beacon, with a great lanterne on the top, which may be seen and discerned a great space off, and when the noise is once bruted that the enimies approch neere the land, they suddenlie put fire in the lanterne, and make shouts and outcries from towne to towne and from village to village. Some run in post from place to place, admonishing the people to be readie to resist the jeopardie and defend the perill. And by this policie the fame is soon blowne to euerie citie and towne, insomuch that as well the citizens as the rurall people be in short space assembled and armed to repell and put backe the newly arrived enimies."

But though these precautions sufficed to repel small predatory attacks, they were incapable of resisting anything like a national invasion; and the attacks of our foreign enemies were generally successful whenever they were made upon a sufficient scale. For instance, Dover was taken and partly burnt by the French under Montmorenci in 1296; and in 1339 the same nation took and sacked Portsmouth in retaliation for the English attack upon Boulogne. In the following year the French ravaged all the southern coasts of England during the absence of Edward III. in Flanders, destroyed several ships at Hastings, reduced the greater part of the town of Southampton to ashes, and burnt Plymouth nearly to the ground. In 1377, the French attacked the town of Rye and the Isle of Wight, and made great havoc at Hastings, Portsmouth, Dartmouth, and Plymouth; and three years later they pillaged Winchelsea, and several other towns. In 1405, a large French force was landed at Milford Haven, and before the army sent by our Henry IV. could reach the spot, the French had plundered Carmarthen and some other towns, and had safely regained their ships.

Fortunately, however, the progress of civilization has introduced better usages into the practice of modern war, and it is not necessary now to provide against such plundering expeditions. In fact, the length of our coast is far too great, and the accessible points are far too numerous, to render it possible to fortify them all. When we speak then in the present day of our "Coast Defences," we must not be understood to advocate a vain attempt to secure by defensive works every vulnerable point; we mean to apply the term only to the means which it is necessary to provide for the protection of our national dockyards and arsenals, and of our main commercial ports.

We are sometimes told that the idea of fortifying these important points of our coasts is "un-English;" but if it be so, we ceased to be English some 330 years ago, when extensive fortifications were erected along our coasts by King Henry VIII. During the first years of that monarch's reign, his attention was attracted to the facility with which the French effected some landings on the coasts of Sussex. He had also been struck by the success of the French in their campaigns on the Continent in the fifteenth century, when their superiority in artillery turned the tide in their favour; and at a later period, when he had quarrelled with the Pope, he received information that the Roman Pontiff, at the instigation, as we read, "of Cardinall Pole, had moued and stirred divers great princes and potentates of Christendome to invade the realme of England." Thereon King Henry gave his anxious consideration to the question how best to fortify the most important points of the coast; and the steps he took for that purpose were such as might have been expected from his energetic character. He himself rode to inspect a considerable portion of the coast, sent his counsellors to survey all the ports and exposed places, caused forts and bulwarks to be erected, ordered general musters to be made, and armour and weapons to be "seen and viewed."

Lord Herbert of Cherbury, in his life of Henry VIII., tells us that "all these preparations being made against a danger which was believed imminent, seemed so to excuse the King suppressing of the abbeys, as the people (willing to spare their own purses) began to suffer it easily: especially when they saw order taken for building divers forts and bulwarks on the sea coasts."

In the State Papers, I find an interesting letter from Sir Antony Knyvet to the king, dated October, 1541, highly extolling the new fortress at Portsmouth, then recently erected; and begging the king to take an early opportunity of inspecting the plans of that great work; thus indicating that King Henry took a personal interest even in the details of his fortifications. The king's attention had previously been strongly drawn to the progress in the science of artillery. Mr. Froude, in his 'History of England,' observes that Henry VIII. was one of the first men to foresee and value the power of artillery; he tells us that Sebastiniani mentions experiments in the range of guns which were made by the king in Southampton Water; and he adds that "when the history of artillery is written, the labours of Henry VIII. in that department must not be forgotten."

During the reigns of Edward VI. and of Mary the subject of coast defence was almost entirely lost sight of. Not only was nothing done to extend, but scarcely anything was done to maintain the works constructed by King Henry. It was not until Queen Elizabeth came to the throne that public attention was again directed to the question.

At this time (1559) England being actually at war with the second power in the world, the whole naval force in commission amounted to seven coastguard vessels—the largest of which was only of 120 tons—and of eight small merchant ships altered for fighting. Of ships in harbour fit for service there were but nine, of from 200 to 800 tons, with twelve sloops and boats. In artillery, the destitution was even more pitiable. Of cannon (48-pounders) and "demi-cannon" (24-pounders), in all the dockyards, there were but thirty which were reported sound, with 200 culverins (12-pounders), minions (3-pounders), and falconets. As for the troops, a certain Captain Turner, who was sent to command at Portsmouth, where he was in daily expectation of a visit from the French, reported to Cecil "that they were all grown to misorder and mischief, and to the greatest ill that man's head could imagine."

There is a quaint letter written in 1587 by Lord Sussex to the Council, in which he complains among other things that the platform of the round tower at Portsmouth was so old and rotten that on the day of Her Majesty's coronation he "durst not shoot off the piece." The forts at Gravesend and Tilbury were in no better plight; for we find in a letter from Walsingham to Leicester, dated July, 1588 (the Armada almost in sight), that at Tilbury and Gravesend he could not find a single platform on the ground or aloft that was fit to bear any "ordinance."

The country was, however, now aroused to a conviction of the necessity for taking active steps towards improving the state of the defences, and we now hear for the first time of a system of batteries for the defence of the Medway, and, a few years later, of permanent fortifications at Milford Haven. Orders were also given for the repair and arming of minor works at Rye, Eastbourne, Hastings, &c.

It would take too much of your time to dwell upon the measures for the minor improvement of our defences which were undertaken during the succeeding reigns; and I will pass on to the year 1666, when there was war with France, Denmark, and Holland, and when the House of Commons, to quote the words of Macaulay, "readily voted sums unexampled in our history" towards putting the country into a state of defence. This period appears especially interesting, as showing the course pursued when our fleet, as Macaulay describes it, existed only upon paper, and when our enemies had the command of the sea.

The chief engineer of this time, Sir Bernard de Gomme, was a man well fitted for such an emergency, and under his superintendence every effort was made to place the country in a state of defence.

At Portsmouth, the old mud walls which had been constructed by King Henry VIII. had fallen into decay, and new lines were ordered and executed. The chief portion of the work, which is substantially the same as is now known as Portsmouth Lines, was completed in July, 1667. The lines of Gosport, which formerly only enclosed the town of Gosport itself, though designed and actually commenced at this time, were not completed for many years after. The fortifications of the Isle of Wight were jealously examined, and by the strenuous exertions of all concerned, the whole position was put into so excellent a state of defence, that we find by intercepted letters from the Dutch Admirals De Ruyter and De Witt, dated July, 1667—mark this—that they were deterred by the strength of Portsmouth and the Island from making their intended attack. At Plymouth the citadel on the Hoe was built, and at Dover, Weymouth, Dartmouth, and the Scilly Isles the fortifications were also strengthened. Sheerness was fortified now, and Pepys in his diary tells us that the king and the Duke of York went to Sheerness to see the ground which Sir Bernard de Gomme had staked out for the new fort there. The works, however, were not completed soon enough to prevent its guns being captured by the Dutch fleet under Admiral Ruyter in 1667, as they swept up the Medway to Chatham. The defences of Gravesend, Tilbury, and Woolwich, were also strengthened, and the city of London granted a loan of 10,000*l.* for building fortifications on the Thames and Medway. The castles of Deal, Walmer, and Sandown had turf laid on their walls, but it is written of Sandwich that "the chief magistrates there kept to their old trade of disagreeing, and have left off fortifying themselves."

Along the east coast too, the indefatigable Sir Bernard found scope for his energies, and at Harwich works were constructed by him, which repulsed the attack of the Dutch fleet in July, 1667. In the north, fortifications were either erected or repaired at Tynemouth, Scarborough, Bridlington Quay, and Hull.

Doubtless at other places works were erected or strengthened at this juncture; but the foregoing are the principal that were executed up to the date of the peace which was concluded between England and her foreign enemies in the autumn of 1667.

Nothing further appears to have been undertaken in the construction of coast defences until the middle of the reign of Queen Anne, when an invasion of England was projected by the King of France on behalf of the Pretender. This project received both the pecuniary help and the prayers of the Pope; and great preparations were made for the extension of our fortifications, so far as the purchase of the necessary *lands* is concerned. Large quantities were taken under acts of Parliament, at Portsmouth (for the Portsea Lines, &c.), at Harwich, and at Chatham; but with the exception of one fort at Portsmouth, I do not find that works were commenced at any of these places until nearly fifty years afterwards.

The next works of importance were constructed during the reign of George II.—*viz.*, the lines at Chatham and Devonport, which were commenced towards the end of the reign of that monarch. An extension of the fortifications at Gosport, batteries on the Southsea shore, and a small work since replaced by Fort Cumberland, were erected at about the same time. Batteries were also ordered on sundry points in Milford Haven, and at several points along the south coast; the cause for all these preparations being, as cited in the preamble of the Act 31 Geo. II., cap. 39, "the unjust and hostile invasion made on His Majesty's dominions in America and the Mediterranean, and great preparations made in France for invading these realms."

Towards the middle of the reign of George III., when the Duke of Richmond was Master-General of the Ordnance, and during the French Revolutionary War, still further additions were made to our coast defences. At Portsmouth, Portsea Lines were built, Fort Cumberland was entirely reconstructed, the old Hilsea Lines were thrown up, and the western defences were extended by the construction of Fort Monckton; a few batteries were also erected for the defence of Stokes Bay. About this time, also, Sheerness Lines were constructed, and the works at Dover were largely added to. Numerous small earthworks were also built at this juncture at various parts of the English coast; but they were allowed to fall into decay after the war. At Plymouth, large quantities of land were acquired at various parts in the vicinity of the town and dock for the erection of Devonport Lines; and amongst other works which were now constructed was the line of redoubts and barracks which occupy the position known as the Maker Heights. Pitt (who took a great interest personally in these questions) intended to have erected a citadel on Maker Heights; but he was defeated by a majority of one (the Speaker's casting vote) in a proposal for the further extension of our fortifications at Portsmouth and Plymouth.

After this, for half a century, we acted like the magistrates of Sandwich, and "left off fortifying ourselves." Men indulged in dreams of universal peace, which were, however, rudely dispelled by the great Crimean war.

Now we come to the efforts of the present generation, and never has there been a time when it has been so difficult to deal with questions of defence.

The introduction of steam, of rifled guns, then of iron-clad ships, which rendered necessary the construction of artillery of enormous power; these are the conditions with which we have had to deal during the last few years, and it may be confidently asserted we are second to no other nation, if we are not superior to all, as regards the steps taken to meet present requirements, in every department of coast defence.

I will now proceed to consider the *principles* which should guide us in the defence of this country in the present day.

Since the periods of which I have been speaking, the British empire has been greatly extended, and the demands upon our resources have become proportionably enlarged. For the general defence of so vast an empire, we must first of all and mainly depend upon our seagoing fleet. It is to the fleet we must look for our first line of defence against invasion of this country, to maintain our communications with our foreign possessions, and to protect our commerce and our interests generally both at home and abroad.

But the bases on which our naval power must rest are the ports, dockyards, and arsenals, in which our fleets and squadrons are harboured, coaled, and refitted. Thus we have naval establishments at Portsmouth, Plymouth, Chatham, and Pembroke, at home; at Bermuda, Malta, Gibraltar, and several other places abroad. These places are the roots from which our naval power springs, and they require special protection against attacks that may be made upon them, during the absence of our fleet, either by hostile naval forces alone, or by combined naval and military expeditions.

If our navy could be kept up in sufficient strength to meet the navies of other nations at all points where hostile fleets or cruisers might attack us, questions about attack upon these naval arsenals would be disposed of.*

Very little reflection and calculation, however, are necessary to show that the resources even of this country, whether in money or in seamen, would not admit of our maintaining such enormous naval means as would, of themselves, suffice to defend these sources of our naval strength, and at the same time to protect our commerce, or to prevent an enemy from either landing on our shores or attacking our widely scattered commercial ports, whether at home or abroad.

Even if it were possible that a fleet sufficient to fulfil all these duties could be maintained, such an application of the resources of the nation would lead to an expenditure of public money far exceeding that which would suffice for the defence of the country with the aid of other means. The first cost of such a navy would be enormous, and would have to be repeated every twenty or thirty years, and the expense for maintenance would be great and continual.

Further, if the several sections of our navy were employed for the protection of the arsenals whence they are maintained, we should be using the fleet to maintain the dockyards instead of the dockyards to maintain the fleet; and each of the sections of our navy thus scattered over the world would be liable to be overpowered by the concentrated forces of the enemy.

Other defences besides the navy are, therefore, essential for these nurseries of the fleet.

First, to consider the means which should be applied for the defence of these places against naval attack.

These are, *big guns above water*, and *big mines below water*, placed, of course, at the proper distance in advance of the object to be protected.

Booms of timber, rafts, chains, or nets, might also be used for obstructing channels; but the attention that has been given during the last few years to the application of submarine mines has rendered these *passive* obstructions of minor importance.

To proceed then to consider *active* obstructions, or mines below water—

These are now, in most cases, necessary to obstruct the passage of ships, and to keep them under the fire of the guns above water, and have become of especial importance since iron armour has been applied to the sides of ships, and thus these vessels of war have become most vulnerable at their bottom.

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* Many of the arguments adduced in this and the following paragraphs are repeated from a lecture given by me at the United Service Institution in June, 1868.—W. F. D. J.

There are two classes of these mines—one, exploded by *mechanical* contrivances, the other exploded by *electricity*.

Those exploded by mechanical means should be of a size to contain about 150 lbs. of powder. They are much less costly than the others, and can be placed in position more expeditiously; they could moreover be applied in any number, but they are inapplicable in positions where it is necessary to keep the channel open to friendly vessels.

In such cases, electric torpedoes must be used. These admit of a friendly vessel passing over them in safety, and of a ship in chase being sunk by the completion of the electric circuit.

Their charges would be about 1000 lbs. of gunpowder, or about 400 lbs. of gun-cotton, and would ensure the destruction of a ship by their explosion, not merely when *immediately* over them, but if even any portion of her were within forty or fifty yards of their position.

It used to be the fashion, even amongst professional men, in this country, to consider this mode of defence as being incapable of practical application; but of late years a very different opinion has prevailed, and the effective application of submarine mines, used in conjunction with forts and floating-batteries, is of immense importance, not only in connection with fortifications, for the defence of our great naval stations, but also for our minor ports, and for the protection of all places on the seaboard.

Foreseeing the important part that submarine mines would play in coast defences, I had the honour of proposing to Lord de Grey, in 1862, that a special committee should be appointed to consider and report upon the question of their application to defence of harbours.

Lord de Grey at once recognized the importance of such an inquiry, and appointed a committee, which, after prolonged investigation and experiment, has made a most valuable and interesting report upon the subject.

We are now in a position to apply this system of defence to any extent that may be required for the protection of our harbours and other points upon our coasts; and we are much indebted to the committee for the thoroughly practical results which we are now enabled to attain in this important and inexpensive element of national defence.

The question is sometimes asked,—Whether the application of submarine mines will not render unnecessary the employment of forts and batteries for defence against naval attack?

Forts and batteries, however, are still required in all important cases to cover the torpedoes and prevent their being tampered with. It must also be remembered that whilst the submarine mine is harmless unless the ship comes near it, the shot from the battery can injure the ship whatever may be her position within effective range.

Further, although probably our harbours might be efficiently obstructed by torpedoes in at from seven to fourteen days' notice, yet one condition is that the weather should be sufficiently favourable to allow them to be exactly laid. There are, again, certain positions where, even if the torpedoes *are* laid, they might be disturbed by a violent storm, and, possibly, an attack on the positions in which they were to serve *might* take place before they could be renewed; and though the periods of the year at which these difficulties might arise are short, yet the bare possibility of interference in the application of a complete torpedo system prevents our placing entire reliance on such a defence for the protection of places on which the warlike power of the nation, both for offence and defence, must in a great measure depend. Therefore, although submarine mines are a most important element in the defence of our harbours and coasts, and add greatly to the power of our forts to resist a naval attack, they must not be regarded as substitutes for permanent works of defence at our naval arsenals and harbours, and other important ports.

To proceed now to consider the employment of big guns *above* water. These with all the numerous accessories for their service must be placed in positions so protected and arranged as to give them a decided superiority over the artillery of assailing ships.

The question then arises whether they shall be placed afloat in strongly protected vessels, *i.e.* in floating-batteries; or at fixed points either on land or on shoals, *i. e.* in forts.

It is often said, "Why don't you protect your ports by floating-batteries alone?" The same reasons, however, as have been before adduced against the employment of our sea-going navy, prevent our employing special floating-batteries alone for this object.

Such a system would necessitate our maintaining at each of our chief ports a naval squadron, sufficiently powerful to resist the attack of the superior force of the enemy. Then again arise the questions, "What is a sufficiently powerful force to maintain at each point for this object; what would be its first cost; in how many years will it be necessary to repeat the outlay for it; what will be the expense of its annual maintenance?"

It is impossible to examine these questions without arriving at the conclusion that even if our resources in money and in seamen rendered it practicable to maintain such a force in addition to our sea-going navy, the defence of our ports can be effected much more efficiently and economically with the *aid* of other means.

Irrespective, however, of the question of the *expense* of providing for coast defence by floating-batteries alone, very little consideration is requisite to understand that if there *be* positions on *land* from whence an effective fire can be brought to bear on the channel, anchorage, or shore to be defended, there is no *object* in placing the guns in vessels afloat.

In positions such as I have referred to, there cannot be any object in substituting an unsteady platform—on which the amount of protection that can be afforded is limited by considerations inherent to floating structures, and which is liable to be taken away or to be sunk—for a fixed and perfectly steady platform on shore, which can be fully protected, either against its fire being silenced, or from capture by an enemy.

In cases, however, where the distance between forts is so great that the intervening space cannot be properly commanded by their fire, or where it may be necessary to have advanced batteries of artillery at a distance from the shore, and where foundations for fixed works cannot be obtained without expense and difficulty disproportioned to the object, it becomes *necessary* to employ floating defences.

In short, we must, in each case, consider—

1. Whether we can provide for the defence by forts *without* floating-batteries.
2. If not, to what extent floating defences should be applied in *conjunction* with forts. And
3. Whether the circumstances are such as to render it advisable to employ floating-batteries in *substitution* of forts.

The question is not one, as it is often put, of "floating-batteries *versus* forts." There is no "versus" in the matter. Both are required in their proper places.

The question of the *kind* of floating-battery to be employed for harbour defence has from time to time been much discussed.

Ten years ago, at my suggestion, a committee was appointed by General Peel to consider the subject. Admiral Cooper Key, Colonel Wilmot, R.A., and myself were the members of this committee. We then recommended the employment for harbour defence of small vessels, each carrying a fixed iron tower for four guns, and provided with eight ports. It is curious how nearly this vessel approached the 'Monitor' type first used in the memorable fight at the mouth of the James river, in America, with the 'Merrimac,' in 1862. I believe it is generally admitted that the 'Monitor' class of vessel is the best kind of armour-clad floating-battery for coast defence; but amidst the many projects for floating structures for defence now advocated, it would be presumptuous for me to give any decided opinion on this subject. In some cases iron-clad 'Monitors,' supplemented by a mosquito squadron of gun-boats, might be employed, and, to oppose unarmoured cruisers or privateers to the attacks of which alone the less important harbours would be liable, small gun-boats of light draught in conjunction with submarine mines would alone suffice. I take this opportunity of showing a model of a small gun-boat for one gun, proposed by Mr. Rendle, of the Elswick Ordnance Company, in consultation with Admiral Cooper Key, which appears admirably well adapted for the small class of vessels for harbour defence.

I now pass on to consider the defence of our great naval arsenals, in the event of an enemy obtaining a footing on our shores.

We maintain a large military force of regulars, reserves, militia, and volunteers, and thereby admit the possibility of a campaign taking place in this country.

The landing of a hostile army on our coasts must be admitted to be a difficult operation; but it would, to say the least, be very unwise if we were to conclude that invasion is impossible because it is difficult.

It must be assumed that the main object of an enemy in an invasion would be to get to London; for by doing so he would not only occupy the commercial heart of the empire and the seat of government, but the main military arsenal of the empire at Woolwich would also fall into his hands.

Supposing him to have obtained a footing on shore, and to be advancing on London, we interpose between him and his object the regular army, in conjunction with the armed and organized manhood of the country, more or less disciplined, and aided by such temporary defences as could be thrown up at the time of expected attack. But as the best disciplined and the greater part of our military forces must be employed to cover the capital, we must arrange our plan of defence so that as few disciplined troops as possible may be necessary for the defence of other points in the country which must be defended, but which cannot be covered by the operations of the main army.

Portsmouth and Plymouth, for instance, are therefore defended on the land side by the aid of fortifications which will enable a comparatively small number of partially disciplined forces, with the aid of a few regular troops, to protect those places against capture or bombardment, whilst the main army would be employed in the defence of London and Woolwich.

A mistake is commonly made that because the places I refer to are fortified, the garrisons of those places must be largely increased. The case is precisely the reverse. Even if all the outer line of forts to landward at either Portsmouth or Plymouth were fully manned at the same time (which would be quite unnecessary, not more than one-half need be fully manned at the same time), only between 6000 and 7000 men would be required for the purpose at each place respectively, and only a very small portion of these need be regular troops. The remainder of the garrisons would consist of a movable force, which in any case we *must* have for the defence of these places, but which in the absence of the forts must be of sufficient strength and sufficiently disciplined to meet the enemy in the open field, whilst *with* the forts it may be comparatively small in number, and only disciplined to take up a fighting position, under the support of the works, as that part of the fortified line assailed.

Unfortified, an enemy would only have to detach about 15,000 or 20,000 men from the main invading army to effect in a few days the destruction of all our ships and naval establishments at Portsmouth; *fortified*, he must employ an army of at least three times that number, and must have a considerable time at his disposal to undertake a regular siege.

Unfortified, no force that, in the case referred to, we could afford for the garrisons of these places could protect *either* against the attack of 15,000 regular troops; *fortified*, there is no difficulty in providing the numbers and description of troops that would be capable of making a good defence of these nurseries of the navy.

Unfortified, they at once fall if an enemy were to obtain a decisive victory over the army in the field; *fortified*, they remain in our hands even under such untoward circumstances, and thus enable us to avert the destruction of our naval power at a period when all the resources of the country will be required to enable us to retrieve the position temporarily lost.

A few words in conclusion. We are often told that these works are unproductive, and so in one sense they are; but who shall say what effect permanent measures of defence may have upon the position of this nation? I verily believe they tend to peace and to the development of the prosperity of England.

But we are met with other objections. It is commonly said that there is no use in constructing permanent fortifications, because the inventions of one age render useless the efforts of the previous generation. History, however, does not support this statement. At this moment we are turning to account, and with good effect, works erected by Henry VIII. more than three centuries ago. In like manner, works constructed in the time of Charles II. and in the reigns of George II. and George III. form parts of the system of fortifications we are adopting at the present day.

Then we are told that these measures are so extravagant. The fact, however, is that, by the aid of fortifications, we are enabled to defend the empire with fewer troops and a smaller navy than would otherwise be necessary, and that they thus tend greatly to economy. If we are to make provision for defence *at all*, it is only by the aid of fortifications that we can have an *economical* system of defence.

They say then, that our fortifications "lock up" troops. I have shown you that, on the contrary, they enable us to utilize our auxiliary forces and to set the regular army free.

But again, we are told that we ought to depend upon our navy; but the main purpose of the fortifications is to enable the navy to do its duty effectively. The navy is without doubt the arm on which we must mainly depend; but that is no reason why we should tie it down to our side!

Lastly, they say, "An enemy won't go near these fortifications." My reply is, that that is the very object for which they are provided.

To sum up. The truth is, that to provide an efficient system of defence at the least cost to the state, the sailor, the soldier, and the military engineer must each occupy his proper place. The navy and the army are the vital principle of defence; and fortified arsenals and harbours are the centres of refuge and action for both. Take away fortifications, and you are unable to turn your auxiliary forces to proper account; you leave an army insufficient for the duties it has to perform; a navy, scattered, unsupported, and with no protected home.

[W. F. D. J.]

WEEKLY EVENING MEETING,

Friday, Feb. 19, 1869.

Sir Henry Holland, Bart. M.D. D.C.L. F.R.S. President,
in the Chair.

C. Greville Williams, Esq. F.R.S.

On the Female Poisoners of the Sixteenth and Seventeenth Centuries.

[No Abstract received.]

WEEKLY EVENING MEETING,

Friday, February 26, 1869.

Sir Henry Holland, Bart. M.D. D.C.L. F.R.S. President,
in the Chair.

John H. Bridges, M.A. M.B.

On the Influence of Civilization upon Health.

Rousseau, representing the metaphysical and absolute spirit of the revolutionary politicians of the eighteenth century, maintained that civilization was destructive to health and morals, and would have it altogether swept away. The positive sociologist recognizing that the phenomena with which he deals, like all others, are amenable to natural laws, seeks to find out what the Law—the natural tendency of modern civilization—is, and how it may be modified by human effort in a direction favourable to the moral and physical development of the individual. It is the greatest of all human problems. Leibnitz's maxim, *On ne doit penser essentiellement qu'à deux choses, d'abord la vertu et puis la santé*, may be taken as the text of this discourse.

Health may be defined as, *The greatest energy of each part compatible with the energy of the whole;* or again, more simply, as, *Being able to do a good day's work easily.* Energy is measurable by the amount of work done. Where there is perfect health there will be the greatest economy of the vital energies; the most complete *synergy* of the functions, the minimum of loss from antagonism, or from degradation of higher into lower forms of force. Three examples were taken by the speaker; good and bad digestion; clumsy and skilful muscular effort; uncontrolled and self-contained emotion, as shown in the contrast between the well-drilled soldier and the savage. In the imperfect performance of each of these three functions there is waste of energy; that is, ineffective transformation of nervo-muscular into calorific or some other lower form of force.

Again, Health may be defined as *the most perfect form of Life.* Life having been defined by Auguste Comte as the constant adaptation of organism to environment, health is the state in which such adaptation is most complete. Healthy respiration implies the adaptation of a breathing apparatus hereditarily vigorous to a perfectly pure atmosphere. Healthy vision implies a sound ocular mechanism adjusted to appropriate conditions of luminous forces.

Rising from the Vegetal and the Animal phase of life to the highest mode peculiar to man, the Social phase, we find that Social Life depends, like the others, on two conditions: a brain in which lies latent the hereditary capacity for receiving and transmitting the long tradition of our race, its emotions, its thoughts, its modes of action; and a social environment supplying the appropriate stimulus which calls these latent capacities into action. The environment for this phase of vitality is Humanity; that is to say, the resultant sum of all human effort throughout the immeasurable past, represented more or less perfectly by the society in which the lot of the individual may be cast. The function of the human brain, as transmitting the mutual reactions of Humanity and the Individual, has been compared by Comte to the function of the placenta with the mother and the unborn child.

The speaker then proceeded to point out the influence upon the individual of different social environments. In order to render the meaning more precise, a single well-defined instinct was chosen, the desire of praise. In one society personal bravery might be the object of the highest admiration; in another, it might be civic duty; in others, religious asceticism. Obviously the instinct under consideration would in these three societies prompt three wholly distinct courses of action; and in a society like our own, which had ceased to be warlike, which was too overgrown to encourage the sense of citizenship, and which from a combination of causes had lost its religious faith, wealth, as comprising the power of individual enjoyment, became the object of the highest admiration. The instinct of "approbativeness" therefore would in such a society prompt the acquisition of wealth. The same analysis would hold good of the action of the other instincts. In a society without strong religious faith, without any firm binding principles, and endowed with new and extraordinary powers over natural forces, the inevitable result is immensely rapid accumulation, accompanied by prodigal expenditure, of wealth; with small regard to health, with small provision for the future maintenance of the vigour of the breed.

The speaker then proceeded to consider some of the principal facts illustrating the morbid condition of modern industrial life :—

1. The growth of great towns in the last half century.
2. The influx into these towns from country districts.
3. The mortality of infants.
4. The mortality of adults at the reproductive age.
5. The condition of the agricultural population, which may be regarded as the reserve stock of national vitality on which rapidly increasing demands are being made.

With regard to the two first heads, statistics were produced showing that in 1811 the towns above 10,000 inhabitants contained only 24 per cent. of the population ; in 1861 they contained 44 per cent. In 1811 there was no town in England, except London, with a population above 100,000 ; in 1861 there were twelve such towns, containing one-quarter of the people. To illustrate the extent to which the increasing population of London was fed by the rural districts, it was mentioned that out of eleven persons over the age of twenty in London, six were born in the country.

Statistics and charts were then produced, illustrating infant and adult mortality. Four populations were compared : that of England as a whole ; of Liverpool ; of ten cotton manufacturing towns of Lancashire and Cheshire, not including Manchester ; and of seven purely agricultural counties. These populations were compared : first, with respect to their rate of mortality at the infantile period ; secondly, at the reproductive period ; the mortality in the case of women being examined at successive periods from 15 to 45 ; in the case of men, from 25 to 55. It was shown that in both periods the rate of mortality in Liverpool and the cotton districts was far above the rate for England at corresponding times ; that of the agricultural districts far below it. Attention was also called to the fact, that in towns where women were largely employed in factories, their mortality relatively to men of the same age was far beyond what it should be, taking the standard of England as a whole.

The question was started whether the excessive mortality of infants might or might not be regarded as a preservative action of nature, sacrificing the unhealthy lives, and so preserving the vigour of the breed. It was also considered, whether or not sanitary measures, by preserving weak lives, might not in some measure tend to deteriorate the breed. With regard to the latter point, the speaker's view was that unless sanitary measures were carried much farther and deeper than at present, there might be some such danger. And meantime he brought statistics to show that the high rate of mortality in infants in large towns did not preserve the reproductive portion of their population from similarly high rates.

After a brief allusion to the unsatisfactory condition of our reserve stock of health, the agricultural population, the speaker then proceeded to discuss the historical causes of this state of things. He showed, by illustrations from the vegetable and animal kingdoms, that the state of equilibrium we call health is more difficult, yet at the same time more perfect, as we ascend from the lower organisms to the higher. The same holds true of primitive and barbarous times as compared with the more advanced stages of civilization. Reference was made to Captain Cook and to travellers in Central Africa to prove the remarkable immunity from European disease enjoyed by savages. The influence of an organized religion in controlling social action, and its bearings upon public health, were then considered, and were illustrated by allusion to the theocracies of Egypt, Palestine, and India, to the military polytheism of Greece and Rome, and to mediæval Catholicism. The two causes of the decline of Catholicism, the decay of theology, and the growth of science, were then pointed out, and it was shown how both these causes co-operated in producing the exceptionally morbid state of modern industrial England.

The question then remained, By what methods can we hope to remedy this state of things?

There are two forces, the speaker remarked, available for the modification of natural social agencies – Capital, and scientifically trained Intellect. If asked, therefore, where lies the efficient and radical remedy for the evils we deplore, the answer would be, nowhere but in a moral and religious change, as profound as that of the first century of our own era in Western Europe, or that of the seventh century in the East, the principal result of which will be to concentrate these two forces on such problems as have here been discussed. The present attitude of scientific men does not inspire immediate hope in this respect. Absorbed in their own specialities, they revolt against the idea of any moral or spiritual discipline, which would have the effect of concentrating their investigations more closely round human interests, although these, largely viewed, offer an abundant and super-abundant field for every intellectual energy. They rebel against all such claims as an unpleasant restraint. And the sense of civic duty *is* restraint. *Noblesse oblige.*

This great remedy therefore will be slow in its action. But while there are some remedies which are deep, but not immediate, there are others which are immediate, though not deep. To some of these the speaker briefly alluded. A revision and consolidation of our sanitary laws; the appointment by Government of public inspectors of health; the introduction of very simple sanitary teaching into our primary education; the establishment on a large scale of public parks and gymnasia; and finally, special attention to the sanitary and social requirements of the agricultural labourer, from whom it was essential not to cut off, as had been done during the last century, the hope, however distant, of becoming a peasant proprietor.

With regard to the cost of these and similar measures, the speaker merely remarked that London alone expended a yearly sum of between three and four millions upon doubtful charities; and that we had found little difficulty in raising ten millions, of late years, to protect ourselves against the imaginary danger of invasion. Whereas the dangers now pointed out were not imaginary, and were undoubtedly progressive.

[J. H. B.]

GENERAL MONTHLY MEETING,

Monday, March 1, 1869.

Sir Henry Holland, Bart. M.D. D.C.L. F.R.S. President, in the Chair.

Peter Allen, M.D.
Frederic Kett Barclay, Esq.
Mrs. Bowie,
Thomas Boycott, M.D. F.L.S.
Henry Chester, Esq.
Professor A. H. Church, M.A.
Charles Cogswell, M.D.
Mrs. Charles Crokat,
Edward Dent, Esq.
William Gardiner, Esq.
Gilbert Finlay Girdwood, M.D.
Walter Henty, Esq.
Charles Latham, Esq.
John Macdougall, Esq.
Edward Moberly, jun. Esq.
Robert Palmer, Esq.
Rev. George Charles Pearson, M.A.
Alfred Rowlls Rowlls, Esq.
Archibald Travers, Esq.
Alfred Wills, Esq.

were *elected* Members of the Royal Institution.

Frederick Leighton, Esq.

was *admitted* a Member of the Royal Institution.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

Asiatic Society of Bengal—Journals, Nos. 149, 150. 8vo. 1868.
Proceedings, 1868. Nos. 9, 10, 11. 8vo.
Astronomical Society, Royal—Monthly Notices. Vol. XXIX. No. 3. 8vo. 1869.
Chemical Society—Journal for Feb. 1869. 8vo.
Devonshire Association for the Advancement of Literature, Science, and Art—Report and Transactions. Vol. II. Part 2. 8vo. 1868.
Dublin Society, Royal—Journal No. 37. 8vo. 1868.

Editors—American Journal of Science and Arts. Jan. 1869. 8vo.
Artizan for Feb. 1869. 4to.
Athenæum for Feb. 1869. 4to.
British Journal of Photography for Feb. 1869. 4to.
Chemical News for Feb. 1869. 4to.
Engineer for Feb. 1869. fol.
Geological and Natural History Repertory. Feb. 1869. 8vo.
Horological Journal for Feb. 1869. 8vo.
Journal of Gas-Lighting for Feb. 1869. 4to.
Mechanics' Magazine for Feb. 1869. 8vo.
Pharmaceutical Journal for Feb. 1869. 8vo.
Photographic News for Feb. 1869. 4to.
Practical Mechanics' Journal for Feb. 1869. 4to.
Revue des Cours Scientifiques et Littéraires. Feb. 1869. 4to.
Franklin Institute—Journal, No. 516. 8vo. 1868.
Geographical Society, Royal—Proceedings. Vol. IX. No. 1. 8vo. 1869.
Geological Society—Quarterly Journal, No. 97. 8vo. 1869.
Horticultural Society, Royal—Proceedings, No. 12. 8vo. 1869.
Jencken J. F. M.D. (*the Author*)—Treatises on Light, Colour, Electricity, and Magnetism. Translated by H. D. Jencken. 8vo. 1869.
Photographic Society—Journal, No. 202. 8vo. 1869.
Royal Society of Edinburgh—Transactions. Vol. XXV. Part 1. 4to. 1868.
Proceedings, Nos. 74–76. 8vo. 1867–8.
Royal Society of London—Proceedings, No. 108. 8vo. 1868.
Philosophical Transactions. Vol. CLVIII. Part 2. 4to. 1869.
Symons, G. J. Esq. (*the Author*)—Symons' Monthly Meteorological Magazine, Feb. 1869. 8vo.
Teale, James, Esq. (*the Author*)—A Dynamical Theory of the Universe. (K 96). 8vo. 1868.
Teyler Foundation, Haarlem—Archives du Musée Teyler. Vol. I. Fascicule 4. 8vo. 1868.
United Service Institution, Royal—Journal, Nos. 50, 51. Index to Vol. I.–X. 8vo. 1868.

WEEKLY EVENING MEETING,

Friday, March 5, 1869.

Sir Henry Holland, Bart. M.D. D.C.L. F.R.S. President, in the Chair.

William Huggins, Esq. F.R.S.

On some further Results of Spectrum Analysis as applied to the Heavenly Bodies.

The speaker commenced by saying that four years ago he had the honour to give in the theatre of the Royal Institution an account of the results of an attempt to apply the method of analysis by the prism, for which science is indebted to Kirchhoff, to the light of the heavenly bodies. It was the speaker's purpose to describe, on the present occasion, some of the results which had been obtained in his observatory since the spring of 1865. The peculiar suitability of spectrum analysis as a mode of investigation of the bright objects in the heavens had been confirmed, not only by the gain of further information of the chemical and physical constitution of some of these immensely distant bodies, but also by knowledge of another kind which this elegant and searching method of analysis had revealed to us.

The speaker then described the three typical forms under which all spectra may be classed, and the interpretation which our present knowledge enables us to give of these different spectra when the light is emitted by bodies rendered luminous by heat. The spectra of fluorescent and phosphorescent bodies were not to be described.

1. *A continuous spectrum without dark or bright lines* shows, as a general rule, that the luminous source is in the solid or liquid state. In certain exceptional cases, however, a gas may give a spectrum which is apparently continuous. Dr. Balfour Stewart pointed out that as gases and vapours possess a power of general absorption, in addition to the selective absorption peculiar to each gas, a gas when luminous would emit light of all refrangibilities, producing a continuous spectrum, in addition to its spectrum of bright lines, and further that the intensity of this continuous spectrum would be in proportion to the opacity of the gas. The researches of Plücker and Frankland have shown that under certain conditions of density and temperature, the bright lines of hydrogen expand so as to produce a spectrum which is apparently continuous.

2. *A spectrum of bright lines* indicates that the luminous body is in the state of gas. Each gas and vapour has its own set of lines. The lines may be greatly modified, or even altogether changed, under different conditions of temperature and density, as is well known in the case of nitrogen, the vapour of sulphur, and some other substances; but throughout all these changes each gas behaves in a way peculiar to itself. There appears to be one exception to the statement that a spectrum of bright lines is peculiar to luminous gas. Bunsen found that when solid erbia is heated to incandescence, the continuous spectrum contains bright bands.

3. *A continuous spectrum interrupted by dark lines* informs us that the light has passed through vapours at a lower temperature than the source of light. As the kinds of light absorbed by each vapour correspond precisely with the set of bright lines which that vapour emits when in the luminous state, it is possible to learn if the vapours are those of any of the substances with which we are acquainted.

The speaker said that following the arrangement adopted in the former discourse, the most important recent information obtained of the *fixed stars* results from the application of prismatic analysis in a new direction. Under certain conditions the spectrum of a luminous body is adapted to tell us whether that body is moving towards or from the earth. The importance of information on this point will be seen from the consideration that the proper motions of the stars represent that part only of their whole motion which is transverse to the line of sight; for any motion they might have in the visual direction, towards or from the earth, would not cause any visible displacement of the star, and could not therefore be ascertained by the ordinary methods of observation.

As it is upon the length of the waves, or upon the number contained in the series that enters the eye, or falls upon the prism, in a second that a judgment is formed of the colour of the light, or its place in the spectrum is determined, it follows that any circumstance which would alter the length of the waves *relatively to the observer*, or, in other words, cause a larger number of waves to enter the eye in a second of time, would cause a change in the colour or refrangibility of the light so far as the observer is concerned. It is obvious that if the observer advances to meet the light, a longer series of waves falls upon the retina in a second of time, each wave appears shorter, and he ascribes to the light a higher refrangibility than he would do if he were not advancing to meet the light. If he were receding from the star an alteration of refrangibility in the opposite direction would take place. The same effect would ensue, if the luminous source were in motion. Thus to a swimmer striking out from the shore, each wave appears shorter, and he passes a greater number of them in a given interval in proportion to his speed through the water.

Illustrations were given of this principle, which was first suggested in 1841 by Doppler, by means of an analogous change of pitch in sound. Two tuning-forks sounding in unison were moved rapidly towards and from the audience, when beats were heard, which told of a difference of pitch produced by the opposite motions of the forks.

As there exists beyond the visible spectrum, at both ends, a store of invisible waves, these would be advanced or degraded into visibility, in proportion as the colours of the spectrum were altered, and no change of colour would be perceived. It is therefore essential before we can apply this method to detect the radial motion of the stars, that we know the original refrangibility of some part of the light at the moment it left the star, and also that we are able to recognize this particular part of the light again in the spectrum of the star's light. When by means of a group of dark or bright lines we learn the presence of a terrestrial substance in the star, both these conditions are fulfilled.

Of all the stars which the speaker had compared with terrestrial elements, when working with his distinguished friend Dr. W. A. Miller, Treas. R.S., Sirius, which contains four very strong lines which are due to hydrogen, appeared the most suitable for this investigation. The apparatus employed, and the special precautions which were taken to ensure the perfect coincidence in his instrument of the stellar lines with those of the substance compared with it, were described by the speaker, who stated that after a prolonged comparison, extending over many weeks, of the line of hydrogen in Sirius in the green, at the place of F in the solar spectrum, with the line of terrestrial hydrogen, he found that the line in the star had undergone a shift in the spectrum equal to a difference of wave length, which would correspond to a motion of recession between the star and the earth of 41 miles per second. The speaker had obtained evidence from experiment that this shift was not due to unsymmetrical expansion of the line in hydrogen as the density is increased. The greater width of this line in Sirius than in the solar spectrum would show that the hydrogen in Sirius, though at a pressure considerably less than that of our atmosphere at the surface of the earth, is more dense than the hydrogen in the solar atmosphere by which the dark line F is produced. This conclusion is in accordance with the presumably enormous mass of Sirius, as suggested by its great intrinsic splendour.

The earth at the time of observation was moving from Sirius at about 11 miles per second, which would leave 30 miles as due to the star. A further connection is required for the solar motion in space, which is believed to be towards Hercules, with a velocity of 4 or 5 miles per second. The whole of this must therefore be deducted, leaving about 26 miles as the motion of Sirius from the earth in the line of sight. The true motion of the star would consist of this radial motion compounded with the transverse motion of from 24 to 40 miles per second, which is shown by its proper motion.

The speaker then described a further examination of the nebulæ (about fifty have been successfully observed) with a more powerful spectroscope, which confirms his previous conclusion that these bodies consist mainly of the gases nitrogen and hydrogen. He also found that when the spectra of these gases are made faint by the removal of the spark to a distance, all the lines are extinguished with the exception of the one line in each spectrum which is found in the nebulæ. If such an extinction takes place in the case of the nebulæ, since they are objects of sensible size, it must be attributed to a power of extinction of light existing in cosmical space.

Observations of four comets have been made. A large part of the light of these strange objects was found to be peculiar, and therefore emitted by the cometary matter. Brorsen's comet at its return in 1868, and a comet discovered by Winnecke, gave a spectrum of three bright bands. The spectrum of Winnecke's comet (comet II, 1868) was found to be identical with the spectrum of carbon as it appears when the induction spark is taken in olefiant gas, and in some other compounds of carbon. The spectrum of the comet was compared directly in the instrument with the spectrum of olefiant gas.

The speaker then described some observations of the sun. He found that while the solar lines are for the most part thickened when viewed in the light from the umbra of a spot, the lines C and F, due to hydrogen did not appear to be altered. This observation is of interest in connection with the constitution of the solar prominences as shown by the observations of the great eclipse of last August. The speaker nearly three years ago, at the same time that he had independently made attempts to see the prominences by means of the spectroscope, also tried the method of using absorbing media, by which the parts of the spectrum where the bright lines occur might remain, while all the rest of the spectrum was extinguished. In this way the faint prominences would be rendered visible, in consequence of the much greater relative diminution of the intensity of the illuminated screen of air, which on ordinary occasions conceals them from view. Recently he had succeeded in viewing the outline of these objects by means of a coloured glass combined with a spectroscope with a wide slit. He expected to be able to view these objects by means of coloured media alone.

[W. H.]

WEEKLY EVENING MEETING,

Friday, March 12, 1869.

William Robert Grove, Esq. M.A. Q.C. F.R.S. Vice-President, in the Chair.

F. A. Abel, F.R.S. For. Sec. C. S.

On some Applications of Electricity to Naval and Military Purposes.

The applications which electric science has received within the last few years in connection with the military and naval services are various and important. The employment of the electric light for signalling and reconnoitring purposes; the permanent establishment of telegraphic equipments by which an army in the field or at a siege is maintained in the most intimate communication with the directing powers; the employment of electric signalling arrangements in ships of war, and the accurate investigation of the ballistic force of gunpowder and other explosive agents, are among the uses to which electricity has been put in connection with war-purposes; but the earliest application, and one of the most important and extensive—one, moreover, possessing great interest from industrial points of view—is the employment of electricity as an agent for exploding land- and submarine mines.

The possibility of applying the electric spark to the ignition of charges of gunpowder suggested itself both to Franklin, in 1751, and to Priestley in 1767; but it was not until some years after the discovery of the electric pile by Volta, that serious attempts were made to apply electricity to mining and military purposes. The first practical application of the voltaic battery in this direction was made little more than thirty years ago by French military Engineers; a few years afterwards that agent was successfully applied in this country in connection with important blasting operations, such as the destruction of Round Down Cliff at Dover, and the removal of the wrecks of the

Royal George and Edgar at Spithead. The general method of operation then pursued was adhered to by military Engineers in this country until very recently, and is, in fact, still occasionally employed, though it has been in great measure superseded by other systems which present very important advantages. It consists in inserting into the charge of gunpowder a short piece of thin wire, composed of a metal of inferior conducting power, such as iron or platinum, and placing this wire into connection with the circuit wires of the battery. The resistance offered to the passage of the current gives rise to the development of heat, the intensity of which is regulated by the conducting power and the length and thickness of the wire. The latter may, in this way, be raised to a red heat, or even fused; and thus, by completing circuit through the wire at the desired moment, a charge of powder may be inflamed. A number of charges may be simultaneously exploded by introducing several pieces of thin wire into the circuit.

Although the employment of a voltaic current of low tension presents obvious and great advantages over old systems of igniting charges by trains or slow-burning fuzes, its application to military purposes is attended with some difficulty and uncertainty, arising out of the want of uniformity of action of the same voltaic arrangements at different periods, the difficulties attending the transport and proper preservation of the battery and materials required for its use, the dependence for success upon care and experience in preparing and preserving the batteries, and the very considerable increase which it is necessary to make in the power of the battery when the operations to be performed involve the simultaneous explosion of a number of charges, or the ignition of gunpowder at very considerable distances from the battery.

For these reasons, soon after the first successful application of voltaic electricity to mining purposes, the attention of military Engineers on the Continent, and of others here and abroad who were specially interested in operations of this kind, became directed to the possibility of rendering electricity of high tension available for exploding purposes, whereby voltaic batteries, for mining operations, might be greatly reduced in size, if not altogether dispensed with. In 1853 a Spanish officer, Colonel Verdu, associated himself with M. Ruhmkorff in experiments on the application of electro-magnetic induction coils to the explosion of gunpowder. The success of their experiments led Colonel Verdu to pursue them further in Spain, where he soon succeeded in firing six mines simultaneously by one element of Bunsen's battery, at a distance of upwards of three hundred yards, through the agency of the Ruhmkorff coil. The mode of operating and difficulties which Colonel Verdu had to overcome will be presently described. While the success of these operations led the military Engineers in Spain, France, and Russia to pursue the development of the application of electro-magnetic induction instruments to exploding purposes, a committee of Austrian military Engineers (of which Baron von Ebner was from the first a most distinguished member) was labouring to apply frictional electricity to military uses as an exploding agent, having come to the conclusion that the electro-magnetic induction apparatus was too complicated and too greatly susceptible of derangement for military uses. But little success had up to that time attended attempts to apply frictional electricity to this purpose. In 1831, Moses Shaw, of New York, succeeded in exploding several mines simultaneously by means of frictional electricity, but was foiled in his attempts to apply this agent to practical purposes, by the fact that he could not conduct operations with any chance of success except in very dry weather. Somewhat more promising results attended several attempts in Germany between 1842 and 1845; but the prospect of practical success was still not encouraging when the Austrian Committee of Engineer Officers took the matter in hand, and eventually produced a portable glass frictional electric machine, which, when in good working order, furnished results surpassing those which had been obtained with volta-induction apparatus. Some very extensive operations were conducted with this machine; thus, fifty land-charges and, afterwards, thirty-six submarine charges, were simultaneously exploded. Even, however with all the precautions adopted, the machine was still too seriously affected by damp to be thoroughly serviceable for military purposes. But the persevering labours of Baron von Ebner eventually resulted in the production of an electric machine which was almost entirely free from the objections hitherto attached to this form of apparatus.

While the progress just indicated was being made in different parts of the Continent in the application of electricity to mining operations, but little attention was directed in this country to effecting improvements in the utilization of electricity for military mining purposes. In 1855, however, Sir C. Wheatstone directed the attention of Field-Marshal Sir John F. Burgoyne to the importance of instituting an experimental inquiry into the relative advantages of different sources of electricity of tension as agents for exploding gunpowder. The Ordnance Select Committee, of whom Sir C. Wheatstone and Mr. Abel were then members, were consequently instructed to pursue this inquiry; and a series of investigations was carried out, in the first instance, to a working branch of the Committee, and subsequently by Mr. Abel at Woolwich and Chatham, the results of which were eventually embodied in a report presented by the above-mentioned gentlemen to the Secretary of State for War in 1860. Since then Mr. Abel, as a member of the Government Committee on Floating Obstructions, has continued systematic investigations on the applications of electricity to the explosion of mines, and especially to submarine operations, and considerable improvements and simplifications of the arrangements and appliances have resulted. Some advance has also been made on the Continent in this subject, and during the last two or three years our military Engineers have acknowledged in the most practical manner the advantages to be derived from the use of electricity of tension as the agent for exploding mines, by gradually and to a very great extent abandoning the old system of operation, and by devoting considerable attention to the practical elaboration of the new systems.

The following is an outline of the results obtained up to the present day with different classes of instruments which furnish electricity of tension.

It has been stated that Colonel Verdu succeeded, in 1853, in exploding several mines simultaneously by means of a Ruhmkorff induction-coil. The ignition of the gunpowder was effected in these experiments by introducing one or more small but complete interruptions into the circuit, across which the electric spark of high tension would leap upon the current being passed. This spark will inflame gunpowder, but not very readily, although its production is attended with development of heat considerably in excess of that required; the reason being that powder requires for ignition either the close proximity of a considerable heated surface, or the continuous application of heat for a brief period, while the disruptive discharge from an induction coil consists of a series of instantaneous discharges following each other in very rapid succession. Hence a charge of gunpowder is not always instantaneously fired when the spark is passed; indeed, unless the powder be closely confined round the wire-terminals between which the spark passes, it is sometimes dispersed by the mechanical action of the spark without being exploded; and when a succession of sparks is passed simultaneously through a number of charges, it frequently happens that only a few are exploded, in which some of the grains happened to be in positions or conditions more favourable with reference to the source of heat than in other instances, where the powder would escape ignition. Colonel Verdu succeeded in increasing the certainty of simultaneous ignition of several charges, by surrounding the wire terminals with a substance much more readily inflamed than powder—the fulminate of mercury. Another source of difficulty in effecting the simultaneous ignition of a considerable number of charges by the spark from the coil is the enfeebling effect upon the spark-discharge exerted by a number of successive small interruptions in the circuit. This was to some extent overcome by employing a fuze constructed by Messrs. Statham and Brunton, in which the space between the wire-terminals was bridged over with a film of a finely-divided substance—the subsulphide of copper, the conducting power of which is sufficiently great to aid the passage of the electric discharge across the interruption, while it is at the same time readily combustible, and therefore directly promotes the ignition of the powder. Finding that, even with the combined use of this fuze and of fulminate of mercury, the power of the induction-coil to explode charges simultaneously was limited, Colonel Verdu adopted the following simple arrangement. Separate small groups of mines were all connected with earth, and an insulated conducting wire connected each group with a distinct small insulated plate. By bringing these plates in very rapid succession into circuit with the coil-machine, the several groups were so rapidly exploded as to produce results somewhat similar to those attainable by the really simultaneous discharge of a considerable number. Not long after this contrivance was adopted by M. Verdu, M. Savare devised another arrangement, whereby a much more rapidly successive discharge of a number of mines was accomplished through the agency of the coil. The metallic circuit which passed to the mines was divided into a number of branches, so that, upon completion of the circuit, the currents, following each other in very rapid succession, would distribute themselves through all the branches with a degree of uniformity regulated by the resistance met with in each branch. Thus, when one or more fuzes were interposed in each branch of the circuit, those which happened to offer the greatest facilities for the passage of the current would be first fired, whereupon the escape of electricity in that direction would be interrupted, and the explosion of fuzes in the other branches would follow. With the employment of currents following each other with the enormous rapidity with which they pass off from the induction-coil machines, the discharge of a number of mines may thus be effected with a rapidity which, practically, has almost the effect of a simultaneous discharge.

The Ruhmkorff coil was used to some extent by the Russians in mining operations during the Crimean war, and some very extensive blasting operations were carried on with its aid at Cherbourg in 1854. A series of experiments was instituted at Woolwich in 1856 with two excellent induction-coils, produced by M. Ruhmkorff, in the course of which various descriptions of materials were tried in the fuzes, for the purpose of increasing the power of the machine to fire numbers of charges simultaneously. At that time the fulminate of mercury was found to be the best inflaming agent; but not more than twelve charges were fired simultaneously by means of the most powerful coil available and a battery of twelve cells (without employing Verdu's or Savare's methods of explosion). One defect in this class of instrument was found to be the want of uniform action of one and the same apparatus at different periods; another was the liability to derangement of the machine, especially of the condenser. Far more successful results were afterwards obtained with the same coils and the fuze constructed at a later period of these investigations; fifteen charges were fired simultaneously with a battery of six cells, and fifty charges, arranged in branch-circuit in groups of ten, were exploded with the effect of a simultaneous discharge. These results were obtained with machines produced by Ruhmkorff in 1855; but the improvements since then effected in the construction of this apparatus have reduced to insignificance the results at that time obtained with it. There is no question therefore that the Ruhmkorff coil is available for special operations of considerable magnitude; but in point of simplicity, certainty, and constancy of action, it is far surpassed by other forms of electric instruments, which will be presently noticed.

At the suggestion of Sir Charles Wheatstone, experiments were commenced in 1856 on the application of currents *induced by permanent magnets* to the explosion of gunpowder. The first experiments were instituted with a very large and powerful magneto-electric machine, constructed by Mr. Henley, of which the armature, carrying two powerful coils, was suddenly detached from the magnet by means of a lever. A few experiments sufficed to show that the induced current obtained even with this powerful intrument was not adequate to ignite one single charge of gunpowder with certainty. Somewhat better, but still uncertain, results were obtained with Statham's and one or two other forms of fuzes existing at that time. A careful investigation was then undertaken by Mr. Abel (with the invaluable assistance of Mr. Brown, of the Chemical Department, Woolwich), into the conditions to be fulfilled in the production of a fuze which should be certain of action with the magneto-electric machine. The results of extensive experiments indicated that a combination of comparatively high conducting power with great susceptibility to ignition appeared to be essential elements of success in a material to be used as the exploding agent in a fuze. The uniform arrangement of the poles or wire-terminals in the fuze, the space between which was to be bridged over by the igniting composition, also proved a matter of great importance. A mode of constructing the fuzes which ensured perfect uniformity in this respect was ultimately perfected, and has proved quite successful. A very fairly efficient fuze was obtained with the aid of the poles thus arranged, by employing as the igniting agent gunpowder impregnated with a small proportion of calcic chloride, which caused it, upon brief exposure to air, to imbibe moisture sufficient to render the gunpowder highly conducting. It is obvious however, that, although the fuze itself was hermetically closed when complete, there must be a liability to want of uniformity in the proportion of water absorbed by the powder, and a consequent variation in the conducting power of the latter. Eventually a material was prepared (consisting of the subphosphide of copper, subsulphide of copper, and potassic chlorate) which combined the essentials of perfect certainty of action with very great sensitiveness to ignition. Henley's large magnet fired *three* of these fuzes simultaneously with perfect certainty, while a small horse-shoe magnet with revolving armature exploded twenty-five in divided circuit in exceedingly rapid succession. A combination of six small compound magnets was afterwards employed, with which an exceedingly rapid succession of currents was obtained, and this apparatus exploded twenty-five fuzes, in divided circuit with a rapidity which to the ear had the effect of an instantaneous explosion. Even the small magneto-electric instruments which are used for medical purposes will explode these fuzes with certainty.

The application of magneto-electric machines having been successfully accomplished, a series of experiments was carried on by Mr. Abel, with the valuable aid of Colonel H. Scott, R.E., at Chatham, during the years 1857–58, on the explosion of charges, both land- and submarine; and the great advantages of these instruments, as regards simplicity and permanent efficiency, over the voltaic arrangements hitherto used, was fully demonstrated. Very compact but powerful exploding instruments were constructed by Sir C. Wheatstone, and these have for the last seven or eight years received many important applications; thus, the proof of cannon at Woolwich and the firing of guns, from a safe distance, in the numerous experiments at Shoeburyness, is effected by means of Wheatstone's exploder, which is, moreover, an important adjunct in all electro-ballistic experiments, when the operator desires himself to fire a gun at a particular moment. Magneto-electric machines have also been found very useful in connection with blasting operations on land, except in instances when the absolutely simultaneous explosion of a large number of mines is required.

Since the success of Wheatstone's exploders has been fully established, several other forms of magneto-electric-machines have been devised, especially on the Continent and in America. Powerful instruments similar to Wheatstone's are manufactured by Siemens and Halske, of Berlin; Markus, of Vienna, has constructed very efficient instruments in which one separation and return of the armature to the magnet are made to explode the charges. The disadvantages of these instruments is that a *succession* of currents cannot be obtained from them as in the case of machines with revolving armatures; hence the number of mines which can be exploded by them in divided circuit is comparatively limited. Mr. Beardslee, an American, has also devised a modification of Wheatstone's exploder, in which the magnets are made to revolve between the armature-coils, and which furnishes currents of greater quantity but lower tension than Wheatstone's. The fuze constructed by Mr. Beardslee for employment with this instrument is similar in principle of construction to Abel's; but the materials which bridge over the space between the terminals or poles of the fuze are black lead, with the addition of a minute quantity of some substance, apparently collodion, which adds to the size of the scintillations produced when the current passes, and thus increases the certainty of ignition of the powder which is in close contact with the poles. These fuzes are efficient with magneto-electric instruments like that of Mr. Beardslee, but they are much less delicate than the Woolwich fuzes, and the number which can be simultaneously exploded is therefore much more limited. Sir C. Wheatstone has also lately constructed more powerful modifications of his original magnetic exploder, which may, at will, be made to furnish currents of greater quantity and lower tension, or to produce the high tension currents. Lastly, Mr. Ladd and Mr. Browning have produced instruments of comparatively low price, but quite powerful enough for ordinary blasting and quarrying operations. The only obstacle, but a most important one, to the general use of these machines for the explosion of mines on land and under water is, that very slight defects in the insulation of the conducting wire which leads from the instrument to the mines are quite fatal to its exploding power. In consequence of the high tension of the current developed by these machines, and the small quantity put into circulation by even the most powerful of them, the diversion of the current from its destined course to earth is promoted by the smallest points of escape presented to it; a result which is, moreover, facilitated by the resistance of the fuzes in circuit. With care this source of failure can be guarded against in operations on land, but such is not the case with regard to submarine arrangements; while, moreover, minute defects in the coatings of the *submerged* wires, which would hardly influence the results at all on land, completely nullify the exploding power of the machines. Hence magneto-electric instruments are the least reliable of all electric exploding apparatus for submarine purposes.

A few experiments were instituted at Woolwich in 1857 on the employment of *frictional electricity* as an exploding agent, and especially with a small hydro-electric machine constructed for the purpose by Sir William Armstrong. As regards its power of exploding a number of charges simultaneously, when it was in good working order, it far exceeded any other instrument experimented with at that time; one hundred fuzes, arranged in a single current, were frequently exploded by its means; but the great uncertainty of its action, and the difficulty of employing it in the field, did not afford encouragement for a continuation of experiments with it.

The great difficulties encountered in the Austrian experiments in the attempts to employ glass frictional electric machines as exploding agents for military purposes, led Baron von Ebner to direct his attention to the production of an instrument in the construction of which glass was altogether avoided, and which might therefore be expected to be less subject to atmospheric influences. His labours in this direction were eventually crowned with success; for he found in the hard vulcanized india-rubber (known as ebonite or vulcanite) a dielectric material excellently adapted to the construction of the frictional apparatus; while by employing a sheet of vulcanized india-rubber coated with tinfoil and compactly rolled up, he obtained without the use of glass a powerful condenser, or Leyden jar arrangement. The improved machines were constructed in a very compact form (with cases excluding all the working parts from direct exposure to air) by Messrs. Siemens of Berlin and Lenoir of Vienna, who exhibited specimens in England in 1862, at which time the electric machine had already received important applications in the Austrian service, and had been regularly adopted for military uses. Baron von Ebner had also, from the commencement of the Austrian experiments, laboured assiduously at the production of an efficient fuze to be used with electricity of tension; and the Austrian service is indebted to him for a simple and thoroughly serviceable fuze, which, as regards the arrangement of its poles and the character of the igniting composition, may be said to combine the principles of the Statham and Abel fuzes. Though less sensitive than the present English service electric fuze, a very considerable number may be exploded in simple circuit by the ebonite electric machine. The power of this apparatus in its portable form is nearly equal to that of the hydro-electric apparatus just now referred to, when the latter is in perfect working order. A far greater number of mines may therefore be simultaneously exploded by its means than by very large batteries or by the most powerful magneto-electric machines hitherto constructed. One hundred of Abel's fuzes have frequently been simultaneously exploded with one of the portable machines, and still greater results can be obtained with a larger instrument having a battery of condensers, which was specially constructed for submarine operations by Mr. Becker, at the suggestion of Captain Maury. In very damp weather, when the most perfect glass electric machines would have been useless unless housed in a warm apartment from which the external air was as much as possible excluded, these ebonite machines have been used from time to time throughout the day with very satisfactory results.

Another important advantage which these instruments possess over magneto-electric machines consists in the fact that very considerable defects in the insulation of even submerged conducting wires do not so greatly reduce the power of the current furnished by them as to interfere with the accomplishment by its agency of the most extensive operations under water which are likely to occur in practice. Unfortunately, however, the very circumstance which constitutes its chief advantage, namely, the powerful character of the current of high tension with which it charges an insulated wire, is also a source of serious defect, to be presently noticed, which very greatly limits the applicability of these machines to naval and military purposes.

A class of electrical instruments has been created within the last three years which bids fair to supplant even these very powerful and efficient frictional machines. The instruments in question, of which different forms have been devised by Wheatstone, Wilde, Siemens, and Ladd, have received the generic name of *dynamo-electric machines*, because dynamic force becomes through their agency a direct and powerful source of electricity. In the machines of Siemens, Wheatstone, and Ladd, mechanical power is transformed into electric force without the intervention of permanent magnets. The action of the most simple form of these instruments may be described as follows:—The residual magnetism existing in an electro-magnet suffices to develop an induced current in a rapidly-revolving coil-armature; this current, reacting upon the electro-magnet, determines the development of powerful magnetism in the latter by the inductive action of its insulated coils; the currents developed by the electro-magnet are consequently in their turn greatly increased in power, and react again upon the armature; and thus an immense accumulation of electric force is accomplished with great rapidity until, when that accumulation has reached the maximum attainable without detriment to the insulation of the wire-coils, a simple interrupting arrangement causes the current to be diverted from the machine to conducting-wires, by whose medium it is utilized. The details of the machines vary according to the different plans adopted by the several constructors, but the above explanation applies more particularly to the machines of Messrs. Siemens and Halske, who have been the first to produce a small instrument of this class thoroughly applicable to mining purposes, and which almost equals in power the ebonite frictional-electric machine. Fifty charges, arranged in simple circuit, have been repeatedly exploded, without any failures, by one of these machines; it therefore provides with certainty the power necessary for the most extensive mining or submarine operations, and is at the same time quite free from all disturbing atmospheric influences. Its mechanism is simple, and less easily susceptible of derangement than that of any magneto-electric apparatus, and as it is independent of everything but the application of manual power for the development of its action, it is far superior to the most perfect of these, independently of the fact that it surpasses them all greatly in power. For many important military and mining operations the hand dynamo-electric machines, constructed by Messrs. Siemens and Halske, are therefore unquestionably superior to all other existing apparatus which furnish electricity of tension. This class of instrument, however, shares, to some extent at any rate, one important defect of the frictional-electric machines, which is consequent upon the powerful charges of high-tension electricity sent into conducting wires by their agency. It was observed, in the earlier experiments of the Austrians with frictional electricity, that if two or more insulated wires which led to distinct mines were situated side by side, in moderately close proximity, even only for comparatively short distances, the charge sent from the machine into one of the wires, with the view of only exploding a particular mine or series, might develop in neighbouring wires, not connected with the machine, a charge of induced electricity of sufficient power to explode the mines connected with those wires. Some results obtained at Chatham, and many experiments recently instituted at Woolwich, have not only confirmed those observations in Austria, but have shown that means do not at present exist of avoiding this serious defect of powerful charges of high tension. If, therefore, it is necessary to lead separate wires from the point of operation (from the exploding instrument) to different mines or groups which it is desired to explode independently of each other, it is impossible to employ the frictional electric machine as the exploding agent without great risk of failure, even though the wires, laid upon or imbedded in earth, are separated as widely as possible, as they must unavoidably extend in proximity to each other to a distance from their points of union with the machine, which, if the latter be highly charged, may prove sufficient to determine the development of induced "exploding" currents in the wires leading to mines not intended to be fired. If the wires lead to submarine mines or torpedoes, and are therefore submerged, the unintentional explosion of mines becomes much more certain, and the frictional machine is consequently inapplicable to submarine operations in all instances where mines are arranged in separate circuits. The dynamo-electric machines share this serious defect to some extent; still, with proper experience in their use, they are not altogether inapplicable to such services as above specified. If Siemens' dynamo-electric machine be highly charged by very rapid revolutions of the armature, the inductive action of the charge will be similar in power to that exerted by the charge of the frictional machine, but by revolving the armatures slowly it is possible to charge the machine sufficiently to fire a mine or a small group with certainty, while the inductive action of the charge sent into the wire will not influence neighbouring wires to an extent sufficient to cause the explosion of mines connected with them.

The subject of the application of electricity to the explosion of submarine mines for purposes of defence and attack received some attention from the Russians during the Crimean war, and was practically developed in its most simple form in 1859, in the hands of the Austrian government, when a system of submarine mines, to be fired through the agency of electricity by operators on shore, was applied by Baron von Ebner to the defence of Venice, which, however, never came into practical operation. The subject of the utilization of electricity for purposes of defence did, however, not receive serious consideration in England or other countries until some years afterwards, when the great importance of submarine mines as engines of war was demonstrated by the number of ships destroyed and injured during the war in America. Twenty-five vessels belonging to the Federal navy were destroyed and nine others injured by the explosion of torpedoes, while the Confederates lost three vessels by accidentally coming into collision with their own torpedoes, and one which was attacked by means of a torpedo and destroyed by the Federals.

Soon after the commencement of that war the attention of the English government was called to the importance of practical inquiry into the value of submarine obstructions, both passive and active, as auxiliary agents of defence, and a Committee was appointed at the suggestion of Colonel Jervois, C.B., R.E., to report on the use which might be made of floating or sunken obstructions and of submarine mines, in the defence of channels, harbours, and rivers. The labours of this Committee have recently terminated, and they were enabled, by the aid of systematic investigations conducted for them at Woolwich during the last four years by one of their members, Mr. Abel, and of practical experiments carried on chiefly at Chatham under the direction of another of their body, Colonel A. à'C. Fisher, C.B. R.E., to elaborate the subject of the application of electricity to submarine mines and torpedoes, to such an extent that a solid foundation of information and instruction is now available for those who may at any time have to be entrusted with the actual arrangement and employment of these important means of defence. Continental Governments have also devoted much attention to this subject, and especially the

Austrian Government, for whom Baron von Ebner devised an ingenious and elaborate system of electric torpedo defence, exhibited in detail at the Paris Exhibition of 1867, which received application during the recent war, though its efficiency was not actually put to the test except in the way of experiment.

The application of electricity to the explosion of torpedoes was very limited during the American war; but arrangements for the extensive employment of that agent as the exploding power were far advanced in the hands of both the Federals and Confederates at the close of that war. It appears that only in two instances of the entire number of vessels destroyed and injured was the explosion of the torpedoes effected by electric arrangements, the others having all been exploded by mechanical agency.

The explosion of submerged charges of gunpowder by mechanical contrivances, either of a self-acting nature or to be set into action at desired periods, was accomplished as far back as 1583, during the siege of Antwerp by the Duke of Parma. The English employed self-acting torpedoes against the French ships off Rochelle in 1628, and from that period to 1854 devices of more or less ingenious and practicable character have been proposed from time to time, and even applied to some small extent in different countries, for the explosion of torpedoes either by clockwork, at fixed periods, or by coming into collision with a ship. The Russians were the first to apply self-acting mechanical torpedoes with any prospect of success, and there is little doubt that had the machines which were applied to the defence of the Baltic been of larger size (they only contained 8 or 9 lbs. of gunpowder), their presence would have proved very disastrous to some of the English ships which came into collision with and exploded them. Various mechanical devices for effecting the explosion of torpedoes by their collision with a ship were employed by the Americans during the recent war, a few of which proved very effective. But although, in point of simplicity and cost, a system of defence by means of mechanical torpedoes possesses decided advantages over any extensive arrangements for exploding submarine mines by electric agency, their employment is attended by such considerable risk of accident to those at whose hands they receive application that, under many circumstances which are likely to occur, they become almost as great a source of danger to friend as to foe. Thus the operations of lowering and mooring torpedoes, the explosion of which depends upon the application of a blow, thrust, or pull to some portion of the machine, which is so placed and arranged as to be in a favourable position for the application of mechanical action by a passing ship, are attended with very great danger to those employed, unless some means are adopted for rendering the exploding mechanism inactive until after the torpedo has been placed in position. But the employment of a safeguard of this kind involves a considerable amount of uncertainty as to the torpedo being rendered active by its removal after the operation of mooring is completed, because this very removal is frequently a dangerous operation. Again, when once the mechanical torpedoes have been placed in position and rendered active, they are as dangerous to friendly ships as to the enemy; consequently their employment for the defence of a particular tract of water completely closes it until the torpedoes have been exploded or removed, and their removal obviously constitutes one of the most dangerous services upon which men can be employed. Several instances have recently occurred in America of the destruction of ships in waters defended during the war by mechanical torpedoes, of which it was believed that the subsequent removal had been completely accomplished. Some improvements have recently been made in mechanical and chemical appliances of a self-acting nature for torpedoes, by the employment of which the mooring arrangements can be completed with perfect safety, and the torpedoes afterwards rendered active, by the performance of a simple and safe operation when it is desired to close the defended water. But the complete exclusion of friendly vessels, and the difficulties attending the raising of the torpedoes when no longer required, still constitute formidable objections against the use of mechanical torpedoes, excepting in the case of tracts of water which are not ordinarily navigated, but the passage of which in times of war might be attempted by vessels of light draft.

The most important advantages secured by the application of electricity as the exploding agent of submarine mines and torpedoes are as follows:—They may be placed in position with absolute safety to the operators; they may be rendered active or passive at any moment from the shore; the waters which they are employed to defend are therefore never closed to friendly vessels until immediately before the approach of an enemy; they can be fixed at any depth beneath the surface (while mechanical torpedoes must be situated directly or nearly in the path of a passing ship), a circumstance which very considerably simplifies the arrangements for their application in tidal waters; lastly, electric torpedoes may, when no longer required, be removed with as much safety as attended their application.

There are two distinct systems of applying electricity to the explosion of torpedoes. The most simple is that in which the explosion of a torpedo is made dependent upon the completion of the electric circuit by operators stationed at one or more posts of observation on shore. The particular mode of arrangement and the operation to be adopted depend in great measure upon the nature of the locality to be defended by torpedoes. If this be a river or channel, the plan of arranging and exploding torpedoes is comparatively simple, but will serve sufficiently to illustrate the general nature of this system of applying torpedoes. The mines are arranged across the river or channel in rows or lines, converging towards a station on shore, to which the conducting wires are led which are to connect each torpedo with the exploding instrument. The operator at this station has it in his power, therefore, to explode any one of the torpedoes at will, by completion of the circuit through the particular cable and the earth. Some other position on shore is selected as a second station, which commands points of view intersecting the lines of torpedoes. The operators at the two stations are placed in telegraphic communication with each other, and when a ship is observed by the operator at the second station to approach in the direction of any one of the torpedoes, he will signal to the man who looks along this line of torpedoes, and the latter will complete circuit as soon as the vessel appears over the particular torpedo specified. Should the vessel alter her course in approaching the torpedoes, the operator at the observing station will inform the man at the firing station, who will alter his arrangements accordingly. Or, the man at the observing station, when he perceives a vessel to approach in a line with any of the torpedoes, places the cable of that torpedo in electric connection with the operator at the other station, and the latter will complete the circuit through the earth to the torpedo as soon as he sees that the vessel is over the first line of torpedoes. Other more or less elaborate modifications of these modes of observing and exploding have been proposed; they all depend for efficiency upon the experience, harmonious action, and constant vigilance of the operators at the exploding and observing stations. They are, moreover, entirely useless at night, and in any but clear weather. They are therefore not to be compared, in general efficiency, with self-acting electric torpedoes, which are either exploded by their collision with a ship, whereby electric circuit is completed within them, or by the vessel striking a circuit-closing arrangement moored near the surface of the water, whereupon either the torpedo, moored at some depth beneath, is instantly exploded, or a signal is furnished at the station on shore, which indicates to an operator the particular torpedo to be exploded. The object to be attained in these circuit-closing apparatus, which are so moored as to be within range of a passing ship, is to oppose in the path of a vessel a contrivance which will not be affected by the motion of the water, but which will complete electric circuit between the conducting cable and the fuze, if struck in some particular part, or thrown into a particular position, by the advancing ship. Numerous ingenious contrivances have been proposed for this purpose and experimented with, but in only two or three instances have satisfactory results been attained, the conditions essential to success being numerous, and their combined fulfilment not easy of attainment. Simplicity of mechanism and a combination of sufficient, but not excessive, delicacy of action, with permanence during long immersion, are among the most important objects to be aimed at in the construction of these circuit-closing or signalling machines or self-acting torpedoes, which, if efficient, must contribute most importantly to the success of any arrangements for defence of a water by electric torpedoes.

It has been shown that magneto-electric instruments cannot be relied upon for submarine operations, on account of the perfect insulation of the conducting-wires, joints, &c., required to ensure success with machines of that class. On the other hand, frictional machines and also dynamo-electric machines leave little to be desired as regards power to effect even the simultaneous ignition of numerous submarine mines, through cables in the insulation of which, from long-continued use, some defects exist. These instruments therefore are available as most efficient instruments when any extensive submarine operations have to be accomplished; but the frictional machines cannot be used as the exploding agents in connection with any system of defence by torpedoes, which depends for its efficiency upon the explosion at the proper moment of only the particular torpedo over which a vessel passes, while all surrounding torpedoes still remain intact; because, for the reason which has been given, the explosion of the proper torpedo will almost invariably be attended by the accidental explosion of others which it is not desired to bring into operation. The same objection applies, at any rate to some extent, to the dynamo-electric machines. These two classes of instruments are therefore only susceptible of certain special applications in connection with submarine mines. There is, moreover, another general objection to the use of any source of electricity, the action of which is entirely dependent upon an operation to be performed at the instant that an electric discharge is required. This consists in the fact that, although the torpedoes may be self-acting, their efficiency is still dependent upon the vigilance and presence of mind of an operator on shore.

The only sources of electricity which thoroughly fulfil the conditions essential to its application with perfect confidence, in connection with self-acting torpedoes, are constant voltaic batteries. By substitution of the Abel fuze for the old platinum-wire fuze, it has become possible to use batteries which were previously inapplicable to the explosion of mines, because, even when employed in considerable numbers, the quantity of electricity furnished by them is not sufficient to effect the ignition of platinum wire. Thus, a number of elements of a Daniell's battery or a sand battery, quite incapable of heating a platinum wire to redness, fires an Abel fuze with perfect certainty. The heat developed in the latter by the passage of a current from such a battery amply suffices to raise to its igniting point the readily explosive priming mixture which serves as the conductor in the fuze. Moreover the resistance presented by the fuze is so considerable in comparison with that offered by the longest cables which are likely to be used in actual practice, that a current from a battery which possesses tension sufficient to overcome the resistance of the fuze will explode the latter with as much certainty, through cables of great length, as when it is close to the battery. A number of cells of a Bunsen battery, sufficient to ignite a piece of platinum wire several inches in length, when close to the battery, the current of which possesses also sufficient tension to ignite an Abel fuze, will be incapable of rendering a very short piece of thin platinum wire even moderately warm, if four or five hundred yards of ordinary conducting-wire be introduced into circuit; but its power of exploding an Abel fuze will not have become at all affected. It is evident from this illustration that the necessity for greatly adding to battery-power, when mines are to be exploded through considerable lengths of wires, which exists with the use of the wire-fuzes, is obviated by employing the new fuze; and thus one great objection to voltaic batteries, as exploding agents in mining operations, is set aside. Again, the sand batteries, or Daniell batteries, which are used for telegraphic purposes, and which, when once charged, continue, with very little attention, in constant and good working action for several months, may now be substituted for the batteries (*e.g.* Grove's or Bunsen's) which it was formerly necessary to employ in order to attain sufficient quantity of current, and which only continue in good action for a few hours. Sand batteries have been repeatedly employed at Woolwich for the explosion of fuzes, after having been in action four or five months, with the occasional addition of a little water to compensate for evaporation.

It will be seen, from the foregoing, that constant voltaic batteries combine more thoroughly the essential qualifications of efficient exploding agents, in connection with any system of submarine defence, than all other sources of electricity at present known. They are simple of construction, inexpensive, require but little skill or labour in their production and repair, and very little attention to keep them in constant good working order for long periods. Their action is quite independent of any operation to be performed on shore at the last moment; it is only necessary to place the cables leading to the torpedoes in connection with the battery when it is desired to close a defended water,—the circuit-closing portion of the torpedo, upon being struck by a ship at any time, will then cause the instantaneous explosion of the charge. The defence by torpedoes thus becomes as effective by night as by day; moreover, the efficiency of the constant batteries is not more prejudicially affected by the existence of defects in the insulation of the cables, than that of the frictional electric machines; and they may be used without incurring any risk of the unintentional explosion of torpedoes by induced currents.

Simple and powerful forms of batteries are readily extemporized, and there is no more portable, simple, or economical description of exploding instrument than the ordinary volta-pile, for the construction and employment of which it is only necessary to provide a piece of hard timber, some zinc and copper sheet, an old blanket, and some vinegar and common salt. A pile, composed of 120 elements, the plates being 2¾ in. diameter, is very portable, and suffices to explode a mine in single circuit, or three or four arranged in branch circuit. It will remain in good action for at least twenty-four hours, and is readily and expeditiously cleaned and re-charged. This apparatus has become a favourite exploding instrument with sailors, being easily constructed and charged anywhere, and very handy for boat-operations (in connection with the employment of torpedoes as an arm of attack), in which service more delicate instruments speedily lose in efficiency. Larger piles constructed on precisely the same plan are now being used in some ships of war for the simultaneous discharge of guns, and a very small form of pile, with water only as the exciting agent, is the most convenient instrument for testing the fuzes and cables of torpedoes after they are in position. It is a matter of great importance that a positive knowledge of the efficiency of a torpedo and its conducting wire should be obtained from time to time by electric tests, and there is now no difficulty in including the fuzes themselves in the test. Signals may, in fact, be readily passed from one firing station to another through the fuze in a submerged torpedo, which is arranged to be fired at will from the shore.

It does not come within the scope of this discourse to enter upon a discussion of numerous important subjects connected with the actual use of electric torpedoes, or of the considerations involved in the question as to how these formidable agents of defence may be most efficiently applied, in addition to, or in the absence of, artillery defences. The object of the discourse will have been attained if it has been satisfactorily demonstrated that electrical science is destined to contribute most invaluably to the efficiency of a country's defences.

[F. A. A.]

WEEKLY EVENING MEETING,

Friday, March 19, 1869.

Admiral Sir Henry John Codrington, K.C.B., Manager, in the Chair.

Dr. A. Crum Brown, F.R.S.E.

On Chemical Constitution, and its Relation to Physical and Physiological Properties.

Chemists have long endeavoured to answer the question, What is the relation in which the constituents stand to one another in a compound? and numerous hypotheses more or less ingenious have been devised for this purpose. Two of these modes of representing chemical phenomena occupy so prominent a place in the history of the science as to merit special notice, even in so slight and hurried a sketch as this must be. These are, 1st, the Electro-chemical and Radical Theory; and 2nd, the Theory of Atomicity and Chemical Structure.

The first was the product of the genius, learning, and laborious research of Berzelius; it was soon adopted by all chemists, and formed for many years the foundation of all chemical teaching and the guide in all chemical work. The point of view from which it regards chemical phenomena is that of combination and decomposition, of the union of elements to form compounds and the separation of compounds into elements. A very important form of chemical decomposition is electrolysis, or the breaking up of a compound by means of current electricity. From the nature of the case electrolysis gives rise to a dichotomous decomposition, and this duality was extended to all cases of combination and decomposition. Elements combine with each other in pairs; these pairs may again combine in pairs, forming compounds of the second order, and so on. Thus calcium combines with oxygen to form lime, sulphur combines with oxygen to form sulphuric acid, and sulphuric acid combines with lime to form sulphate of lime. This union of compounds with compounds was not supposed to depend on a union of the constituents of the one with the constituents of the other, but to be a combination of the one as a whole with the other as a whole; not a combination of the calcium of the lime with the sulphur or with the oxygen of the sulphuric acid, or of the sulphur of the sulphuric acid with the oxygen of the lime, but of the lime as such with the sulphuric acid as such.

This view may be illustrated by a reference to the relations of human life. Individuals unite to form partnerships or corporations, and these may again enter into alliances, although the members of the one allied corporation may be altogether unacquainted and unconnected with the members of the other.

But the progress of discovery brought to light facts which seemed to contradict this view of binary combination. Cases were observed in which a compound of two elements united directly with an element, and to meet this new class of facts the theory was modified by the introduction of the notion of Radicals. A Radical was a compound which acts like an element.

The simile introduced above may be used to illustrate this extension of the theory. Some combinations of men (corporations) can be treated as individuals, can enter into legal relations with individuals, while others cannot; so some compounds can unite with elements, while others have not this capability.

The Theory of Atomicity regards chemical phenomena from an altogether different point of view. In it the various substances are considered as modifications of one another rather than as compounds. The rise of this mode of viewing chemical phenomena may be traced from the early papers by Dumas, and by Laurent, on Substitution. It appears more prominently in the position given to double decomposition as the representative of all chemical action, by Laurent and Gerhardt, in the types of Gerhardt and Williamson, in Frankland's theory of the organo-metallic bodies, and in its extension by Kolbe to the compounds of carbon. It was reserved, however, for Kekulé to combine these ideas into a consistent theory.* The theory has been further elaborated by Butlerow, to whom we owe the name "Chemical Structure," by Erlenmeyer, and by many others, and it has been adopted and applied with slight modifications by almost all chemists engaged in organic research.

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* It is right to observe that although Kekulé has used this theory with the most eminent success, both in the explanation of facts already known, and in the discovery of new chemical relations, he does not exclude the possibility of the union of compounds with each other to form compounds of a second order.

According to this theory the typical form of chemical action is what we may call the *chemical exchange.* To illustrate this idea we may consider the simplest case; that of double decomposition where two molecules act on one another to produce two new molecules.

Chloride of sodium, for instance, acts on nitrate of silver, producing chloride of silver and nitrate of sodium. Comparing chloride of sodium and chloride of silver, we at once see that while there are important respects in which the sodium and the silver differ as to the nature of their union with chlorine (thus the amount of work required to separate the metal from the chlorine is very different in the two cases) still, from one point of view (and that is the point of view taken by the atomicity theory), the silver may be said to replace or to be substituted for the sodium. In the same way a cup filled with mercury is very different from the same cup filled with water; and the relation of the mercury to the cup differs in many respects (such as pressure and adhesion) from the relation of the water to the cup; but they agree in this, that the cup is *filled* in both cases. In the same way the chlorine is said to be *saturated* by the sodium or the silver, although the intimacy or firmness of the combination is not the same in the two cases.

We may also consider this double decomposition from the other side. As the silver and sodium have changed places, so the chlorine has changed place with the *rest* of the nitrate of silver—with what in the nitrate of silver is not silver; or, representing the action in symbols ($NaCl + AgNO_3 = AgCl + NaNO_3$) Cl and NO_3 have changed places.

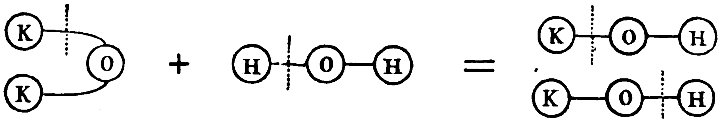
In this example we have one atom or group replacing one other atom or group; but all cases of double decomposition are not of so simple a kind. Thus, when water is treated with pentachloride of phosphorus we find that *one* atom of oxygen (from the water) replaces, and is replaced by, *two* atoms of chlorine from the pentachloride; thus, $PCl_5 + H_2O = PCl_3O + 2\ HCl$. So that while the two atoms of hydrogen were formerly united to one atom of oxygen and formed with it one molecule, they are, after the change, each united to a separate atom of chlorine and form with them two molecules.

Oxygen, therefore, in this case (and, as far as we know, in all cases) enters into two relations, while hydrogen, chlorine, silver, and sodium only enter into one. In a similar way it has been shown that the different elements have different "atomicities" or enter into different numbers of relations. It is to this "polyatomicity," or *multiple-relatedness*, that the complexity of compounds is due; for it is obvious that by the union together of several multiply-related atoms a very complicated structure may be produced.

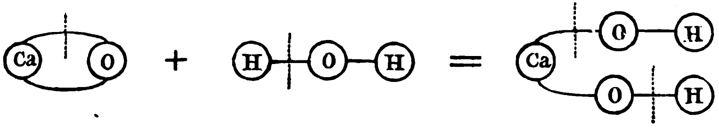
In the case of a compound containing only two atoms, such as chloride of sodium, there is clearly only one way in which it can break into residues; but a complex substance containing many atoms may, and generally does, break in different ways when acted on by different substances; and it is by the study of these ways of decomposition of a substance, and by the study of the ways in which by means of double decomposition it can be produced, that we arrive at a knowledge of its structure, that is, of the mutual relation of its atoms.

But the multiple-relatedness of some atoms produces a further complication, producing a kind of chemical action, which, while still a chemical exchange, cannot be called double decomposition. In double decomposition we saw that each molecule breaks into residues which change places with the residues of the other molecule, and that this breaking into residues results from the rupture of one or more relations between pairs of atoms. But where we have multiply-related atoms, it may happen that such a rupture takes place without a separation of the residues, these being retained in combination by some other relation of their multiply-related atoms. To illustrate this we may compare the action of anhydrous potash, K_2O, and of anhydrous lime, CaO, on water.

Using graphic formulæ, we have in these two cases:—



And,



Here the dotted lines indicate the relations ruptured; and it will be seen that, while in the first case the rupture produces a separation into two residues, in the second case it does not; what would otherwise be residues remaining united, on account of the double-relatedness of the calcium atom.

From this examination of chemical exchange, it will be obvious that no operation of this kind can produce a change in the "atomicity" of an atom; for, for every relation ruptured, a new one is entered into. But we have no reason to suppose that all chemical action is of this kind; and there are numerous phenomena which it is very difficult to explain, except by the assumption that there is another kind of chemical action, in which the number of relations of an atom is increased or diminished. Such actions are those by which we pass from one series of compounds to another. Thus the ferrous salts are connected together by processes of exchange; but it is only by making new hypotheses that we can thus explain the passage from the ferrous to the ferric salts. Similar relations exist between the manganous salts, the manganic salts, the manganates, and the permanganates, where a consideration of each group, apart from the others, would lead us to a different atomicity for manganese; and many other examples might be given of the same kind. The speaker considered it, in the mean time, to be better to regard each such series separately, rather than, by an attempt to bring all chemical processes under one class, to endanger the stability of the theory of chemical structure which, while it is probably not destined, in its present form, to remain as a permanent part of the great edifice of the science, is certainly a most convenient scaffolding, not easy to replace, and not hastily to be thrown down.

Having thus seen what is meant by chemical structure, and how we arrive at a knowledge of it by a study of the *history* of the substance, of the ways in which it may be formed and in which it may be decomposed; we may now glance at the relations which exist between the chemical structure of a substance and its physical and physiological properties. We shall consider specially two of the physical characters of matter, volatility and colour, and examine in what way these are modified by the performance upon the substance of certain specified chemical operations. The volatility of a substance depends upon two things:—1st, the temperature at which the substance boils under a particular pressure; and, 2nd, the change of boiling-point produced by a change of pressure. In order, therefore, fully to know the volatility of a substance, its boiling-point must be determined through a very great range of pressure. This involves great labour; and only a few substances have been thus fully examined. Almost all we know on this interesting question is due to the ingenious and patient experiments of Regnault. These do not, as yet, furnish us with sufficient data to enable us to deduce anything like a law. They show us, however, that a mere comparison of boiling-points under an arbitrarily-selected pressure (such as 760 millimètres, which happens to be the mean pressure of the atmosphere) cannot lead us to a law, as the boiling-points of two substances are frequently changed very unequally by a change of pressure.

Such comparisons of boiling-points have been made, and from them have been deduced, especially by Kopp, a series of very interesting, and certainly not fortuitous coincidences. That distinguished chemist and physicist has shown that, in a very large number of instances, the same change of chemical structure produces nearly the same change of boiling-point. These "laws" of Kopp are only approximate, and are not even approximate in the cases where the boiling-points of the substances compared are very differently changed by change of pressure.

Turning to the other physical character which has been mentioned, namely colour, we see at once a marked regularity. As a rule, substances belonging to the same series differ from one another in degree rather than in kind of colour; while in passing from one series to another, we observe that the colour undergoes a total change of character. This is well illustrated by comparing the colours of substances belonging to such series as the ferrous salts, the ferric salts, the ferrates; the manganous salts, the manganic salts, the manganates and the permanganates; the cuprous and cupric salts, the chromous and chromic salts, the chromates and perchromic acid. Possibly such changes of colour as we see in the transformation of rosaniline and its derivatives into leukaniline and analogous bodies, and of blue into white indigo, may be cases of the same kind. It is also interesting to note, that while the nitro-substitution products of the aromatic series are generally yellow, all the known substances of the same kind in the fatty series are colourless.

These considerations of colour would naturally incline us to regard the operations which lead from one series to another as different in kind from those which lead from one member to another of the same series; and when we examine the physiological action of bodies of the same, and of different series, this impression is greatly strengthened.

The speaker described in some detail a few of the observations made within the last two years by Dr. T. R. Fraser and himself, pointing out the similarity of the action of substances belonging to the same series, and the remarkable change of physiological action produced by those chemical changes which lead from one series to another. The illustrations were drawn from the natural alkaloids—a group of substances containing trebly-related nitrogen, and those derivatives of the alkaloids which contain fivefold-related nitrogen. It was shown that the salts of the alkaloids, although containing fivefold-related nitrogen, were not adapted for this comparison, on account of the readiness with which they lose acid in the presence of alkaline substances, their nitrogen thus returning to the trebly-related condition. The bodies formed by the addition of a compound of methyl have not this disadvantage; and as the nitrogen in them is *permanently* fivefold-related, their physiological action may be satisfactorily compared with that of the alkaloids themselves.

The experiments leading to a knowledge of the action of strychnia and of the salts of methyl-strychnium were described; and it was shown that while the former acts by *exciting* the *origins* of the motor nerves in the spinal cord, the latter act by *diminishing the action* and ultimately paralyzing the *terminations* of the same nerves in the muscles. Similar relations exist between brucia and methyl-brucium, thebaia and the salts of methyl-thebaium, morphia and the salts of methyl-morphium, &c. Indeed, it may be stated generally that, as far as observation goes, compounds of trebly-related nitrogen exert an action totally different in kind from similar compounds of fivefold-related nitrogen, that a similar difference exists between the triatomic and pentatomic compounds of other members of the nitrogen family, and that this principle appears to be of still wider, and probably general, application.

The speaker, in conclusion, drew attention to the peculiar interest attaching to those regions of science which lie on the frontiers between two distinct departments, as on their successful exploration would depend the ultimate fusion of all physical sciences into one, the science of dynamics, the science which treats of matter and energy, and their relations to one another. Such a fusion is probably very remote; but we now see in the border-land between chemistry and physics that slow process of absorption going on which has already converted the once independent sciences of sound, light, heat, electricity, and magnetism into more or less completely subjugated provinces of the great empire of applied mathematics. If we believe in the unity of the plan of creation we must believe that this process will advance and ultimately triumph.

[A. C. B.]

GENERAL MONTHLY MEETING,

Monday, April 5, 1869.

Sir Henry Holland, Bart. M.D. D.C.L. F.R.S. President, in the Chair.

Charles Chapman, Esq.
Mrs. Mary Cunliffe,
Walter Graham, Esq.
Archibald Hamilton, Esq.
Henry Stone, Esq.

were *elected* Members of the Royal Institution.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

Agricultural Society of England, Royal—Journal, Second Series. No. 9. 8vo. 1869.

American Philosophical Society—Proceedings. No. 80. 8vo. 1868.

Astronomical Society, Royal—Monthly Notices. Vol. XIX. No. 4. 8vo. 1869.

Bavarian Academy of Science, Royal—Sitzungsberichte, 1868. Band II. Heft 3, 4. 8vo.

Belgique, Académie Royale de—Bulletins. Tome XXV. XXVI. 8vo. 1868.
Almanach. 1869. 12mo.

British Museum Trustees—Catalogue of Carnivorous, Pachydermatous, and Edentate Mammalia. 8vo. 1869.

Chemical Society—Journal for March, 1869. 8vo.

Davis, Alfred, Esq. M R.I.—La Géographie du Talmud. Par Adolphe Neubauer. 8vo. Paris, 1868.
Treasures of Oxford (Hebrew Poems, with Translations). 8vo. 1851.
Jewish Reply to Dr. Colenso's Criticism on the Pentateuch. 8vo. 1865.

Editors—Artizan for March, 1869. 4to.
Athenæum for March, 1869. 4to.
British Journal of Photography for March, 1869. 4to.
Chemical News for March, 1869. 4to.
Engineer for March, 1869. fol.
Geological and Natural History Repertory. March, 1869. 8vo.
Horological Journal for March, 1869. 8vo.
Journal of Gas-Lighting for March, 1869. 4to.
Mechanics' Magazine for March, 1869. 8vo.
Pharmaceutical Journal for March, 1869. 8vo.
Photographic News for March, 1869. 4to.
Practical Mechanics' Journal for March, 1869. 4to.
Revue des Cours Scientifiques et Littéraires. March, 1869.
Meteorological Society—Proceedings, Nos. 40, 41. 8vo. 1868.
Moore, Charles H. Esq. F.R.C.S. M.R.I.—Dr. Tregelles's Greek Testament. Part IV. 4to. 1869.
Peacock, R. A. Esq. (*the Author*)—Physical and Historical Evidences of Sinkings of Land. 16mo. 1868.
Quetelet, M. A. Hon. Mem. R.I. (*the Author*)—Phénomènes Périodiques, 1865–6. 4to. 1868.
Annales de l'Observatoire Royale de Bruxelles. Année I. II. 4to. 1867–8.
Royal Society of London—Proceedings, No. 109. 1869.
Sandys, S. Esq. (*the Author*)—A Problem for Trisecting an Angle, &c. 8vo. 1869.
Scottish Society of Arts, Royal—Transactions. Vol. VII. Parts 4, 5. 8vo. 1868.
St. Petersbourg, Académie Impériale de—Mémoires, VII. Série. Tome XII. Nos. 1, 2, 3. 4to. 1861–8.
Bulletins. Tome XIII. Nos. 1–3. 4to. 1868.
Symons, G. J., Esq. (*the Author*)—Symons' Monthly Meteorological Magazine, March, 1869. 8vo.
British Rainfall, 1868. 8vo. 1869.
Tyndall, Professor, LL.D. F.R.S. M.R.I.—Der Schall: von John Tyndall; autorisirte Deutsche Ausgabe, herausgegeben durch H. Helmholtz und G. Wiedemann. 8vo. Braunschweig, 1869.
Vienna, Geological Institute—Verhandlungen: Jahrgang, 1868. 8vo. Nos. 14–18.
Jahrbuch, 1868. Nos. 3, 4. 8vo.

Royal Institution of Great Britain.

WEEKLY EVENING MEETING,

Friday, April 9, 1869.

Sir Henry Holland, Bart. M.D. D.C.L. F.R.S. President, in the Chair.

William B. Carpenter, M.D. V.P.R.S.

On the Temperature and Animal Life of the Deep Sea.

The results of recent inquiries into the condition of the abyssal depths of the Ocean, tend to prove:—that notwithstanding their apparently profound stillness, undisturbed by the storms which agitate the surface, an incessant motion is everywhere taking place in them, which exerts a most important moderating influence on what would otherwise be the intolerable heat of the Equatorial and the unbearable cold of the Polar regions, and thus affects the distribution of Animal and Vegetable life, alike on land and in the ocean waters:—that the line of demarcation which has been supposed to separate their dreary wastes from the comparatively shallow stratum (300 fathoms) to which life has been affirmed to be restricted, has no real existence; but that whilst the ocean bottom in one region may be as barren as Sahara, it may in another, though at ten times the depth, be teeming with varied forms of organization:—that notwithstanding the tremendous pressure exerted by the superincumbent mass (amounting to nearly 3 lbs. per square inch of surface for every fathom of depth, and thus to 3000 lbs. per square inch at a depth of 1100 fathoms), animals of the softest conceivable texture can "live and move and have their being" without suffering any inconvenience from the burden:—that not even the total privation of Light, which the highest authorities have affirmed to be essential to the existence of life,[*] prevents these rayless depths of ocean from supporting a vast and continuous mass of Animal life, which is actively engaged in forming a Calcareous deposit over the bed of the Atlantic; a deposit which, when hereafter raised above the waters, so as to form dry land, will be designated by the Geologist of the future as Chalk.:—and lastly, that in these abyssal depths are preserved in continued existence many types of organization which had been supposed to have long since become extinct; revealing themselves as living denizens of the present epoch, when brought to the surface by the searching dredge of the Naturalist, just as the reminiscences of our past lives, stored up in the hidden chambers of our "under soul," are called up in the process of recollection when groped for with the right clue of Association.

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[*] "Organization, sensation, voluntary motion, life," said Lavoisier, "only exist on the surface of the earth, and in places exposed to light. It might indeed be said that the fable of Prometheus was the expression of a philosophical truth which had not escaped the penetration of the ancients. Without light, nature were without life and without soul; a beneficent God, in shedding light over creation, strewed the surface of the earth with organization, with sensation, and with thought."—"These words," says Dumas ('The Chemical and Physiological Balance of Organic Nature,' p. 8), "are as true as they are eloquent."

It had been the good fortune of the speaker to bear a part in these inquiries: a proposal for a deep-sea dredging expedition, which originated with his friend Prof. Wyville Thomson, of Belfast, having been brought by him last June before the President and Council of the Royal Society, and by them strongly recommended to the Admiralty; which, on the strong recommendation of the Hydrographer, assigned for the service the surveying vessel 'Lightning,' and provided her with the best appliances that could be got ready on so short a notice. The work of the expedition, which was placed under the scientific charge of the speaker, was specially directed to the exploration of the deep channel which lies between the north of Scotland and the Faroe Islands; and although necessarily limited in time, and much interfered with by bad weather, it afforded results of great interest, in regard both to the Physics and the Animal Life of the Ocean depths.

I. The current belief among Physical Geographers in regard to the temperature of the *deep sea* had been that it is everywhere 39°; *descending* towards that point in the Equatorial region, from a surface temperature of between 75° and 85°, in proportion to the depth to which the thermometer is sunk, and falling to 39° at about 1200 fathoms; whilst it *ascends* towards that point in the Polar region, as the thermometer sinks through the ice-cold waters nearer the surface, until it rises to 39° at a depth of about 750 fathoms. Between the Equatorial and the Polar regions it has been supposed possible to draw a boundary line at which the temperature of the sea is 39° *at all depths;* and this line in the Antarctic ocean, of which the local temperature seems comparatively little disturbed by currents, has been set at 56½° south lat. This doctrine mainly rests upon the temperature-soundings taken in Sir James Ross's expedition; which were not inconsistent with the prevalent belief that *sea*-water, like *fresh* water, has its maximum density at this temperature, and that, consequently, still water at 32° or 33° cannot underlie water at 39°.

Several instances, however, had been recorded, in which temperatures below 39° had been observed; but these were regarded as mere local exceptions, depending upon particular currents. The most remarkable of such observations, made by General Sabine more than fifty years ago, in Captain (afterwards Sir John) Ross's arctic voyage, was thus chronicled in his journal:—"Having sounded, on Sept. 19, 1818, in 750 fathoms, the registering thermometer was sent down to 680 fathoms, and on coming up the index of greatest cold was at 25¾°. Never having known it lower than 28° in former instances, I was very careful in examining the thermometer, but could discover no other reason for it than the actual coldness of the water." Strange as this record seems, it is by no means incredible; for, as the careful experiments of Despretz have since shown, Sea-water, although it ordinarily freezes at 27½°, may be cooled down to 25½° without freezing, if kept free from agitation; whilst, instead of *expanding* as its temperature falls (which is the case with fresh water below 39°), it continues to *contract*, so as to acquire its greatest density at 25½°; which temperature, therefore, might be expected to be that of the still depths of Polar seas, when not disturbed by the intrusion of any warmer currents.

Now the 'Lightning' temperature-soundings gave a *minimum* temperature of from 32° to 33·7° through a considerable part of the deep channel (from 500 to 600 fathoms) lying E.N.E. and W.S.W. between the north of Scotland and the Faroe banks, the *surface*-temperature being about 52°; and it is probable that if there be any error in the observations (all of which rest on the agreement of at least two thermometers), it is that the temperatures were not registered low enough by these thermometers, in consequence of the pressure of 100 atmospheres or more on their bulbs. Though it cannot be positively asserted that these *minima* were the *bottom*-temperatures of the area in question, this may be considered next to certain, for the following reasons:—1. It is improbable that water at 32° should overlie water at any higher temperature, which is specifically lighter than itself, unless two strata have a motion in different directions sufficiently rapid to be recognizable. 2. The nature of the Animal life found on the bottom of this cold area exhibited (as will presently appear) a marked correspondence with its presumed depression of temperature. 3. At a depth of 170 fathoms in the cold area, the minimum was found to be 41·7°; which is just what might be expected at that depth, if the temperature progressively descends with the increase of depth.—The positive determination of this question, however, may be expected from the temperature-soundings to be taken this summer; in which means will be adopted for ascertaining the temperature at every 50 or 100 fathoms in the same sounding.

On the other hand, in other parts of the same channel, *at the very same depths, and with the same surface-temperature* (never varying much from 52°), the minimum temperature was never less than 47°; and where this prevailed, not only was Animal life far more abundant, but its type was that of the warmer temperate seas.

It does not seem possible to account for the existence of two such very different Submarine Climates within so short a distance of each other, save on the hypothesis that the water which is 8° or 10° *warmer* than what may be regarded as the normal temperature of the latitude, has come thither from some region nearer the Equator, whilst the water which is 6° or 8° *colder* than the normal temperature of the latitude, has come thither from some region nearer the Pole. How far the first of these phenomena is attributable to what is properly called the "Gulf Stream"—that is, to the current of heated water which issues from the Gulf of Mexico, and can be traced for a great distance across the Atlantic, in a N.E. direction—is a matter still open to discussion. But that it is *not* attributable to *surface-drift*, seems perfectly clear from the depth to which the excess of warmth extends. And the two facts taken together may with confidence be taken as an example of that continual interchange between the oceanic *waters* of Equatorial and Polar regions, which is as much a physical necessity as that interchange of *air* which has so large a share in the production of winds. For the water that is cooled in the Polar seas must sink and displace the water that is warmer than itself, pushing it away towards the equator; so that in the *deepest parts* of the ocean there will be a progressive movement in the *equatorial* direction; whilst, conversely, the warm water of the Tropical sea, being the lighter, will spread itself north and south over the *surface* of the ocean, and will thus move towards the *polar* regions, losing its heat as it approaches them, until it is there so much reduced in temperature as to sink to the bottom, and thus return towards its source.

A set of temperature-soundings recently taken across the Arabian Gulf, between Aden and Bombay, by Captain Shortland, in H.M.S. 'Hydra,' give a line of bottom-temperature of 33½° Fahr. at depths exceeding 1800 fathoms, the surface-temperature being 75°. It seems impossible to account for this fact on any other hypothesis than that of a deep current from the Antarctic Polar region, which must have maintained this extremely low temperature throughout the vast course it had to traverse.

II. The collective results of the deep-sea Dredgings recently carried on by the Swedish Government under the direction of Professor Sars and his son, by the United States coast survey under the direction of Count Pourtales, and by the 'Lightning' expedition under the direction of the speaker, have conclusively established the justice of the inference previously drawn by Dr. Wallich from the more restricted data collected by the Sounding apparatus,* as to the existence of *a varied and abundant submarine Fauna*, at depths which have been generally supposed to be either altogether *azoic*, or tenanted only by animals of very low type. The dredgings obtained in N. lat. 59° 36′

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* The earliest recorded fact of this class still remains one of the most interesting. In the Arctic expedition of Captain Ross, in 1818, a sounding having been taken at a depth of 1000 fathoms, the line brought up a magnificent *Astrophyton* (then known as *Asterias Caput Medusæ*), which according to the distinct recollection of General Sabine, who was a member of that expedition, *must* have come from the bottom; since it was partly imbedded in very soft greenish mud into which the heavy deep-sea weight had sunk.

and W. long. 7° 20′, at a depth of 530 fathoms and a minimum temperature of 47½°, included an extraordinary collection of *Siliceous Sponges* and *Foraminifera*, with *Zoophytes*, *Echinoderms*, *Mollusks*, *Annelids*, and *Crustaceans;* and among them two specimens of the little *Rhizocrinus* (to be presently noticed more particularly), whose recent discovery by M. Sars on the coast of Norway had furnished a principal "motive" of this expedition. And a single dredging subsequently taken in N. lat. 61° 2′ and W. long. 12° 4′, at the depth of 650 fathoms and a minimum temperature of 46°, gave evidence of the like variety, though the specimens obtained were less numerous and more fragmentary. These two dredgings, it is believed, are by far the deepest that have yet been taken; and the facility with which they were obtained fully justifies the belief that no serious difficulty will be found in the way of the exploration of the ocean bottom by means of the Dredge at depths twice as great.

III. The results of the 'Lightning' dredgings seem to warrant the conclusion that the distribution of Animal life in the deep sea is much more closely related to the *temperature* of the water than to its *depth.* No contrast could well be more striking than that which presented itself within a distance of 50 miles between the Fauna of the *warm* and that of the *cold* area: the former containing, with the animals proper to the locality, a number of forms hitherto known only as inhabitants of the *warmer temperate* seas; whilst the far more scanty aggregate of the latter consisted to a great extent of animals of a proper *Boreal* type, of which few were met with elsewhere even as far north as the Faroe Islands. And whilst the bottom itself was for the most part composed, in the *warm* area, of the Globigerina mud, the presence of which (as Dr. Wallich pointed out) seems to go along with the Gulf stream, it consisted in the *cold* area of stones and sand, the latter including many particles of distinctly volcanic minerals, indicative of a probable derivation from Iceland or Labrador.—The comparatively shallow bank in this cold area, at which a temperature of 41·7° was found at a depth of 170 fathoms, presented a Fauna distinctly intermediate between that of the colder and that of the warmer area; the intermixture of the proper Boreal forms being less, and its predominant character being that which might be expected from its geographical position, with which its temperature closely accorded.

IV. The remarkable fact has been ascertained, that two deposits may be taking place within a few miles of each other, *at the same depth and on the same geological horizon* (the area of one penetrating, so to speak, the area of the other), of which the Mineral character and the Fauna are alike different;—that difference being due on the one hand to the *direction of the current* which has furnished their materials, and on the other to the *temperature of the water* brought by that current. If the "cold area" were to be raised above the surface, so that the deposit at present in progress upon its bottom should become the subject of examination by some Geologist of the future, he would find this to consist of a barren Sandstone, including fragments of older rocks, the scanty Fauna of which would in great degree bear a boreal character; whilst if a portion of the "warm area" were elevated at the same time with the "cold area," the Geologist would be perplexed by the *stratigraphical continuity* with the preceding of a Cretaceous formation, the production of which entirely depends upon the extensive development of the humblest forms of Animal life under the influence of an elevated temperature, and which includes not only an extraordinary abundance of Sponges, but a great variety of other animal remains, several of them belonging to the warmer temperate regions; and he would naturally suppose these widely different climatic conditions to have prevailed at different periods. And yet they have been shown to exist *simultaneously*, at *corresponding depths*, over *wide contiguous areas* of the sea-bottom; in virtue solely of the fact that one area is traversed by an Equatorial and the other by a Polar current.—Further, in the midst of the land formed by the elevation of the "cold area," our Geologist would find a hill some 1800 feet high, covered with a Sandstone continuous with that of the land from which it rises, but rich in remains of animals belonging to a more temperate province; and might easily fall into the mistake of supposing that two such different Faunæ occurring at different levels must indicate two distinct climates separated in time; instead of indicating, as they have been shown to do, two contemporaneous but dissimilar climates, separated only by a few miles horizontally and by 300 fathoms vertically.

V. But further, the examination of the *sample* brought up by the 'Lightning' dredgings, of the Fauna of the Chalk-like deposit now in progress over the warm area, has shown that it presents many points of most interesting relationship to the fauna of the Cretaceous period. Thus of the Siliceous Sponges which were obtained in such remarkable abundance, some correspond so precisely in structure of the *Ventriculites* of the Chalk, that their identity cannot be doubted, although in the process of fossilization the original material of the skeleton has been replaced by carbonate of lime, to be itself applied to the solidification of Sponges of another type in the production of Flints.* Again, the speaker stated that he had found the microscopic

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* The mode in which the Silex which furnished the material of flints was first separated from the ocean-waters, has long been an unsolved problem. By Ehrenberg the aid of hypothetical beds of siliceous *Diatoms* and *Polycystina* which were subsequently removed by solution, has been invoked to account for it. But all the examinations which have been made of samples of the Globigerina-mud have indicated that where this great calcareous deposit is taking place, the production of minute siliceous organisms is extremely limited. On the other hand, the vast multiplication of Siliceous Sponges, and the extension of their root-fibres through the Globigerina-mud on the surface of which they grow, furnishes a depôt of silex, which, if re-dissolved, would afford ample material for the solidification of other sponges into flints.

Xanthidia, so common in sections of Flint, attached to a filamentous substance entangled in fragments of Sponges washed out from the Globigerina-mud. Further, among Mollusca there were two *Terebratulidæ*, of which one at least (*Terebratulina caput-serpentis*) may be certainly identified with a Cretaceous species, whilst the second (*Waldheimia cranium*) may be fairly regarded as representing, if not lineally descended from, another of the types of that family so abundant in the chalk. Among *Echinoderms* the most interesting was the little *Rhizocrinus*, that carries us back to the *Apiocrinite* tribe which flourished in the Oolitic period, and which was until lately supposed to have had its last representative in the *Bourgetticrinus* of the Chalk, to which the *Rhizocrinus* presents many points of remarkable correspondence.* Among *Zoophytes*, the *Oculina* met with in a living state seems generically allied to a Cretaceous type.

It can scarcely be doubted that a more systematic examination of the remarkable Formation at present in progress would place in a still stronger light the intimacy of the relationship of its Fauna to that of the Cretaceous period; and if this view should be confirmed by further inquiry, it would go far to prove, what seems on general grounds highly probable, that the deposit of Globigerina-mud has been going on, over some part or other of the North Atlantic sea-bed, from the Cretaceous epoch to the present time (as there is much reason to think that it did elsewhere in *anterior* Geological periods), this mud being not merely *a* Chalk-formation, but a continuation of *the* Chalk-formation; so that *we may be said to be still living in the Cretaceous Epoch.* For, as was pointed out by Professor Wyville Thomson, in the letter which gave occasion to the 'Lightning' cruise, the oscillations of the earth's crust in the northern portion of the Northern hemisphere do not appear to have ranged much above 1000 feet since the commencement of the Tertiary epoch; so that an immense area of the North Atlantic must have been continuously submerged throughout the Tertiary and Quaternary periods; while, for the reasons already mentioned, there must have been a continual movement of the Equatorial waters towards the Polar region.

VI. It is obvious that the facts previously stated throw a great light on the changes which Palæontological research proves to have often taken place in the Marine Fauna of any particular area, without any corresponding changes in its own Geological condition. For as there must have been *deep seas* in all Geological periods, so there must have been *varieties in Submarine Climate* at least as great as those discovered by the 'Lightning' temperature-soundings; depending upon those Equatorial and Polar Currents, whose existence has been shown to be a Physical necessity. Hence it is obvious that since changes in the direction of such opposing currents must have been produced by any upward or downward movement of the sea-bottom (as in the areas of elevation and subsidence marked out by Mr. Darwin in our existing seas), a considerable modification, or even a complete reversal, of the Submarine Climates of adjacent areas might have been consequent upon alterations in the contour of the land, or in the level of the sea-bottom, at a great distance,—perhaps thousands of miles off.

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* This most remarkable animal, first discovered by M. Sars, near the Loffoden Islands, has been since obtained not only in the 'Lightning' dredgings, but also by Count Pourtales in the Gulf of Mexico.

A renewal and extension of the Researches of which the more general results have thus been stated, having been asked for by the Council of the Royal Society at the hands of her Majesty's Government, adequate provision has been made by the Admiralty with this object; and H.M. surveying vessel 'Porcupine' will be employed during the ensuing season in the prosecution of them, with all the appliances which science and experience can suggest as likely to be serviceable. It may be confidently hoped that, unless the weather should prove exceptionally unfavourable, very important additions will be made by this expedition to our knowledge of the Temperature and Life of the Deep Sea.

The speaker thus concluded:—"The *facts* I have now brought before you, still more the *speculations* which I have ventured to connect with them, may seem to unsettle much that has been generally accredited in Geological science, and thus to diminish rather than to augment our stock of positive knowledge; but this is the necessary result of the introduction of a *new idea* into *any* department of scientific inquiry. Like the flood which tests the security of every foundation that stands in the way of its onward rush, overthrowing the house built only on the sand, but leaving unharmed the edifice which rests secure on the solid rock, so does a new method of research, a new series of facts, or a new application of facts previously known, come to bear with impetuous force on a whole fabric of doctrine, and subject it to an undermining power which nothing can resist, save that which rests on the solid rock of Truth. And it is here that the moral value of scientific study, pursued in a spirit worthy of its elevated aims, pre-eminently shows itself. For, as was grandly said by Schiller * in his admirable contrast between the trader in science and the true philosopher,—'New discoveries in the field of his activity which depress the one, enrapture the other. Perhaps they fill a chasm which the growth of his ideas had rendered more wide and unseemly; or they place the last stone, the only one wanting, to the completion of the structure of his ideas. But even should they shiver it into ruins, should a new series of ideas, a new aspect of nature, a newly-discovered law in the physical world, overthrow the whole fabric of his knowledge, *he has always loved truth better than his system*, and gladly will he exchange her old and defective form for a new and fairer one.'"

[W. B. C.]

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* Lecture introductory to a Course on Universal History, delivered at Jena, 1789.

WEEKLY EVENING MEETING,

Friday, April 16, 1869.

Sir Henry Holland, Bart. M.D. D.C.L. F.R.S. President,
in the Chair.

William Carruthers, Esq. F.L.S.

The Cryptogamic Forests of the Coal Period.

The student of fossil botany encounters greater difficulties in his efforts to restore the vegetation of former epochs in the earth's history, than those which beset the labours of the comparative anatomist in his restoration of extinct animals. These difficulties arise chiefly from two causes: First, the absence in the vegetable kingdom of a substance which would resist decay like the solid skeleton found in all the vertebrate, and in many of the invertebrate members of the animal kingdom, causes the fragments of plants which have escaped decomposition to be preserved much less perfectly than the remains of animals. Carbonaceous stains or amorphous casts are the most frequent indications of the former vegetation of the globe; specimens exhibiting structure are comparatively rare; and it is such specimens only that give certain evidence of the nature and affinities of the organisms to which they belong.

The other serious source of difficulty arises from the fact that no relative proportions exist among the different parts of a vegetable individual. The size of the leaf, the flower, or the fruit, can give no indication of the size of the plant. Indeed, these are more frequently found large in humble plants which never rise above the surface of the ground than in large trees. And this is true, not only in the general, but even among members of the same natural group; where great differences exist in the size of the individuals, no corresponding differences are to be found in the parts of which they are composed. Thus the foliage and fruit of our only indigenous pine—the Scotch fir—are greater than those of the mammoth *Wellingtonia* of California; and the fruit of the small willow (*Salix herbacea*, L.), which covers with a dense carpet the summits of some of the higher mountains of Scotland, is as large as that of the huge willows which ornament the margins of our English rivers. On the other hand, the different parts of an animal possess such relations to each other in size, form, and structure that a zoologist has not so difficult a task before him in restoring, even from imperfect materials, the general aspect of an extinct animal.

I state these difficulties which face us at the very threshold of our investigations, not to magnify the work before us this evening, but to account for the comparatively little progress that has been made in the interpretation of extinct floras, and for the great diversity of opinion that exists among botanists as to the systematic position of numerous fossil plants; and further to account for the very large number of genera and species which have been established on imperfect and fragmentary materials, the systematic position of which is consequently indeterminable.

The progressive accumulation of observations, and the more careful preservation of instructive specimens in local and private museums are supplying the means of dealing with fossil botany after a different method. The most important recent advances in this science have been made in uniting the separate fragments,—roots and stems, leaves and fruits,—described under different names and placed in different and often widely separated genera, so as to build up vegetable individuals, the systematic position and affinities of which can be understood.

These observations are specially true in regard to the vegetation of the Coal Period. Little information has been obtained from the vast stores of the carbonized remains of the plants of this period which are ever being brought under the inspection of man in the form of coal, for this material is so completely altered as to be almost destitute of structure. The best preserved plants occur in the beds of shale which accompany the coal, or are obtained from earthy nodules in the coal itself, which injure its marketable value, and are consequently got rid of by the miners.

We may at once set aside that great division of the vegetable kingdom with which we are most familiar, comprising all plants that have true flowers and seeds, and confine our attention to the more obscure cryptogamous plants which are destitute of flowers, and for seeds have bodies of much simpler structure called spores. The cryptogams are either wholly cellular in their composition, like the mosses and sea-weeds, or they are composed partly of cells and partly of vessels, like the ferns and club-mosses.

If we except some supposed *Algæ*, no traces of true cellular plants have been hitherto detected in the coal measures. The long-continued maceration to which the coal plants were subjected when the beds composed of their remains were forming on the surface of the earth, and the subsequent changes they have undergone, have reduced to one common structureless mass the varied vegetation of which the coal is composed. One of the first results of these operations would be the disappearance of the cellular plants, which under the then existing very favourable conditions must have abounded; just as the soft cellular parts are almost always destroyed of those specimens which have been so favourably situated as to have their vascular tissue preserved.

Excluding then the cellular cryptogams, we may shortly consider the classification and structure of the vascular forms. They are divided into four groups, all of which are represented in the indigenous Flora of Britain.

I. Ferns (*Filices*). Polypody, Brake, Spleenwort, &c.
II. Horse-tails (*Equisetaceæ*). Horse-tail.
III. Club-mosses (*Lycopodiaceæ*). Club-moss and Quill-wort.
IV. Pill-worts (*Marsileaceæ*). Pill-wort.

I. The FERNS have a rhizome which creeps below or upon the surface of the ground, or rises into the air like the trunk of a tree. This trunk in some species attains a great height; it is nearly uniform in diameter throughout its whole length, and is covered with the symmetrical and regularly-arranged markings of the stalks of the old leaves. Internally it is composed of a central cellular pith surrounded by a cylinder of scalariform tissue, and this is invested by a cortical cellular layer or bark.

The woody cylinder is composed of simultaneous vascular bundles, which originate and are completely developed at the same time; there is consequently no addition to it from subsequent growth. It is penetrated by large open meshes, each of which permits the passage of the vascular bundles that supply a leaf, accompanied with a certain amount of cellular tissue from the medulla which occupies the centre of the mesh.

The leaves, which are very variable in size and form, not only perform the functions of ordinary leaves, but also bear the fruit, and are hence called fronds. The fruit is produced in clusters on the back or margin of the fronds; each cluster contains many sporangia, and each sporangium numerous uniform spores.

Though there is a great diversity in the size of the plants of this order—from the humble Wall Rue to the giant Alsophilas,—there is a remarkable uniformity in the size of the spores.

When the spore germinates it bursts through the outer membrane and puts forth a tubular prolongation, which increases by cell-multiplication until a small green leaf is produced, called the prothallus, on the under-surface of which two kinds of glandular-like bodies are developed: the one, the antheridia, containing numerous cells with spermatozoids; the other, the pistillidia, one of which when fertilized develops into a true fern.

II. The HORSE-TAILS have slender, hollow, and jointed stems. Each joint terminates in a toothed membranous sheath, composed of leaves reduced to this elementary state. Whorls of branches and branchlets are given off at the joints in some species.

The fruit is produced in terminal cones composed of numerous stalked peltate scales, each of which bears on its under-surface a circle of sporangia filled with numerous uniform spores. The spores have a spiral covering, which, when they are ripe, breaks up into four clavate threads called elaters, which are remarkably hygrometric.

The spores germinate like those of ferns.

III. The CLUB-MOSSES have solid stems composed of an axis of spiral vessels, surrounded by a thickish cortical cellular layer. The leaves are simple, and arranged spirally on the stem. The branches are irregular and dichotomous.

The fruit is produced in terminal cones composed of imbricating scales. Each scale bears on its pedicel a small sporangium full of spores. In *Selaginella* two kinds of spores exist. The one, called microspores, produces spermatozoids; the other, macrospores, germinates, and forms a prothallus on which pistillidia appear; and these, when fertilized by the spermatozoids of the microspores, grow into perfect plants. In *Lycopodium* microspores only have been seen, and the process of its germination is still unknown.

The little Quillwort (*Isoetes*) which grows at the bottom of most of our mountain lakes, agrees with *Selaginella* in having two kinds of spores; but it differs from the true club-mosses in its habit and in the structure of the stem. Like *Welwitschia* it never increases in height; but this is even more remarkable in the Quill-wort than in *Welwitschia*, seeing that in it there is, as long as the plant lives, a continual development of nodes with their foliar appendages going on. The axis of the stem is composed of cellular tissue. This is surrounded by a vascular cylinder, which grows, as in exogens, by the addition of external layers, there being in this plant a true cambium layer outside the wood, a structure unknown in other cryptogams.

IV. No plants allied to the PILLWORTS have hitherto been detected in the Coal Measures; we need not, therefore, be detained by an examination of their structure and development.

In examining the palæozoic cryptogams of the Coal Forests, I will follow the same order as that in which we have glanced at their living representatives.

I. The FERNS need not long occupy our attention. They were very abundant, though as a rule they were humble herbaceous plants. Arborescent stems are extremely rare—only two undoubted species have been met with in Britain. The numerous known forms have either grown on the earth, or, as is very probable, been Epiphytes. Fructification is rare; in the few cases in which it has been found it agrees with that of recent ferns. Occasionally young fronds exhibiting circinnate vernation have been met with, showing that this method of unrolling the frond was as characteristic of the ferns of that period as it is of those of the present.

The fern is a remarkably stable type of vegetation. The earliest forms, like the *Cyclopteris Hibernica* of Forbes from the Old Red Sandstone, agrees in all comparable points with the recent plants; and throughout all the intervening space no divergence in any point of importance has been detected.

II. No group of fossil plants can more fully illustrate the imperfect materials with which the palæontological botanist has to deal than that group which I have united under the name *Calamites*. The various parts of the plant—the root, the stem, the leaves, and the fruit—have been formed into numerous genera, which have been referred to widely different positions in the vegetable kingdom.

Considerable diversity of structure is to be found in those stems which are referred to *Calamites*. I shall ask your attention to one of these forms which I have described, and which is beautifully illustrated by a series of drawings, recently published, of specimens in Mr. Binney's collection. This stem was composed of a central medulla surrounded by a woody cylinder, composed entirely of scalariform vessels and a thin cortical layer. The medulla penetrated the woody cylinder by a series of regular wedges, which were continued, as delicate laminæ of one or two cells in thickness, to the cortical layer. The cells of those laminæ were not muriform; their longest diameter was in the direction of the axis. The wedges were continuous and parallel between each node. As the axial appendages were produced in whorls, the only interference with the regularity of the tissues was by the passing out through the stem at the nodes of the vascular bundles which supplied these appendages. As the leaves of each whorl were (with one or two exceptions) opposite to the interspaces of the whorls above and below, there was also at each node a rearrangement of the wedges of vascular and cellular tissues.

The stem is described as having been fluted on the outer surface. This error had its rise in the specimens examined being only casts in the amorphous substance of the rock of the medullary cavity, surrounded by a thin film of coal representing the cylinder of wood. On the death of the plant, the cellular medulla decayed, while the woody cylinder was still able to retain its original form. The hollow interior was filled with some of the mud or sand in which the plant was buried. In the course of time this offered greater resistance to the pressure of the beds above than the originally hard cylinder of scalariform tissue, now softened by the moisture in which it had so long lain: the more indurated amorphous axis on pressure necessarily produced its characteristic ridges and furrows on the smooth outer surface of the film of coal. This coal is described as the cortex or bark, and stems exhibiting only the rocky casts of the medullary cavity are called decorticated specimens; but besides the cortical layer they have also been deprived of all that remained of their woody tissue.

The stem terminated below somewhat suddenly in a blunt cone, the internodes of which were slightly developed; and from the nodes were given off whorls of large roots, which again gave off innumerable branching rootlets (*Pinnularia*).

The stem or main axis was simple, supporting numerous branches arranged in whorls, which again produced numbers of whorled leaves. Three different forms of leaves have been formed into as many genera. When the structure of the fruits associated with them is better known, by the discovery of better preserved specimens, it is possible they may be found to constitute three genera, but there are no characters possessed by the leaves which prevent them belonging to one well-defined genus.

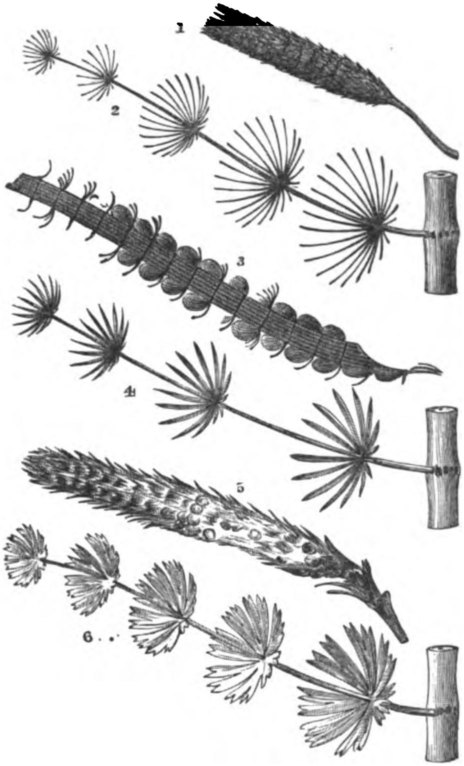

Plate I.—Foliage and Fruits of Calamites.
1 and 2, Asterophyllites; 3 and 4, Annularia; 5 and 6, Sphenophyllum.

The simplest form of leaf (*Asterophyllites*) is slender and linear, with a single nerve. This can scarcely be separated from the form to which the name *Annularia* has been given, and which differs chiefly in having a larger amount of cellular tissue spread out on either side of the midrib. This form has a different aspect in the fossil state from the other, for its whorls of numerous broad leaves are spread out on the surface of deposition, while the acicular leaves of *Asterophyllites* have penetrated the soft mud, and are generally preserved in the position they originally occupied to the supporting branch. The third form (*Sphenophyllum*) consists of whorls of wedge-shaped leaves with one or more bifurcating veins. They occur like those of *Annularia*, spread out on the surface of the shale.

The plan of arrangement of the three forms is the same, and fruits are found associated with them which have the same general appearance; but they are so ill preserved that their internal structure has not hitherto been determined. The different forms have been placed together as allied genera, and have been referred, by those who have specially studied them, to the phanerogamous order *Haloragaceæ* near to the Water Milfoil (*Myriophyllum*), with some species of which they agree very remarkably in the arrangement and aspect of their foliage and fruit.

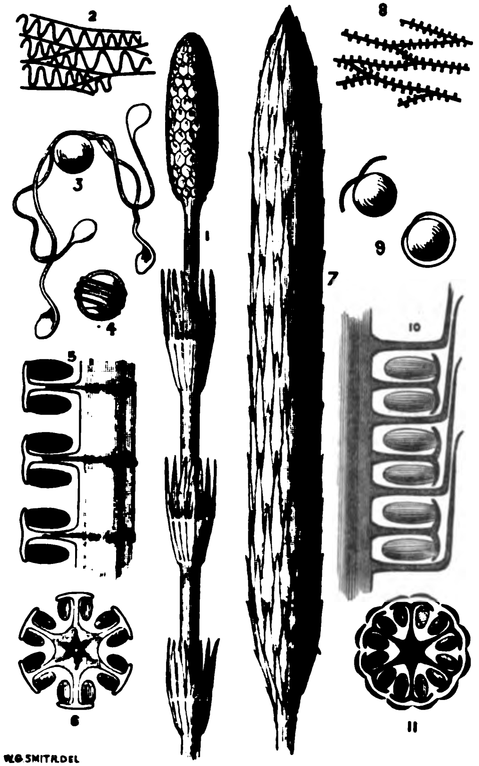

PLATE II.—FRUITS OF EQUISETUM AND CALAMITES.

Fig. 1. *Equisetum arvense*, L. 2. Portion of the sporangium wall. 3. Spores, with the elaters free. 4. Spores with the elaters clasping. 5. Longitudinal section of the part of one side of cone with three fruit-bearing scales supporting sporangia. 6. Transverse section of cone. 7. *Calamites* (*Volkmannia*) *Binneyi*, Carr., magnified three times. 8. Portion of the sporangium wall. 9. Two spores, one showing the bases of two elaters free, the remainder being removed in slicing the fossil, and the other showing the elaters clasping. 10. Longitudinal section of the part of one side of cone with three fruit-bearing and four simple leaves. 11. Transverse section of cone, showing six fruit-bearing leaves and twelve protecting scales.

The determination of the internal structure of one of these fruits which I made, first from specimens collected by Mr. Binney, and have since confirmed from specimens which have been some years in the cabinet of Dr. Millar, has enabled me to refer with certainty these fossils to the cryptogamous order *Equisetaceæ* as near allies of our living Horsetails.

This fruit, to which I have given the name *Volkmannia Binneyi*, is a small slender cone, composed of whorls of inbricated scales (twelve in each), arranged like the successive whorls of leaves on the branch, so that the scales of one whorl are in a line with the spaces between the scales in the whorls above and below. The scales completely conceal the fruit-bearing leaves. These are stalked and peltate, arranged in whorls alternating with the scales, but having only six—half the number of the scales in a whorl. The sporangia, four in number, are borne on the under-surface of the peltate leaves; their walls are formed of elongated cells, which have in their interior a secondary deposit of cellulose proceeding in short truncate processes from the sides of the cell-walls which are in contact, and having the appearance of an incomplete spiral. The sporangia are filled with simple spherical spores, which in the closely-packed sporangium appear to be furnished with double cell-walls. In the half-empty sporangia the outer wall cannot be detected, but there appear instead a number of thread-like processes proceeding from the spore like the elaters in the living Horsetails.

A comparison of this fossil cone with the fruit of *Equisetum* exhibits a remarkable agreement in every point of importance. In the form of the fruit-bearing leaves, the arrangement and structure of the sporangia, the form, size, and structure of the spores, even to the possession of hygrometric elaters, both fruits agree. The only difference is that in the modern plant all the leaves of the cone are fruit-bearing, while in the fossil every other whorl retains a form closely approaching that of the normal leaf of the plant. As these envelop and protect the fruit-bearing leaves, they may be held to give to the fossil a somewhat higher systematic position than is possessed by the living genus. This superiority is further exhibited when we contrast the complex structure of the stem, and the free leaves of *Calamites* with the fistular and sheathless stems of *Equisetum*.*

III. The stems, branches, and fruit of the genus *Lepidodendron* are so abundant in the shales that cover the coal, that the external aspect of this tree has been for a long time well known. Specimens exhibiting structure are more rare, but these also have been met with, so that we know the internal organization as well as the external aspect of the fossil.

The stem is composed of a central pith surrounded by a slender cylinder of scalariform woody tissue, and by a large cortical layer which is divided into two portions, an inner consisting of large spherical and thin-walled cells, and an outer made up of regularly arranged elongated cells with a small diameter. The vascular cylinder is penetrated by radiating meshes through which the vascular bundles passed that supplied the leaves. The outer surface of the stem is covered with the spirally arranged and beautifully marked stigmata of the fallen leaves. The stem branches repeatedly in a dichotomous manner. The younger branches are densely covered with small lanceolate leaves, having a single median vein.

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* Mr. Binney has beautifully illustrated the structure of the stem and fruit of *Calamites* in a series of drawings from specimens in his rich collection, published by the Palæontographical Society in the end of last year.

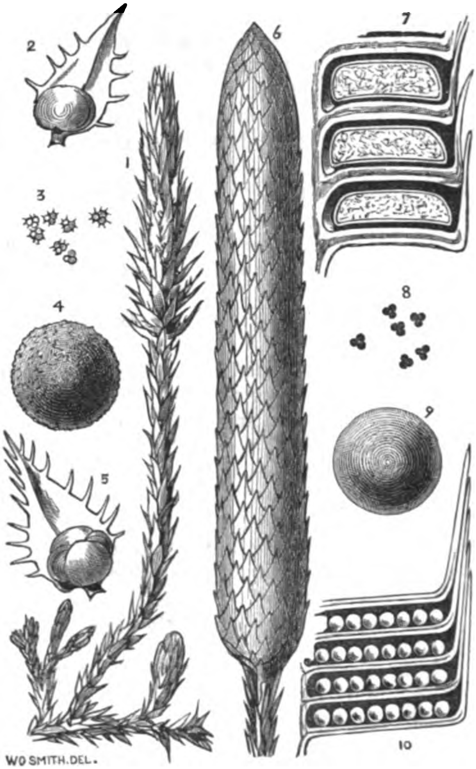

PLATE III.—FRUITS OF SELAGINELLA AND TRIPLOSPORITES.

Fig. 1. *Selaginella spinulosa*, A. Braun. 2. Scale and sporangium from the upper portion of the cone. 3. Antheridian microspores from ditto. 4. Macrospore. 5. Scale and sporangium from the lower part of the cone containing macrospores. 6. *Triplosporites Brownii*, Brongn. 7. Three scales and sporangia of ditto. 8. Microspores from the sporangia of the upper part of the cone. 9. Macrospore from the sporangia of the lower part (drawn from Brongniart's description and measurements). 10. Scales and sporangia of a cone of *Flemingites*.

The fruit is a cone composed of imbricated scales arranged spirally on the axis like the true leaves, and bearing the sporangia on their horizontal pedicels. Three different forms of fruit belong to this genus, or it should perhaps rather be called group of plants.

The first of these is the cone named by Robert Brown *Triplosporites*, and described by him from an exquisitely preserved specimen of an upper portion, in which the parts are exhibited as clearly in the petrified condition as if they belonged to a fresh and living plant. The large sporangia have a double wall, the outer composed of a compact layer of oblong cells placed endwise, or with the long diameter perpendicular to the surface; the inner is a delicate cellular membrane. The sporangium is filled with a great number of very small spores, each composed of three roundish bodies or sporules. Recently Professor Brongniart has described a complete specimen of this fruit, in which the minute triple spores are confined to the sporangia of the upper and middle part of the cone, but the lower portion which was wanting in Mr. Brown's specimen, bear sporangia filled with simple spherical spores ten or twelve times larger than the others.

The structure of another form of cone (*Lepidostrobus*) has been expounded by Dr. Hooker. The arrangement of the different parts comprising it is precisely similar to what occurs in *Triplosporites;* but the sporangia are filled with the minute triple spores throughout the whole cone.

The third form of cone, which I have described under the name *Flemingites*, differs from the other two in having a large number of small sporangia supported on the surface of each scale; and it agrees with *Lepidostrobus* in the sporangia containing only small spores.

In comparing these fossils with the living club-mosses, one is struck with the singular agreement in the organization of plants so far removed in time, and so different in size, as the recent humble club-mosses and the palæozoic tree Lepidendrons.

The fruit of *Triplosporites*, like that of *Selaginella*, contains large and small spores, the microspores being found in both genera on the middle and upper scales of the cone, and the macrospores on those of the lower portion.

On the other hand, the fruits of *Lepidostrobus* and *Flemingites* agree with that of *Lycopodium* in having only microspores.

The size of the two kinds of spores also singularly agrees in the two groups. This is of some importance, for among the recent vascular cryptogams there is a remarkable uniformity in the size of the spores in the members of the different groups, even when there is a great variety in the size of the plants. Thus the spore of our humble Wall-rue is as large as that of the giant *Alsophila* of tropical regions. So also the spores of *Equisetum* and *Calamites* agree in size, as may be seen in Plate II., Figs. 3, 4, and 9, where the spores of the two genera are magnified to the same extent. And a similar comparison of the macrospore and microspore of *Triplosporites* with those of

Selaginella, and of the microspore of *Lepidostrobus* with that of *Lycopodium*, exhibits a similar agreement. This is made apparent by the drawings of the two kinds of spores of *Selaginella* on Plate III., Figs. 3 and 4, with those of *Triplosporites*, Figs. 8 and 9, which are drawn to the same scale.

The fossils represented by the group of stems known under the name of *Lepidodendron*, and by the three fruits described, agree in all essential characters with the living Club-mosses, the only difference of importance being that the stem of the fossil has a higher organization suited to its arborescent habits. The vascular tissue continued to increase with the growth of the plant somewhat like an exogenous stem. In all the living vascular cryptogams, the vascular tissue is produced at once in its full extent except in *Isoetes*, which has a cambium layer surrounding the cylinder of wood in which as the plant grows new vascular tissue is developed. The zone of thin-walled spherical cells which surrounds the woody cylinder in *Lepidodendron*, and which is so rarely preserved, has been a true cambium layer like that in *Isoetes*. But for the existence of this small water-plant, the large trees of the coal-forests would present in the growth of their stems an inexplicable anomaly.

Sigillaria, a very abundant carboniferous fossil, is a member of the same family as *Lepidodendron*. Its stem is rarely preserved so as to exhibit structure, the only specimen hitherto described being *S. elegans*, Brongn.; but its roots are frequently found in a very perfect condition. The name *Stigmaria* was given to the roots at a time when they were supposed to be independent plants. Their relation to *Sigillaria* was suggested by Prof. Brongniart from the correspondence in their structure, by Sir W. Logan from the position the two fossils occupied in the beds in which they occur, and the matter was finally set at rest when Mr. Binney observed the roots and stems in actual continuity.

As the structure and arrangement of corresponding parts in the same plant are uniform, as of the root, stem, branches, and axis of the cone, we may supply the want of information regarding the stem by that which can be obtained from the root.

The root is composed of a central medulla surrounded by a cylinder of scalariform tissue, and this again is invested by a large cellular layer. The vascular cylinder is broken up by meshes through which passed the vascular bundles to the rootlets. There are no traces whatever of medullary rays in the wood. The supposed medullary rays which have been described in *Sigillaria* are the accidental results of desiccation in particular specimens. The internal structure of the stem is precisely the same as in *Lepidodendron*, to which it is closely allied. Externally it has a very different appearance, being either a simple cylindrical column, or in some species dividing dichotomously into a few thick branches. The leaves are long, slender, and parallel-sided. Their scars ornament the older portions of the stem, on which they are arranged in perpendicular series with intervening furrows.

The fruit has been described by Goldenberg. It agrees with that which I have described in *Flemingites* except that the small sporangia are scattered in an irregular patch over the dilated base of an ordinary leaf, and this confirms the systematic position which I have given to *Sigillaria.**

The ferns and other genera which I have described may be considered the types of the plants to which we are indebted for our stores of mineral fuel. They grew in extensive level plains, their fleshy roots penetrating the soft mud which formed the surface soil. The moist atmosphere (not at all likely to have been charged with more carbonic acid gas than that of our own day) would encourage the growth of cellular parasites and epiphytes, and the Aroid discovered by Dr. Paterson, with the several species of Antholithes, most probably represent races of epiphytes of a much higher organization than the cryptogamic trees on which they flourished.

Coniferous trees may have grown on the margins of the plain, but their proper habitat seems to have been the higher ground, from which an occasional stem was floated down by running water to the plains below. What plants were associated with the Conifers in those upland regions, is as yet quite unknown. The Flora of the coal period as at present ascertained is that of the plains. And this is of high interest, apart from the economic value of its products, because it reveals to the biologist an assemblage of plants agreeing in all essentials with some of the humble members of our present Flora, but attaining at so early a period in the history of the world, a development not only in size, but in organization, greatly in advance of their modern allies.

[W. C.]

WEEKLY EVENING MEETING,

Friday, April 23, 1869.

William Spottiswoode, Esq. M.A. F.R.S. Treasurer and Vice-President, in the Chair.

E. B. Tylor, Esq.

On the Survival of Savage Thought in Modern Civilization.

The present argument is concerned with portions of the vast mass of evidence bearing on the subject of the development of culture, of which some examples were discussed by the speaker two years since in a discourse on the Early Mental Condition of Man. It is now proposed to change the point of view, and, taking for granted an early rude condition of mankind, to explain some phenomena of our present civilization as being traceable survivals from more primitive states of culture.

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* For a lengthened examination of the affinities and structure of this genus, see a Memoir read to the Geological Society at its meeting on March 24th, and to be published in its Quarterly Journal on the 1st of August.

Among the most important uses of the study of survival in civilization, is the light it throws on superstition. Three times out of four superstition is a case of survival. When the Hindu Brahman, making his sacrifice, has to forget his flint and steel, and go back to the simple wooden fire-drill for making fire by friction, one Brahman pulling the thong backwards and forwards, and another standing with tinder to catch the sacred spark, he believes that he keeps up this time-honoured process in order to obtain pure and holy fire; but we see that it is a rude old primitive art, long discarded in practical life, but retained for ceremonial use: in a word it is a survival.

Thus it is with superstition. Some old belief or custom belonging to a low level of culture is carried on into the midst of a higher civilization which practically disowns it, and such relics of ancient thought not only survive, but sometimes revive with wonderful vigour. Mediæval witchcraft is a typical instance; it was no new product of mediævalism, but a revival in principle, and mostly even in detail, from the crudest savage sorcery, which had been carried along the course of civilization till, finding in mediæval life a congenial soil, it burst out afresh, and grew apace like the ill-weed it was.

Witchcraft is all but dead among us, but there is going on at this day a great revival of belief and philosophy from the same low stage of culture to which belongs the witchcraft of the New Zealander or of the Puritan of the Commonwealth. Some details of the ethnography of spiritualism will serve to show that it is an example of savage thought surviving in modern civilization.

The world-wide doctrine of spiritual beings has been described before by the general name of Animism. Animism is the doctrine of all men who believe in active spiritual beings; it is essentially the antagonist of materialism, and in some form or other it is the religion of mankind, from the rude savage of the Australian bush or the Brazilian forest, up to the most enlightened Christian. Now Animism in the lower civilization is not only a religion, but also a philosophy; it has to furnish rational explanations of one phenomenon after another, which we treat as belonging to biology or physics. If a man is alive and moving, the animistic explanation is that his soul, a thin, ethereal, not immaterial being in the man's likeness, is within him animating him, just as one gets inside a coat and moves it. If the man sleeps and dreams, then either the soul has gone out of him to see sights that he will remember when he wakes, or it is lying quiet in his body, receiving visits from the spirits of other people, dead or alive—visits which we call dreams. If the man when fasting or sick sees a vision, this is a ghost or some other spirit; if he faints or falls into a fit, his soul has gone out of him for a time, and must be recalled with mystic ceremonies; if it returns, he recovers, but if it stays away permanently, then the man is dead. If the man takes a fever or goes mad, then it is a spirit which is hovering about the patient, shaking and maltreating him, or it has got inside him and is driving him, tearing him, speaking and crying by his voice.

These details are only a few out of the great system of savage animism, which accounts for what we call physical cause and effect as produced by the immediate action of spiritual beings; but even these are enough to show that it is far from being nonsense, that in fact it is a highly rational theory for men in a low state of knowledge. It is common to hear the religion of savages spoken of with contempt by those who have never realized its meaning or its place in history, but it is surely unjust to despise a religion which is abreast of the highest intellectual level of the people it prevails among, and which is part and parcel of their most advanced knowledge.

This early animistic doctrine is to a great degree superseded by science, which sees in dreams and visions, not objective spiritual visits, but subjective phenomena of the mind, and regards the afflicted cataleptic now no longer as doctor, but as patient. Yet it survives largely in popular belief, and has even from time to time come up vigorously in revivals. One of these revivals is the great modern Spiritualistic movement, a movement due to many men, but perhaps especially, though indirectly, to the intensely animistic teachings of one man, Emanuel Swedenborg. In comparing savage and barbaric with modern spiritualism it will be better to give typical cases rather than to multiply details.

As the Australian native sorcerer or the Tatar shaman lies in lethargy while his soul departs to the land of spirits, so it is usual in modern spiritualistic narratives for persons to be in an insensible state when their apparitions visit distant places, whence they bring back information, and where they communicate with the living. The Greenland angekok sees in his visions the souls of the dead; they are pale and soft, and he who tries to seize them feels nothing, for they have no flesh, nor bone, nor sinew. Among the Finns the professional shaman can see the ghosts of the dead, but they are not visible to common men except in dreams. Thus the apparitions of the dead are seen by the modern spiritualist in vision or dream, as the case may be. Swedenborg relates that for twenty-seven years he conversed with the departed spirits of relatives and friends, of kings and princes, and wise men; and he protests that these are not fictions of the imagination, as many will believe, but really seen and heard in a state of complete wakefulness. There may be some here who have visited the house of a great living French novelist, and have seen the arm-chair where the spirits of the dead sit and hold converse with him—there is a chain fastened across the seat to keep out profane visitors.

When the soul is liberated at death, is a suitable moment for it to appear to people in whom it takes an interest; and accordingly the wraith or fetch, the apparition which announces death, occupies in savage psychology the intermediate place between the outgoing soul of the living and the ghost of the dead. The Karens say a man's *la*, or spirit, appearing after death may thus announce it; the Caribs give the name of *marangigoana* to souls, which by their appearance announce impending death; in Madagascar, the *ambiroa*, or apparition which announces death, appears not only to others but even to the dying man himself. Thence we trace on the belief into the lives of the saints, as where, when St. Ambrose died, newly-baptized children saw the apparition of the holy bishop, and pointed him out to their parents; but their grosser eyes could not behold him. Folk-lore kept up the wraith in Europe as part of the well-known Highland second-sight. Fifty years ago Macculloch, in his 'Description of the Western Islands,' declared the old superstition to be dying out; "ceasing to be believed, it has ceased to exist." But if he had lived now, he would have had to finish his sentence, "coming to be believed again, it has again begun to exist." Stories of wraiths are among the most habitual phenomena of the "night side of nature." The mass of apparition stories in spiritualistic books are of types so familiar that it is needless to quote examples from them.

Among savage animists it is to be observed that there always arises a class of professional conjurors, who live in special intercourse with the spirits and perform wonders by their aid. One of the old Moravian missionaries, a century ago, gives an account of the way in which the Greenland sorcerers used to go on their spirit journey to the other world. When the angekok has drummed and writhed about for a while, he is bound by one of his pupils, his head between his legs, and his hands behind his back. The lamps are put out and the windows darkened, for no one must see him hold intercourse with his spirit; no one must move or even scratch his head, that the spirit may not be interfered with; or rather, as the old missionary says, that no one may catch the sorcerer at his trickery, and there is no going up to heaven in broad daylight. At last, after strange noises have been heard, and a visit received from or paid to the spirit, the magician re-appears unbound, but pale and excited, and gives an account of his adventures. The Ojibway conjurors also do this untying trick; and across in Siberia the shamans practise the same coarse juggle. The shaman sits down and is bound hand and foot, the shutters are shut, and he invokes the spirits; all at once there arises a ghostly horror in the dark—voices are heard in different parts, and a rattling and drumming on the dry skin the shaman sits on; bears growl, snakes hiss, squirrels leap about the room. At last it is over, and behold, in walks the shaman free and unbound from outside. No one doubts, says Castren, that it was the spirits who were drumming, growling, and hissing in the yurt, and who released the shaman from his bonds. The unbinding trick is not unknown in English folk-lore, and it is needless to point out the similarity in the exhibition of the Davenport Brothers.

Savage animism flourishes in Central Asia, where the lamas have long been great practitioners in the now familiar art of table-moving. To quote only one instance: John Bell, of Antermony, 150 years ago, describes the process of finding a thief who had stolen some damask. The lama got on a four-legged bench, "and soon carried it, or, as was commonly believed, it carried him to the very tent, when he ordered the damask to be produced. The demand was directly complied with; for it is in vain, in such cases, to offer any excuse."

One of the most celebrated of modern spiritual manifestations is the feat of rising in the air. This, if not savage, has a long and curious ethnographic history. It is familiar to Buddhism, where every saint who has attained to "riddhi," or perfection, is able to rise in the air, as also to overturn the earth and stop the sun. The appearance of the miracle in the Western World belongs it seems to classic times; foreign conjurors were exhibiting it to the Greeks in the first century. After a while it became a regular prodigy of Christian miracle. The Lives of the Saints swarm with it. St. Dominic, St. Dunstan, St. Philip Neri, St. Ignatius Loyola, are among the list of saints who not only metaphorically "rose above the earth," but were thought, particularly by biographers a long while after they were dead, to have literally hung suspended in the air in life. Thus, when St. Richard, the Chancellor to St. Edmund, Archbishop of Canterbury, one day softly opened the chapel door to see why the archbishop did not come to dinner, he saw him raised high in air with knees bent and arms stretched out; falling gently to the ground at sight of the intruder, the prelate complained of being thus hindered of great spiritual delight and comfort. The old archbishop's mantle, or some remnant of it, has now descended on Mr. Home.

As to the means by which disembodied spirits communicate with living men. In the first place they appear in visions or dreams and talk with the living, and here the opinion of the modern spiritualist is absolutely identical with that of the savage. But the modern medium may also introduce into spiritual converse arts unknown to savage life, spelling and writing. Rapping spirits are so far savage in principle, that if one told a North American Indian that mysterious knocks were done by a spirit, he would assent at once, for any mysterious noise is to his mind the action of a spirit. But savages do not seem to have selected a special class of knocking spirits, though this spirit abounds in civilized folk-lore. He is the "knocker" the Welshman hears underground; the "poltergeist" who routs about in German peasants' houses; the "vampire" who tumbles about the furniture in Crete. The spirits had begun to answer questions by knocks, as Dr. A. Bastian has shown, in the middle ages. The device of an alphabet of counted raps, 1 for A, 2 for B, &c., was adopted in America to communicate between disembodied and embodied spirits. Scientific spirits, it is alleged, and especially Franklin's spirit, have contrived to adapt electro-magnetic vital forces to produce the rapping sound. That the messages the spirits send are at the intellectual level of the mediums who receive them, need hardly be said; they are so foolish that intelligent spiritualists habitually apologize for them, and *spirituel* may indeed some day become a word for "silly."

Spirit writing, though of course not belonging to unlettered savages, has a curious ethnography. It is well known in China. When a man wishes to consult a god in this way he places a table before the image with candles and incense, an offering of tea and sham money, and a large platter filled with sand. A V-shaped wooden handle is provided with a sharp tooth at its point; two men hold this instrument, each grasping one leg of it, the point resting on the sand. Then the god is invoked, and his spirit descends and guides the pen which wriggles about in the sand and writes the oracular message. Dr. Bastian, to whom we owe so much valuable information as to the ethnography of spiritualism, adds that when the sprig of the sacred apricot tree is broken to make the spirit-pen, the precaution is taken of scratching a suitable apology on the bark of the tree. There are old European accounts of writing with a spirit-guided pen, and there is an instrument called a "planchette" made and sold in London now, a little tripod with a pencil, which two persons place their hands on and wait for a disembodied spirit to guide them to write messages.

It appears, however, that spirits can dispense with such material instruments. We remember how, during the Council of Nicæa, two of the bishops, Chrysanthus and Mysonius, happened inconveniently to die, so the acts of the council were solemnly laid on their tombs, and were found in the morning with the dead men's subscription, thus,—"Although removed from earth, we have signed the volume with our own hands." This proceeding has been renewed in our own day. For example, the Baron de Guldenstubbé has published a book, 'Pneumatologie Positive et Expérimentale,' in which he says that the spirits of the departed do hover near their tombs, and haunt places where they dwelt "during their terrestrial incarnation." Louis XV. and Marie Antoinette roam about the Trianon; Francis I. manifests himself at Fontainebleau, and what is more, if you leave blank pieces of paper in suitable places, they will concentrate an electric current on it by their force of will, and thus impress characters on the paper. The Baron publishes fac-similes of the spirit-writings he got thus: Augustus and Julius Cæsar's near their statues in the Louvre, Abelard and Héloise at their tomb at Père-la-Chaise, with an inscription that they are united and happy. The alphabetic writing of the surviving ancients is, it must be confessed, rather queer sometimes, as when St. Paul writes himself as *ελζιστος των αποστολον*; when Hippokrates wrote his name, which cured an attack of rheumatism in a few minutes, the virtue of the prescription lay perhaps in the great physician spelling himself with a long ˉ and a short *ĕ*.

What is now being discussed is not the positive truth or falsity of the alleged spiritual phenomena and doctrines, but their ethnography. There may be remarkable psychological phenomena, "brain-waves," or what not, involved in what is called spiritualism, as there were unquestionably remarkable morbid phenomena involved in what was called mesmerism. But this is not the question here. It is not merely that the alleged spiritualistic facts are believed in by savages and barbarians, and disowned by civilized science. It is much more than this. It is that the spiritualistic interpretation of the alleged visions, and rappings, and writings, the belief that they are produced by disembodied spirits, belongs to the philosophy of savages. Set a Chinese and an English medium to obtain written missives from the respective spirits they believe in, and let a wild Ojibway Indian look on at the performance. So far as the presence of disembodied spirits goes, possessing the performers and guiding the pencils, or manifesting themselves by raps, or voices, or other actions, the savage would understand and admit it at once, for such things are part of his recognized system of nature: the only part of the affair out of his line would be the art of writing, which does belong to a higher grade of civilization than his. In a word, a modern medium is a Red Indian or a Tatar shaman in a dress-coat.

Even supposing the alleged spiritualistic facts to be all true, and the spiritualistic interpretation of them sound, this does not alter the argument. It would prove that savages were wise, and that we civilized fools have degenerated from their superior knowledge. But it would remain true that modern spiritualism is a survival and a revival of savage thought, which the general tendency of civilization and science has been to discard. This is the case of spiritualism as seen from an ethnographic point of view.

To turn now to another topic bearing on survival in culture. Modern games are often survivals of weightier matters, just as one of man's most important implements of war and livelihood survives as a toy in the tiny bows and arrows that children play with in the streets. There is one interesting group of sports, which there is some ground for treating as survivals; these are games of chance. We all know that when halfpence are tossed or dice cast, no special physical action takes place more than when a stone is thrown to the ground. We know that betting on the turn-up of the coin or die is an appeal to chance, that is, to our own ignorance; not that the process of turning up is extraordinary, but that it is so difficult to follow that we cannot foresee its result. But we also know that this scientific view of chance is not that of early civilization. It was not thus that the South Sea Islander looked on his divination by lots, that the African fetish-priest shuffled his bits of leather for omens, that the crowd prayed the gods with uplifted hands while the champions cast lots in Agamemnon's helm to learn who should go forth to do battle with Hektor and help the well-greaved Greeks. The uncivilized man fancies that lots or dice are being adjusted in their fall with reference to the meaning he chooses to attach to it; and especially he imagines spiritual beings standing over the diviner or the gambler, shuffling the lots or turning up the dice to make them give certain answers. This view held on strongly into the middle ages, and one of the most remarkable movements of the seventeenth century was when Thomas Gataker, the Puritan minister, attacked the supernatural theory of lots and games of chance in a treatise in small quarto.

The supernatural theory of lots is dying, but not dead, for fortune-telling with cards, turning up texts for omens, and so forth, still survive largely in civilized Europe. How directly supernatural interpretation is connected with gambling in the popular mind, we may judge from the people of Southern Europe, who expect their patron saints to help them to lucky numbers, or from the Lusatian peasant, who slily hides his lottery-ticket under the cloth of the communion-table, that it may receive the blessing with the sacrament and stand a better chance of a prize. Arts of divination and games of chance are identical in principle and in great measure in detail. The dice with which the Greek oracle and the African sorcerer give omens are not to be distinguished from gamblers' dice. Lots serve both purposes. The Chinese gambles by drawing lots, and also his market-places are crowded by professional diviners who draw lots for omens. The Chinese, however, with all their love for old customs, dislike being practically inconvenienced by them; so when a Chinese makes up his mind what to do, he goes to a lot-drawer and takes an omen; but if the omen is not what he wants he will try again and again, and at last, when he gets the omen he required, that he will act on. Again, playing-cards are used alike for games and for cartomancy, fortune-tellers preferring the very old fashioned ones known as tarots, which are much more complicated than ours, and lend themselves to a greater variety of omens.

Now the question is, Are games of chance in general survivals from serious divination? It is hard to settle a precedence between them on distinct evidence, but there are two cases where it is known which use came first. There is a well-known South Sea Island art of divination by spinning a cocoa-nut; the persons interested sat in a circle, and the cocoa-nut was spun in the middle; the oracular answer was according to the person or place towards which the monkey-face of the fruit was directed when it stopped. Now, though the Samoan Islanders in Mr. Turner's time had left this off as a means of divination for discovering thieves, &c., they still kept it up as a game of forfeits. Again, there was a Greek art of divination, called kottabos, which consisted in flinging wine out of a cup into a metal basin some way off without spilling any, the thrower saying or thinking his mistress's name, and judging from the clear or dull splash of the wine what his fortune in love would be; but in time the magic passed out of the sport, and it became a mere game played for a prize. Now the question is whether these two cases are typical. If so, we may consider games of chance as survivals from the corresponding processes of divination; that they are divination in sport made gambling in earnest. And it is so much a rule of survival that the sportive use of an art is derived from its serious use, that this hypothesis of the general origin of games of chance seems a plausible one.

Again, as to the superstitious practices which belong to peasant folk-lore, and which are really survivals from a low philosophy of religion, let us take one example. It is one of the principles of the lower animism that diseases are caused by spirits possessing or attacking the patient. It is another principle that spirits may embody themselves for a time in any material object; this is the main theory of fetishes and fetish worship.* Thus the disease-spirits may be persuaded to come out of the patient, and get into some object prepared for them. To take an instance from the Siberian tribes whose table-moving I have mentioned: when a man is possessed with a demon, or as we should say when he is ill, it becomes the business of the priest to charm the spirit out into a doll, and so the patient gets well. Or the disease-spirits may be got into rags, or locks of hair, &c., and hung on trees. African sacred trees are hung all over with such objects, and such trees with offerings for diseases exist to this day within the limits of Great Britain. There are, probably, some here who can remember their nurses charming little diseases out of them into nails or knots, and so getting rid of them.

But to suppose the principles and rites of the religion of the lower races to be only represented in that of the higher races by little surviving superstitions, would be an utterly one-sided view. Many most important thoughts and rites of religion—worship, prayer, sacrifice, penance, fasting—may be traced upwards from the lower races more or less far into the faiths of the higher nations, modified and adapted in their course to fit more advanced culture and loftier creeds. This is too large a subject to be entered on now, but let us glance at an example or two from the ethnography of religious ceremony.

Ceremony is part of the gesture-language of mankind, and acts dramatically the ideas it signifies. For example, among the religious ideas of men, few lie deeper in history than the association of bodily cleansing with ceremonial or moral purity. By obvious metaphor, such words as clean or pure are applied to purification from guilt, ceremonial contamination, or moral sin. And what we thus express in words, the men of the lower culture began early to act in ceremony, purifying objects or persons by various imitative rites, especially by passing them through fire, or dipping them in, or sprinkling them with, water. If we look at the distribution of these rites of lustration among the races of the world, we shall find that their diversity of detail and purpose, to say nothing of other reasons, seems to forbid our considering them as all adopted from any single common source.

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* It is well known that the Portuguese gave the name of *feitiço*, "charm," to the bits of stone, bone, and other rubbish worshipped by the negroes as receptacles of supernatural beings, and we adopted the word as *fetish*. But the word had really been English ages before in a different sense. Latin *factitius* became Portuguese *feitiço* in the sense of magic art, but was also adopted from Norman-French into English as *fetys*, well made, neat. It occurs in the best-known quotation from Chaucer:

> "And Frensch sche spak ful faire and *fetysly*," &c.

Such ceremonies are either practical cleansings done ceremonially, or they are pure ceremonies; they have little to do with cleanly habits, and do not in the least prove that the people who practise them hold cleanliness to be next to godliness. Genghis Khan's Tatars, who had a conscientious objection to taking off their clothes, considered themselves sufficiently purified by passing through the fire, and the modern Persian is a striking example of the way in which ceremony may override reality. He will wash his eyes when they have been polluted by seeing an infidel, he will carry about a water-pot with a long spout for his ablutions, but he neglects the simplest sanitary rules, and obtains ceremonial purification by dipping in a disgusting little tank of water where a hundred people may have been before him.

The same thought seems to run through all the ceremonies of lustration, but the details differ extremely, and seem to have been in great measure developed independently, as a few typical examples will show. The Kafirs, who are not in the habit of washing on ordinary occasions, perform a ceremonial ablution after a funeral, as do the modern Hindus. The Romans, returning from a funeral, were purified both by being passed over fire and being sprinkled with water, and the same double rite was observed in the annual lustration of the flocks at the Palilia. Among the aborigines of India and South-East Asia, when a child is born the mother undergoes a ceremonial lustration, and it is then that among the Kols of Chota Nagpur the child is named. The New Zealand ceremony of washing young children is highly remarkable. The baby is taken to the stream and dipped or sprinkled by a native priest; the priest chants a list of names of its ancestors, and the one at which the child sneezes or cries is the name it is considered to choose for itself. The object of this ceremony seems to be the removal of the original *tapu* under which the child is born, which *tapu* may also be removed by another ceremony, a pretence of eating the child. The Lapps also named their children with a ceremonial washing in early times, and long kept up this native rite in private after their conversion to Christianity. And again, the Jakuns of the Malay peninsula and the Aztecs of Mexico were remarkable for lustrating infants both with fire and water.

Another motive for ceremonial lustration is to drive out demons, as was done in classic and mediæval times, and as the Zend Avesta describes the driving out of the Drukhs Naçus by sprinkling with holy water, which drives it from limb to limb, till it escapes at the toes. It is needless to enter here into the ceremonial lustrations of the Jews and their baptism of proselytes. The rite which appears over so great a geographical range, and can be traced through so many stages of culture, appears within the limits of Christendom in the comparatively insignificant practice of aspersion with holy water, but especially holds its place almost throughout Christianity in the baptismal ceremony.

To take one last example from religious ceremony: we have but to think of sunrise and sunset to understand how early must have been the association in men's minds of the East with the source of light and warmth, life and happiness and glory; of the West with darkness and chill, death and decay. Where the sun goes to his daily death at sunset, thitherward the soul departs to the other world. As the spirit of the dead Australian hovers for a while on earth, and goes at last toward the setting sun; as Fijian souls start for the judgment-seat from the Western Cape; as the Ojibway's shade follows a wide and beaten path westward, and crossing the deep and rapid river comes to the land abounding in game, and joins his rejoicing kindred in their lodge; so the Egyptian dead went West to the death-land of Amenti, and among our Aryan forefathers, in Max Müller's words, "As the East was to the early thinkers the source of life, the West was to them Nirriti, the Exodus, the land of death."

Nothing could bring out more clearly the full significance of the West as the region of Death than the details of the consecration of the pickaxe by the murderous Thugs of India, worshippers of Kali the death-goddess. In her honour it is that the victims are murdered; to her is dedicated the pickaxe with which the graves of the slain are dug. On that dreadful implement no shadow of any living thing must fall; its consecrator sits facing the West to perform the four-fold washing and the seven-fold passing through the fire, and then, duly consecrated, it is placed on the ground, and the bystanders worship it with faces turned to the West.

On the other hand, the thought of the deities as in the region of sunrise is familiar to the savage mind in South America, as when the Jumanas turn the faces of their dead to the East, where dwell the two great deities, the Good and Bad Spirit; and so the Guarayos turn their corpses to the East, to go to the happy country of Tamoi, the grandfather, the ancient of heaven. In countries where sun-worship prevails, there prevails with it the right of adjusting the temple, and turning the worshippers, to the east. One of the great ceremonial rites of the Apalaches was performed at sunrise, when the priest stood at the door of the temple hut and adored the Eastern sun; the cave-temples of the Floridans opened eastward to receive the first rays of the luminary; in Mexico men turned to the East in prayer, and the kindred Nicaraguans declared the gods to be in the region of sunrise; in Peruvian sun-temples the doors looked east, so that at dawn the sun's rays fell on the golden disc, and the people saw and greeted their national deity. This is the rite which the prophet Ezekiel describes as he sees it in horror-stricken vision: "At the door of the temple of the Lord about five-and-twenty men, with their backs toward the temple of the Lord, and their faces toward the east, and they worshipped the sun towards the east." Predominant as sun-worship was in Aryan thought, what is more natural than that the Brahman should turn to the east, and that Vitruvius should give directions so elaborate for adjusting the temples and altars of the immortal gods by the same rule of east and west followed by church builders now.

In speaking of the solar symbolism of east and west within Christianity, I do not mean such exceptional cases as that Christian sect which Leo I. describes in the fifth century, as stopping on a hill and bowing to the rising sun before entering the Basilica of St. Peter, which the Pope says "comes partly from ignorance and partly from the spirit of paganism, and afflicts us extremely." I mean rather such ceremonies as the baptismal rite about the fourth century, which contrasts East and West with the utmost fulness of symbolism. Cyril of Jerusalem thus describes the scene:—"Ye were first brought into the ante-room of the baptistery, and placed standing toward the west (the sunset), and then commanded to renounce Satan by stretching out your hands against him as if he were present . . . And why did ye stand toward the west? it was needful, for the sunset is the type of darkness, and he is darkness, and has his strength in darkness; therefore symbolically looking to the west ye renounce that dark and gloomy ruler." Then turning round to the east the catechumen took up his allegiance to his master, Christ. Thus Jerome says:—"In the mysteries we first renounce him who is in the west, and dies to us with our sins, and so turning to the east we make a compact with the Sun of Righteousness, and promise to be his servants." This perfect double rite of east and west is retained in the Eastern Church, and may be seen in Russia to this day. The partial ceremony of orientation of churches, and the practice of turning toward the east in worship, which quite naturally caused early Christians to be accused of being sun-worshippers, are common to both churches.

But it is quite curious to see how far the solar origin and meaning of this practice has been forgotten in modern times. If you ask the meaning you will often be told it has to do with turning towards Jerusalem, as if the church-builders in Normandy and England did not know east from south-east. The absurdity of the notion is shown by the fact that the churches in Asia, on the other side of Jerusalem, turn east as religiously as they do in Europe. But how can anyone expect to know the origin and meaning of ceremonies, or of anything else, without knowing the ethnographic facts which show the history of their development. Those who would understand such things must do as the Patriarch of Constantinople himself recommended not long ago, they must have recourse to the "historical method."

In the beginning of his 'Positive Philosophy,' Auguste Comte incidentally lays down a maxim which all ethnographers may adopt as a standing rule. It is simply this remark, that "no conception whatever can be understood except through its history." The more we study civilization, the more clearly we shall see that the civilization of any age is not a new creation to meet the wants of that age, but that it is a result of past times, modified to meet new conditions of life and knowledge, yet showing in its cases of survival clear vestiges of the course of its development.

The attempt to understand advanced stages of knowledge, belief, art, or custom, without understanding their earlier stages, is not only ineffectual but misleading. To a certain extent people acknowledge this: that our forefathers, and the forefathers of the French and Germans, and those of the classic Greeks and Romans, were once barbaric tribes is matter of mere commonplace, and it is not questioned that an acquaintance with their early condition is needed to see the meaning of the higher culture into which they rose. But we must go further than this. If, as it seems, the savage stands in somewhat the same relation to the barbarian that the barbarian does to the civilized man, it is needful that the student should gain the most thorough comprehension not only of barbarian, but also of savage life, in order that he may be able to trace up, from as primitive a state as possible, the phenomena of civilization, whether they have become greater and stronger in their after-development or have lingered as obscure survivals. The moment such an attempt is made, its value becomes evident. To mention only English students, no one could read Mr. M'Lennan's researches in early law, Sir John Lubbock's comparisons of historic with pre-historic savages, Colonel Lane Fox's lectures on the development of weapons, and deny this.

Savages display thoughts and practices whose origin is comparatively intelligible; far more intelligible than in the modified state in which we have them as survivals at higher grades of culture. The notion of transferring a disease-spirit to a bit of stick is part and parcel of consistent savage philosophy, but when it lingers among civilized men it is an absurd superstition; the savage, in child-like good faith, turns toward the rising sun as toward a great and good living lord, whereas the rite is continued in barbaric religions with a less materialistic sense of worship, and passes at last into a new symbolism.

No apology is offered for the incongruous selection of topics which have been discussed or alluded to this evening. Time made it so impossible to trace out the course of survival as a general whole, that examples were intentionally taken almost at random to show how on point after point through the vast range of modern thought the savage has something to say, and even something of consequence. It is a very familiar thought that it may be a duty of civilized life, and certainly is its effect, to put an end to savagery in the world. The settler and the trader are hard at work more or less humanely, in abolishing savagery. The missionary, in his noble efforts to civilize and Christianize the unhappy lingering savage races, tries to help them as best he may across the huge gulf that separates savage from civilized life. But perhaps it is not quite so familiar a thought that knowledge of savage life has actually gained in the course of its destruction. How ridiculously little the classic world knew or cared about savages, though they abounded in its outskirts! Our main knowledge of them is mediæval and modern, collected in the process of improving them off the face of the earth.

What savagery had to teach has been written, as it were, on Sibylline books, little cared for while they were plentifully offered, but which, now that there are but a few left, we are willing to buy for a price, and read with eager eyes. Much as we have lost of the details of the life of these modern representatives of pre-historic man, we are not quite too late. Through the vast range of human thought and art, the savage can give hints full of interest and value as to the origin and development and meaning of our own life; and the civilized man who goes to teach may, in many things, remain to learn.

[E. B. T.]

WEEKLY EVENING MEETING,

Friday, April 30, 1869.

Sir Henry Holland, Bart. M.D. D.C.L. F.R.S. President, in the Chair.

Robert H. Scott, Esq. M.A.

DIRECTOR OF THE METEOROLOGICAL OFFICE.

On the Work of the Meteorological Office, Past and Present.

A little more than seven years ago, Admiral FitzRoy addressed an audience in this theatre on the subject of the system of Meteorological Telegraphy, then for twelve months in operation—a system whose introduction and establishment were mainly due to his own efforts. The popularity which this subject of inquiry very naturally assumed in a country like our own, with its extended coast-line, at times exposed to all but incessant storms, has had a tendency to mask the work for which the office was in the first instance founded, and which it has persistently endeavoured to carry out.

Accordingly, in a discourse "On the Work of the Meteorological Office, Past and Present," I may, perhaps, be excused if I occupy a few minutes in a brief account of what the origin of the office was.

The primary object for which it and all the other similar offices were established, was the acceleration of ocean routes for vessels by an accurate investigation of the prevalent winds and currents in the various parts of the sea. Many years ago, Basil Hall said:—

"It is one of the chief points of a seaman's duty to know where to find a fair wind, and where to fall in with a favourable current."

Long before his time, however, Mr. Marsden, who was Secretary to the Admiralty at the beginning of this century, had proposed the plan of dividing the sea-surface into squares by means of the parallels and meridians, in order to determine accurately the winds, &c., close to the equator. Mr. Marsden's method of subdivision has been since universally adopted.

The first impulse to the present work was given by Sir J. Burgoyne, who, in 1852, started the idea of land observations on an extensive scale, to be carried out by the Corps of Royal Engineers.

Lieutenant Maury had then been working for several years at Washington, and the United States' authorities were consulted by our Government as to the possibility of their co-operating in the scheme. In their reply, they suggested its extension to sea observations.

The correspondence was then referred to the Royal Society, who pointed out to the Government the great difficulties which stood in the way of international co-operation in land observations, owing to the want of satisfactory self-recording instruments which should render the system independent of differences of scales and of hours of observation, to which observers in different countries were attached. The Royal Society warmly supported the scheme for marine meteorology.

In brief, then, the Brussels Conference met in August, 1853; most maritime nations were represented at it, and a uniform plan of action was adopted.

Subsequent to this, the National Observatory at Washington continued to work steadily until the war broke out.

Holland established its Royal Meteorological Institute in 1854, which is now in active operation.

At the end of that year, Mr. Cardwell, then President of the Board of Trade, resolved to establish a Meteorological Department of that office, and appointed Admiral FitzRoy, then a captain, as its chief.

At the present time, sixteen years after the Brussels Conference, the subject is again attracting general attention. The National Observatory at Washington is being resuscitated, if I may use the expression; and other countries are commencing the work, especially our own countrymen in India and in the Mauritius where systematic work has been going on for years under Mr. Meldrum.

The most recent recruits have been the towns of Hamburg and Bremen, who have established the Norddeutsche Seewarte, under the able superintendence of Herr W. von Freeden, formerly of the Navigation School at Elsfleth, and have commenced the investigation of marine meteorology with great vigour.

The Royal Society were requested by the Government in 1855 to furnish what might be called "Sailing Directions" for the new office, which they accordingly did, in a most copious and complete manner. The suggestions contained in their letter continue to form the basis of the operations of the office, with the exception of one or two matters which have been taken up by the Hydrographic Office of the Admiralty.

Admiral FitzRoy on his appointment began work on a very comprehensive scale. The first thing to be done was to provide trustworthy instruments, and accordingly relations were entered into with the Committee of the Kew Observatory, which have since ripened into the present close connection between the two establishments.

A barometer recommended by that Committee was adopted as the one best suited for use at sea, and the same gentlemen furnished specifications for the construction of the other instruments employed in the various observations. In addition to this, all the instruments issued by the office have been verified at Kew.

The practice of the office is to lend to observers instruments, which are returned at the end of the voyage, together with a register of observations taken with them, prepared on a plan similar to that adopted at Brussels.

Admiral FitzRoy, in order to place in the hands of seamen results already obtained, proceeded to publish translations of foreign works, and more especially reproductions of Maury's charts, embodying with the data in them those obtained from materials in his own office. In this way the series of Board of Trade wind-charts have been prepared.

The issue of instruments went on for some time very freely, and registers rapidly accumulated, so that it speedily became evident that the staff of the office could not cope with the work which came on it. When this fact forced itself on the notice of the chief, his own attention was being irresistibly attracted by a new sphere of action, the study and prediction of weather, of which we have more to say presently. Accordingly he checked the collection of materials, and diverted a considerable proportion of the working energy of the office into this new channel.

The marine work, however, though retarded, went on at a steady rate, though its projector did not live to see the completion and publication of the results of the inquiries he had set on foot: most notably of that which he always looked to as his grand work, the examination of the meteorology of the Pacific, which he left in a very incomplete state, owing to deficiency of material.

All these unfinished investigations are in process of being rendered accessible to the public, either in the form of independent publications bearing on special questions, such as sea-surface temperature, or by being supplied to the Hydrographic Office for incorporation in the pilot charts now in process of publication by the Admiralty.

On Admiral FitzRoy's death, the Royal Society were again consulted by the Government, as to the position and future work of the office. They stated that they saw no grounds for materially changing the suggestions in their letter of 1855, and they added to them a scheme for the study of British meteorology.

Subsequently, at the request of the Board of Trade, an inquiry was made into the working of the office, and a report presented to Parliament in 1866 by a committee of three gentlemen, nominated respectively by the Board of Trade, the Admiralty, and the Royal Society. This report supported strongly the views expressed by the Royal Society, and further suggested that the office should be placed under efficient scientific superintendence, so as to ensure its being conducted so as best to carry out those views, and to afford a thorough safeguard against undue prominence being given to any one of its departments to the detriment of others.

In November, 1866, the Royal Society were requested to undertake the superintendence of the former Meteorological Department of the Board of Trade, and the Council agreed to accept the trust, and to nominate a committee of eight Fellows of the Royal Society, who would have the entire and absolute control of the office. This is the whole connection between the office and the Royal Society. The Society itself has nothing whatever to do with the office or with its funds, beyond appointing the Superintending Committee.

In order to ensure the efficiency of the marine branch, Captain H. Toynbee, a gentleman of long experience as a seaman and a practised observer, was appointed to assist the director, by taking entire charge of this most important division of the work.

The collection of materials, especially with reference to the less known parts of the ocean, has been recommenced, care, however, being taken that the instruments are only issued to first-class observers. Many of our leading steamship companies have consented to co-operate in the work.

Meanwhile the documents already in the office are being carefully examined, and a systematic inquiry has been begun into the entire meteorological conditions of the area lying between the Trades in the Atlantic Ocean, known as the Atlantic Doldrums, by which means it is hoped that light will be thrown on several interesting and important questions which affect our climate seriously.

Humboldt said years ago that we must look to the Tropics for the interpretation of the complex meteorology of higher latitudes; and it is a very remarkable fact that the area on which we are now working is precisely that indicated more than half a century ago, by Mr. Marsden, as the most important first field for operations.

The present work of the office in this department cannot be made very interesting to the public, as it simply consists of a patient examination of the registers, and the preparation of the materials contained in them for future discussion.

As this work had to be begun absolutely *de novo*, a year or two must necessarily elapse before we can even form an opinion as to the probable results which the work will yield. We trust, however, that whatever they are they will form a useful contribution to our knowledge of marine meteorology, and one worthy of the justly earned reputation of the mercantile marine of this country.

As I said at the commencement of the discourse, this is the work for which the office was established, and for the performance of which Parliament, fourteen years ago, consented to supply funds. Marine meteorology must ever be considered the prime object of its attention, an object the value and importance of which was recognized before self-registering instruments for land meteorology had been perfected, before storm warnings were dreamt of, and before most, if not all, of my audience were born.

Telegraphic Weather Intelligence.

The origin and development of this branch of the office is fully traced in the report of the committee of inquiry already referred to. Admiral FitzRoy, in 1861, devised a code of meteorological telegraphy in cipher, and instituted a regular service, by means of which information of weather was received from stations on the coast, and issued to the public. This information consisted of occasional warnings of storms, and of forecasts of probable weather, which were published in the daily papers.

The storm-warnings extended over three days at a time, and conveyed distinct intimations of the direction and probable force of the wind which would blow on the line of coast where the signal was hoisted. The forecast of weather was only for two days in advance.

As is known to every one, the signals used by Admiral FitzRoy were a drum and a cone, which latter, according to its position, indicated N. or S. winds. The system was a very decided advance on the scheme originally proposed both here and in France, which was to convey by telegraph the fact of the existence of a storm on any part of the coast, to parts not yet visited by it.

This appears to have been what was contemplated by the committee appointed at the Aberdeen Meeting of the British Association, under whose sanction the system of meteorological telegraphy was commenced. It was most distinctly the plan proposed by M. Le Verrier in a letter to the Astronomer Royal a little later.

The first warnings were issued in February, 1861, and the first forecasts of weather in August of the same year. The service continued almost without alteration for about five years.

Doubts were entertained before Admiral FitzRoy's death, as to whether he was justified in pronouncing so positive opinions as to coming weather as he did; and in 1866 the committee of inquiry reported strongly against the system as actually carried on, and the Royal Society only consented to appoint a committee to take charge of the office on the express proviso that they should not be called upon to issue storm-warnings. The Royal Society recommended that if these were to be continued, the Government should itself find means of carrying them on. This was not done, and their issue was suspended by the Government in December, 1866.

The warnings were, however, very popular; several memorials were presented to Parliament, praying for their restitution; and ultimately in May, 1867, the committee were requested by the Board of Trade to give some intimation of storms.

They at once, three months prior to the Dundee Meeting of the British Association, consented to issue intelligence of the nature of that originally contemplated, *viz.* to communicate to the ports the fact of the actual existence of an atmospherical disturbance, or, in other words, of a storm, or the signs of an imminent storm on any part of the coast, without implying the faintest idea of a prediction that the storm would probably strike the ports warned. They simply give the information to such ports as they think likely to profit by it, leaving the recipients to draw their own conclusions as to whether the storm would come to them or not, but furnishing them at the same time, by means of circulars and the 'Fishery Barometer Manual,' with the most approved and trustworthy rules for forming a judgment as to weather, when aided by observations of the Fishery Barometers so liberally supplied to fishing stations by this office and by the National Lifeboat Institution.

The drum signal is hoisted whenever a message is received from London, and it remains up for thirty-six hours, during which time the weather will probably have declared itself. The general facts which have led to the issue of the order to hoist the drum are conveyed in the telegram, which is posted up for the public to read at many of the ports; while information as to weather previously prevailing elsewhere may be obtained from a copy of the daily weather report of the day before, which is sent regularly by post to any port which asks for it.

The system has now been in operation for fifteen months, and the drum is hoisted at 100 British stations, and in addition intelligence is sent to the entire coast of the continent from Norway to Spain.

The French Government co-operates most warmly in the scheme.

It is obvious, that in order to make full use of the present system, it is necessary that the facts which have been made known by telegraph should be made public. In order to convey to shipping and to persons at a distance from the flagstaff this intelligence, a semaphore was invented by my colleague, Captain Toynbee, which shows at one glance the direction and force of the wind, and the locality where the storm is blowing. A diagram explanatory of the semaphore and its use has been printed and distributed widely. In order to test the apparatus, it has been erected on trial at three ports, London, Liverpool, and North Shields, but no definite decision as to its use has as yet been arrived at.

We now come to the practical utility of the present mode of giving intelligence. It is obvious that such a system can do comparatively little for our exposed Atlantic coasts, but fortunately those are not the shores most frequented by shipping.

As most, *not all*, of our storms are felt first on the west coast of Ireland, we are usually able to give intimation to the ports on the Irish Sea, the English Channel, and *a fortiori* the North Sea, before the gale comes on. This is a natural consequence of the general easterly motion of storms. When the test of results is applied to messages sent to distant stations, we find that out of thirty-seven messages sent to Hamburg in the course of last year, nineteen were followed by gales, nine by strong winds, six by no change of weather at all, while in only three instances did the storm precede the warning.

The possibility of such a system of warnings as Admiral FitzRoy proposed depends mainly on the knowledge of three things:—

1. The signs of approach of a storm.
2. The direction of its motion.
3. The rate of its progress.

Uncertainty as to any one of these points is fatal to the accuracy of the forecast. Now, even if we have discovered that a storm is coming on, some interval of time must elapse before we have learnt the course it will take and its speed of progression.

Had we a series of advanced posts outside our coasts, such as the line of observing-vessels anchored in deep water off the Irish coasts and connected by telegraphic wires with the shore, a plan officially proposed by a foreign meteorologist of eminence, we might hope to gain some earlier information than is now to be had; but meanwhile we must wait until the proposer of the scheme has discovered the secret of mooring vessels in water as deep as is to be met with there. As it presents itself at present, the problem is somewhat similar to what the determination of the path of a comet would be were we to be called upon to lay this down from the first set of simultaneous observations made of it on a short, clear interval on a cloudy night.

Storms are simpler in their character on sea than on land, as the inequalities of surface divert the wind from its true course. Every one knows that it blows much harder on the coast than inland, and so storms have a tendency to pass along the channels, where the air, at least that stratum of it which is nearest the earth's surface, has greater freedom of motion. When a storm is first felt on the west coast of Ireland, it is always a difficult matter to say whether it will go up the Irish or the English Channel. When it reaches Holyhead or Penzance we know more about it; but then much time has been lost.

We are at times visited by storms travelling in an unusual direction, like that on Easter-day, which came down from the north, and wrecked the 'Ferret' at Dover; or moving at such a rate that they are upon us before we are aware of their approach. These are the most dangerous of all. A gale at the end of a long spell of bad weather in winter does comparatively little damage to shipping, for all colliers and coasters are safe in harbour; but a sudden storm coming after calm weather strews our shores with wrecks. Such was the gale of August 22, 1868, of which I exhibit a diagram. On the afternoon before it (the 21st), there were no serious signs of disturbance, and yet next morning there was a furious gale. News was at once sent to Liverpool, and it reached that port at noon; while the storm did not actually reach the Mersey until late in the evening.

Such a storm as this must have afforded a signal instance of failure of any forecasts of weather for two days in advance, had such been made at the time of its occurrence.

I must now proceed to give an account of the modes adopted in the office for the study of weather, and of the practical results which appear to be showing themselves. I say advisedly *appear*, for I dare not say more. I may perhaps venture to hope that the diagrams I have exhibited may possibly contain ideas which may hereafter, when carefully worked up, furnish materials for satisfactory weather study.

The first step taken by direction of the committee was the inspection of the stations in order to ascertain the trustworthiness of the reports sent up. This was effected in 1867, and the state of things revealed by it was far from satisfactory. The geographical position of the stations was in most instances well chosen; but their local situation was frequently very faulty. The reporters were telegraph clerks, who had been left to put up the instruments wherever they pleased; and accordingly the thermometers were at several places attached to the walls of railroad stations, effectually sheltered from the weather, and exposed to heat from engines blowing off steam. One was unavoidably reminded of the gentleman who put up his barometer in the open air, but hung his thermometer close to the study fire, fearing that so small and delicate an instrument might be damaged by exposure to the weather. The climax of mismanagement occurred at one very important station, where a boy of thirteen years of age was in charge of the instruments. His barometrical readings were unaccountably irregular; and after several letters had been written, an official reply was received from him, assigning as a reason for the unsatisfactory nature of his reports that the landlord of the house kept boiled cabbage in the room with the barometer. It is needless to say that no time was lost in dismissing this young gentleman.

The whole service is now carried on in a much better way. The reporters are more on the alert; thanks to the new and clear instructions which have been supplied to them, and to a constant system of inquiry which is instituted whenever anything goes wrong.

Weather study has now been carried on for two years, and daily weather-charts have been drawn, the examination and comparison of which has led to some results which appear encouraging.

A principle has been much before the public of late, which was first urged by Professor Buys Ballot, of Utrecht. It may be stated as follows:—

Stand with your back to the wind, and the barometer will be lower on your left hand than on your right.

[This law was illustrated by a diagram.]

This law is evidently that which holds good in cyclones; but it does not follow from it that the air never moves except in cyclonical sweeps. No matter how gently the wind blows, the law is found to be true. This fact in itself, however interesting, is of no use to us in enabling us to judge of *coming* weather.

Accordingly we set to work to test the relation which the differences of barometrical reading, reported every morning, had to the winds experienced during the succeeding twenty-four hours, and thus to see what indications storms gave of their approach, both as to direction and force, by means of the relations and distribution of atmospherical pressure. It will be carefully remembered that no consideration of temperature was taken into account at all.

The committee have allowed the results of this inquiry to be privately printed, and they are briefly these :—

If we take the area from Valencia to Helder, and from Nairn to Rochefort, we find that whenever the difference of barometrical readings between any two stations is 0·6 in. on any morning, the chance is 7 : 3 that there will be a storm before next morning, whose direction will have been indicated by the law, somewhere within the area covered by our network of stations. On the other hand, the chance is 9 : 1 that no storm will come on without having given unmistakable signs of its approach by means of barometrical readings, even though the absolute difference observed may not have reached 0·6 in.

As regards localizing the storm, the area was triangulated, and, for the lines thus obtained, factors were calculated, and the observed differences turned into gradients per 100 miles. Each morning these gradients were calculated, and the winds which succeeded them were compared with the gradients in their vicinity. On the assumption that a gradient of 0·12 in. per 100 miles indicated a wind of force 8 (Beaufort scale), the percentage of success of the law was about 60. The accordance with facts was greatest with S.W., while exceptions were most common with N.W. winds; a wind notably liable to "back."

These results show us that we do know something about the wind which is coming, from the atmospherical disturbances of pressure which precede it. We do *not* know how many hours after this disturbance of pressure has been observed the storm will burst upon us, nor how long it will last; but we *do* know that when the barometer is much higher in France than in Scotland, it is, at least, extremely unlikely that an easterly gale will come on us. The two great causes of failure of this principle of forecasting have been already pointed out. They are—irregularity, either in the direction, or in the rate of advance of the storm; any uncertainty about either of these points throws all our calculations out.

Hence we are driven to look for some principle which shall give us a deeper insight into the motion of the air than Buys Ballot's law does.

The results as to this law which have been obtained were arrived at by examination of the daily charts. Of these charts, a very large number has by this time accumulated, and, by their intercomparison, very interesting results are coming out.

It has been known of old by those skilled in the science, and in fact by every one that watches the weather, that certain types of weather have a tendency to recur. Thus, two of the diagrams exhibited form part of a ten-day series which occurred in January, 1868, and again in December last. The existence of such parallelisms of weather at once leads us to look for what may be called sequences of weather, in fact, the first step towards the discovery of a relation of cause and effect.

On January 22, 1868, a very remarkable state of things was observable. A westerly gale was blowing in the Channel, easterly winds all over England; a long trough of barometrical depression appeared on the line of demarcation between the two currents, and a thick fog was noticed in London. Next day the barometer rose very rapidly, with northerly winds, and, on the morning after, a terrific hurricane burst on Scotland, and did immense damage in Edinburgh. Now, on the 8th of December last, similar conditions of the co-existence of the two currents, equatorial and polar, were reported, and were followed at the same interval of time by the same sequence of storms. This would attract anyone's attention, and accordingly it was determined to investigate all cases in which the two currents existed, flowing at the earth's surface, the polar winds lying to the northward of the equatorial. This relation of the two currents (allowing for the difference of the hemispheres) has long been insisted on by Mr. Charles Meldrum, the secretary of the Meteorological Society of the Mauritius, who has had great experience in charting the weather of the Indian Ocean. A diagram has been prefaced from an illustration to a recent paper of his in the 'Journal of the British Meteorological Society.' In this paper he says that cyclones *invariably* commence between two parallel currents of air, the channel of the polar winds lying on the polar side of the equatorial winds. You will remember what has been already said about tropical meteorology by Humboldt, and although, with the limited superficial area from which our information is taken, we cannot say whether this principle of Mr. Meldrum's strictly holds true or not, we may say that there seem to be reasons for suspecting that it does. The conditions of coexistence of the two currents are rarely observed; but whenever they present themselves, a southerly storm, or at all events a sudden and considerable freshening of the wind from S.E. or S., *a wind which did not exist in the district before*,* is nearly sure to result within a day or so. I have selected two instances of this: one, a winter gale, which I have just described; the other, a summer one, in fact, that which preceded the naval review at Spithead in 1867.

Conditions similar to those of July 13, 1867, occurred three weeks ago, on the 8th instant, and were succeeded by southerly winds, which did not attain the dignity of a gale, but ushered in that extraordinary burst of warm weather which we had.

It will be seen that the points to which I have drawn your attention are simply bare facts. No attempt at a theoretical explanation has been made—matters are not ripe for that yet. What is mainly necessary for us, is to shape out the solid foundation stones, which will make but little show hereafter, but will have to bear the weight of the entire edifice of weather study—a study with the credit of which the name of Admiral FitzRoy will be ever and deservedly connected.

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* Out of 24 instances, 12 were followed by a storm after 2 days; 4 by a gale immediately; 7 were followed by a freshening of the wind from S.; and only 1 was a failure.

Land Meteorology of the British Islands.

We now come to the last branch of the operations of the committee, their self-recording observatories. I told you at the beginning of the discourse that, fourteen years ago, the Royal Society stated to the Government that there was not much chance of the establishment of a satisfactory system of land meteorology at that time, owing to the want of self-recording instruments which should render the observations uniform in method and independent of the personal convenience of the observers. This want has now been supplied, mainly by the efforts of the Observatory at Kew. The barographs and thermographs adopted have been almost entirely constructed there. The anemograph is the well-known one invented by Dr. T. Romney Robinson, of Armagh. Messrs. Beck, of Cornhill, have kindly lent a barograph and anemograph for exhibition to-night.

In the case of the last-named instrument, there is no difficulty in obtaining sufficient mechanical force to produce a record; for whenever there is wind the cups will revolve, and there will be abundance of mechanical power to make a mark on paper. When there is no wind, the pencil will not be moved. The actual mode adopted is very easily explained. If a pencil which is at rest be pressed against a cylinder which is covered with paper and caused to revolve, a circle will be traced out. If the pencil move parallel to the axis of the cylinder, a helix will be described, the pitch of which will depend on the relative velocities with which the cylinder and pencil are moving, and will thus measure the *velocity* of the wind. The instrument records *direction* on a similar principle.

When we come to the barograph or thermograph, we have no source of mechanical power, and must therefore call photography to our aid. Every one knows that at the top of the column of mercury in a barometer there is an empty space, the Torricellian vacuum. If a light, either of gas or of a paraffin lamp, be placed behind the instrument and a lens before it, an image of the illuminated space above the mercury can be thrown on sensitive paper, and so a photographic picture can be produced, the depth of which will depend on the level of the mercury in the barometer.

The arrangements of the thermograph are somewhat similar, with the exception that the gas-lamp must not be placed near the thermometer [the actual arrangement was shown in a diagram], and that the light is not transmitted through the empty space above the mercury, but through a small bubble of air introduced into the thread of mercury, which acts like a pinhole. The light, passing through this, produces a mark which develops into a little dot on the paper.

In both instruments the paper is stretched on drums moved by clockwork, so that continuous photographic pictures are produced.

[Barograms, enlarged to six times their size, were represented on a diagram.]

Such arrangements as these render us quite independent of the convenience of observers as to the particular hours at which they may like to take their readings. All the care that the instrument requires is attention to the lamp, winding up the clock, and changing the paper regularly.

Having got the instruments, the committee proceeded to select stations where they should be placed, so as to represent as completely as possible the various conditions of climate afforded by our islands. The Kew committee of the British Association at once consented that their observatory should be the central and normal establishment. To this were added two others in England: one at Falmouth, the other at Stonyhurst. Two stations were chosen in Ireland: one at Armagh, under the supervision of Dr. Robinson himself, and one on the island of Valencia, in order to obtain as westerly a position as possible. In Scotland, the Universities of Glasgow and Aberdeen have become stations. The committee were anxious to have founded another observatory in the extreme north of Scotland; but the funds placed at their disposal would not suffice for this extension of their scheme, however desirable it undoubtedly is.

It will be seen from what I have said that existing establishments have been made use of, in so far as their geographical situation would admit. This condition is of course paramount. Accordingly, the committee were compelled to create an observatory at Valencia, without doubt the most important position in these islands. The claims of Wick or Thurso for an observatory have not been overlooked; but funds as yet do not suffice for its establishment.

The whole of this observational system is centred at Kew, under the immediate superintendence of my colleague, Dr. Balfour Stewart, who supervises all the observatories, and examines all the records received from them to see that they are thoroughly correct and trustworthy, before they are sent on for discussion to the central office. This is the special department in which the two committees, that of Kew and of the Meteorological Office, work in such close accord with each other. Of this connection it can only be said that without its existence, it would have been all but impossible to have brought the system of observations of land meteorology to its present satisfactory condition at all.

The first observatories began work at the beginning of 1868, and Valencia, the last of them, six months later. Some months were necessarily consumed in getting so complicated a system thoroughly started, and accordingly it is only now that the discussion of the results furnished by the instruments is being commenced, so as to ascertain the mean values of the meteorological elements for each station and to trace the changes of weather.

It would therefore be premature to attempt to give an account of the work *done.* To show what is doing I exhibit a curve from Aberdeen, to show how irregular atmospherical pressure occasionally is. The second diagram represents a barometrical oscillation which passed over all the stations within twenty-four hours, and which shows how perfectly hopeless it is trying to give an account of such a phenomenon by means of eye-observations, no matter how frequently these are taken.

Similar diagrams might have been drawn to show the changes in temperature and in wind, but that which is exhibited is sufficient for an illustration.

Hereafter, if the office is allowed to continue, we may hope to be able to present the public with worked-out results as to the progress of weather across the country; but for this purpose it will be necessary that observations from additional stations should be obtained, to supplement the information derived from these seven fixed points.

This then is a very imperfect account of the work that the office has done and is doing. Many important points have necessarily been unnoticed, but you will, I doubt not, see that the work is strictly a national one, and such as could not be carried on except under Government auspices and at the public expense.

This country is to a certain extent pledged to it by its consent to the proposals of the Brussels Conference, far more so by the importance of investigating thoroughly our exceptional climate, and bearing a part in what is being so well done in Europe generally; but most of all by the intrinsic value of the science of meteorology to our agriculture, our fisheries, and our commerce.

[R. H. S.]

ANNUAL MEETING,

Saturday, May 1, 1869.

Sir Henry Holland, Bart. M.D. D.C.L. F.R.S. President,
in the Chair.

The Annual Report of the Committee of Visitors for the year 1868 was read and adopted.

The Books and Pamphlets presented in 1868 amounted to 116 volumes, making, with those purchased by the Managers, a total of 220 volumes added to the Library in the year, exclusive of periodicals.

Fifty new Members were elected in 1867.

Sixty Lectures and Twenty Evening Discourses were delivered during the year 1868.

Thanks were voted to the President, Treasurer, and Secretary, to the Committees of Managers and Visitors, and to the Professors, for their services to the Institution during the past year.

The following Gentlemen were unanimously elected as Officers for the ensuing year:—

PRESIDENT—Sir Henry Holland, Bart. M.D. D.C.L. F.R.S.
TREASURER—William Spottiswoode, Esq. M.A. F.R.S.
SECRETARY—Henry Bence Jones, M.A. M.D. F.R.S.

MANAGERS.

George Berkley, Esq. C.E.
William Bowman, Esq. F.R.C.S. F.R.S.
Charles Brooke, Esq. M.A. F.R.S.
George Busk, Esq. F.R.C.S. F.R.S.
Adm. Sir Henry John Codrington, K.C.B.
Warren De la Rue, Esq. Ph.D. F.R.S.
John Peter Gassiot, Esq. F.R.S.
John Hall Gladstone, Esq. Ph.D. F.R.S.
Wm. Robert Grove, Esq. M.A. Q.C. F.R.S.
George Macilwain, Esq.
The Duke of Northumberland.
William Frederick Pollock, Esq. M.A.
Robert P. Roupell, Esq. M.A. Q.C.
The Hon. John William Strutt.
Colonel Philip James Yorke, F.R.S.

VISITORS.

Andrew Whyte Barclay, M.D.
Charles Beevor, Esq. F.R.C.S.
John Charles Burgoyne, Esq.
Sir C. Wentworth Dilke, Bart.
Alfred Gutteres Henriques, Esq.
Sir Thomas Henry.
Thomas Hyde Hills, Esq.
Thomas Lee, Esq.
William Longman, Esq.
Edward Henry Moscrop, Esq.
Rev. Cyril W. Page, M.A.
Edmund Pepys, Esq.
The Lord Josceline W. Percy.
Arthur Giles Puller, Esq. M.A. F.S.A.
Robert Ballard Woodd, Esq. F.S.A. F.R.B.S.

GENERAL MONTHLY MEETING,

Monday, May 3, 1869.

SIR HENRY HOLLAND, Bart. M.D. D.C.L. F.R.S. President, in the Chair.

The following Vice-Presidents were nominated for the ensuing year:—

The Duke of Northumberland.
William Spottiswoode, Esq. F.R.S. the Treasurer.
George Busk, Esq.
J. P. Gassiot, Esq.

Lieut.-Colonel Archibald Campbell Campbell.
Sir William Dickason Clay, Bart.
William Wilbraham Ford, Esq.
John Benjamin Marsden, Esq.
Herbert Schloss, Esq.

were *elected* Members of the Royal Institution.

JOHN TYNDALL, Esq. LL.D. F.R.S. was re-elected as Professor of Natural Philosophy.

The Managers announced that in conformity with the Deed of Endowment, they had appointed **Michael Foster**, M.D. F.L.S. Fullerian Professor of Physiology.

The special thanks of the Members were returned for the following Donations to "the Fund for the Promotion of Experimental Researches":—

T. William Helps, Esq. (4th Donation)	£10
Erasmus A. Darwin, Esq.	25

The **Presents** received since the last Meeting were laid on the table, and the thanks of the Members returned for the same viz.:—

FROM

Actuaries, Institute of—Journal, No. 75. 8vo. 1869.

Asiatic Society of Bengal—Journal, No. 151. 8vo. 1869.
Proceedings, 1868. No. 12. 1869. No. 1. 8vo.

Astronomical Society, Royal—Monthly Notices. Vol. XIX. No. 5. 8vo. 1869.

Baldwin, James S. Esq. (the Author)—Power without Fuel. (L 15) 8vo. 1869.

Bombay Branch of the Royal Asiatic Society—Journal, Vol. IX. 1867–8. 8vo. 1869.

Chambers, George F. Esq. M.R.I.—Bp. Jebb, Speech in Defence of the Church in Ireland (June 10, 1824). (K 96) 8vo. 1868.
J. Jebb, Rights of the Irish Branch of the Church considered. (K 96) 8vo. 1868.
The Irish Difficulty. (K 96) 8vo. 1868.
H. W. Stewart, Diocesan Synods. (K 96) 8vo. 1868.
A. T. Lee, The Irish Church. (K 96) 8vo. 1868.
Our Political Situation; Letter to B. Disraeli. (K 96) 8vo. 1868.
E. Burton, Thoughts on the Separation of Church and State. (K 96) 8vo. 1868.

Chemical Society—Journal for April, 1869. 8vo.
Catalogue of the Library. 8vo. 1869.

Editors—Artizan for April, 1869. 4to.
Athenæum for April, 1869. 4to.
British Journal of Photography for April, 1869. 4to.
Chemical News for April, 1869. 4to.
Engineer for April, 1869. fol.
Geological and Natural History Repository. April, 1869. 8vo.
Horological Journal for April, 1869. 8vo.
Journal of Gas-Lighting for April, 1869. 4to.
Mechanics' Magazine for April, 1869. 8vo.
Pharmaceutical Journal for April, 1869. 8vo.
Photographic News for April, 1869. 4to.
Practical Mechanics' Journal for April, 1869. 4to.
Revue des Cours Scientifiques et Littéraires. 4to. April, 1869.

Franklin Institute—Journals, Nos. 517, 518, 519. 8vo. 1869.

Haughton, E. A.B. M.D.—The Laws of Vital Force in Health and Disease. 16mo. 1869.

Horticultural Society, Royal—Journal, Vol. II. Part 6. 8vo. 1869.

Inner Temple, Honourable Society of the—Students admitted to the Inner Temple, 1571–1625. 8vo. 1868.

Linnean Society—Journal, No. 49. 8vo. 1869.

Mechanical Engineers' Institution, Birmingham—Proceedings, July, 1868—Jan. 1869. 8vo.

Photographic Society—Journals, Nos. 203, 204. 8vo. 1869.

Plateau, M. J. Hon. M.R.I.—Recherches sur les Figures d'Equilibre, &c. Série IX. X. 4to. 1868.

Royal Society of London—Proceedings, No. 110. 8vo. 1868.

Statistical Society—Journal, Vol. XXXII. Part 1. 8vo. 1868.
Symons, G. J. Esq. (*the Author*)—Symons' Monthly Meteorological Magazine, April, 1869. 8vo.
Teyler Foundation, Haarlem—Archives du Musée Teyler. Vol. II. Fascicules 1, 2. 8vo. 1869.
Way, Albert, Esq.—Memoir of the Excavation of Three Tumuli at Broad Down, Farway, near Honiton. 8vo. 1869.

WEEKLY EVENING MEETING,

Friday, May 7, 1869.

Sir Henry Holland, Bart. M.D. D.C.L. F.R.S. President, in the Chair.

Captain Moncrieff,

On the Moncrieff System of Working Artillery as applied to Coast Defence.

Until the time of the Crimean war very little and very slow progress had been made in artillery. Cannon were manufactured on nearly the same models, and of the same materials that had been used for 300 years.

Before that time cast-iron was not in use, but the forged or bronze guns, although in some cases large, were not what is now considered powerful; and the penetration of their shot was not sufficient to pass through a parapet of earth that is now pierced even by light rifled artillery.

The conditions, therefore, under which artillery was worked, and the means provided for protection against its fire, remained much the same as they were in the time of Vauban.

Several events during the Crimean campaign confirmed an impression that has always been more or less entertained, that an increase in the power of individual guns produced greater results than could be obtained by a much greater weight of metal, distributed among a larger number of small pieces of artillery.

It is not too much to say, that the development of this art has, since 1855, changed the character of war both on land and water.

It has established completely the superiority of a few large pieces over a much greater weight of metal in smaller guns.

It has given artillery of all classes a range, a penetration, and an accuracy of *Fire*, which throw into the shade the greatest results that had been previously obtained.

It has also stimulated the advocates of cast-iron smooth bores to produce guns that might rival the rifled artillery; and yet it is by no means probable that the limit of power, either of large smooth-bores or rifled guns, has been arrived at.

When it became apparent that mighty results were to be obtained from improved artillery, a great deal of engineering talent was directed to the subject. Comparatively new appliances, such as the steam-hammer, and new methods of working steel, were called to aid in the construction of the new and powerful guns. So much interest, indeed, was taken in the subject, and so much attention absorbed by it, that the conditions which these improvements in artillery themselves imported with them ran some danger of being neglected.

The power of artillery became so great, that the ordinary provisions for protection against its fire were rendered useless. Forts that were considered strong twenty years ago would crumble under the shock of modern projectiles, and in some cases would even be too weak to support the guns while they were fired.

That service which the new artillery affected most palpably was the Navy, and the Navy accordingly took the initiative in introducing means calculated to resist the penetration of the new and terrible projectiles. Every one is more or less conversant with the process that has been going on of covering ships' sides with iron, which has increased in thickness till it really looks as if the process at last would only be limited by a ship's power of flotation.

War-ships, however, not only protect their sides against shot, but they also carry the heaviest artillery on their decks. This fact could not be overlooked by those who had to construct coast defences, as well as other works against which modern heavy artillery might be used.

I shall not enter into details regarding the successive steps which were taken in England in this direction, as I understand Colonel Jervoise has already done so in this Institution. It is enough to state that great engineering skill has been exercised, and unwearied efforts have been made to meet the new conditions.

That skill and these efforts have, with the experiments at Shoeburyness, given us defensive iron structures which are marvels of strength and ingenuity. Unfortunately they are also marvels of costliness; and there is room to hope that their use will therefore be generally confined to such positions on *land* as can only be protected by such iron structures.

This hope is founded on another system, with which my name is connected, and which I am here to explain.

Before doing so I shall point out the dilemma which left military engineers no alternative, and which compelled them to give up in succession the use of earth, concrete, granite, &c., and at last to resort to the most expensive, but the strongest, material—*iron*.

There are two considerations always to be taken into account in providing the means of using artillery: the one is to place the gun so as to be most formidable to the enemy, and the other is to place it at the same time under as much cover as possible, so that *it* is not liable to be disabled, nor are the men serving it liable to be destroyed by hostile fire.

These two conditions interfere with one another; that is to say, whatever has hitherto been gained in one direction has been lost in the other. Guns, *en barbette*, lack protection; guns in embrasures or in casemates sacrifice, on the other hand, free lateral range, and it is more difficult in their case to see the enemy, and therefore to lay the guns in action.

The difficulty that presented itself with the introduction of late improvements in artillery was simply that the increased precision and range, coupled with great improvements in the manufacture of large shells and also in small arms, rendered barbette batteries too exposed to be relied on. At the same time the tremendous penetration and precision of the new artillery rendered the ordinary parapet and embrasures useless.

What was to be done under these circumstances?

Protection from direct fire must be got at any price.

The first impulse would be, to thicken the parapet.

This could not, however, be done, as the necessary angle in the cheeks of the embrasures required for training the guns opens up a wider aperture, in direct proportion to the thickness of the parapet, making the *maximum* thickness in practice 30 feet.

But shot have been known to penetrate more than 30 feet into the earth; and the most important part of the parapet, *viz.* that near the guns, must always be thin and weak, whatever may be the thickness of the rest.

Shells, striking this part, would just meet sufficient resistance to burst them, and would make havoc among the men.

Next, granite masonry was thought of; but it proved in some respects worse than earth, and was found practically bad; there was no alternative but to go to iron. This conclusion was reluctantly arrived at, and reluctantly it was acted on.

The decisions of committees which investigated all the bearings of the question, the opinions of professional men, and the experiences of the American war, all coincided, and accordingly our important coast-works were designed to receive *iron* shields, casemates, and cupolas.

Vital positions in England, such as dockyards and arsenals, must be fortified. It would be false economy indeed to use any method of fortification that experience has proved to be insufficient. *No savings* could justify the erection of works that might prove at once the tomb of their defenders and perhaps of the nation's honour. Therefore the only proper decision was, to take that means to meet the difficulty which was at the time considered best and safest. Expense was properly a consideration very secondary in importance to efficiency.

I shall now endeavour to point out the difficulty of the task which lay before the engineer, even after the decision in favour of iron, from the extraordinary advances, already spoken of, in artillery. There is only one morsel of comfort left for those who have to provide for the requirements of defence, *viz.* that a form of artillery-fire of a very galling nature remains exactly as before, and indeed is not much better than it was in the time of Queen Elizabeth.

What is alluded to is vertical, or mortar fire. There is some consolation, too, in the reflection that the cause of this fire not being much improved is one to a great extent likely to be lasting. Rifled mortars would no doubt lessen deflection to right or left; but as long as gunpowder is affected in strength by the slightest atmospheric or other influence, and still more certainly as long as a slight error in elevation at long ranges will make a large error on the plane of fire, the comparative inaccuracy of vertical fire must continue.

To show how little can be done in this way compared with the admirable precision and accuracy of direct fire, I may state that 100 rounds were fired one day last season at Shoeburyness at 800 yards range with a 13-inch mortar at the row of experimental casemates which cover a good deal of ground. The mortar was laid with spirit-levels and all the appliances of the school of gunnery, and yet the 100 rounds were expended without a single hit.

If such is the case with a steady platform and under such exceptionally favourable circumstances, it can easily be seen how uncertain in its effects would practice be from mortar-boats, which move with every wave, if directed at an equally small object. During the eleven months' siege of Sevastopol the French had 242 mortars engaged, which were themselves exposed to vertical fire, and yet not one of these mortars was disabled.

It is indeed a strange contrast, that while direct fire is getting more powerful, more accurate, and more destructive every year, vertical fire remains much as it was, and can only be relied on to hit a large object, such as a fort, a town, or anything that covers a great deal of ground. Notwithstanding this, it would be a great mistake to despise it as a powerful and galling means of attack.

To return to the difficulties of meeting direct fire in coast defence.

It must be borne in mind that batteries intended to engage ships are obliged to meet an enemy who can move his position to that quarter where he is least exposed, who can continue in motion while he is conducting his attack, and who can seek out the most vulnerable face of the land-work to operate upon.

In constructing such batteries it is first of all necessary to make them of sufficient strength to resist the guns of ships which are the most powerful that can be made.

It is next required that these batteries should be constructed in such a manner that they can direct their fire with rapidity and precision in any direction in which the ships can take up their position.

And lastly, it is required that they should mount guns of sufficient weight and power to be formidable to the heaviest iron-clads.

In former times guns *en barbette* were preferred for this purpose, because they met the two first requirements alluded to; that is to say, that from not being confined by embrasures or ports, they were able freely to follow their floating enemy whatever position he might take up, naval fire at that time being neither so correct nor so formidable as to make such batteries unserviceable. The case, however, is now completely changed; for not only have guns been improved, but ammunition also; and heavy shells are most destructive. Rear-Admiral Porter, of the United States Navy, in a report on coast defences, says, "Such guns, *standing so high up*, are just the objects that naval gunners would delight to explode their Shrapnell against, and from my experience in naval gunnery, the third shell would kill every man at the gun."

Von Scheliha, in his treatise on coast defences, says, "Guns mounted *en barbette* may always be silenced by an iron-clad."

This form of battery, therefore, is disposed of.

We shall now examine the difficulties connected with the other alternatives. Common masonry batteries have been condemned as worse than useless, as they would only make the ship's fire more destructive than if directed against guns *en barbette*.

Next comes the expensive alternative which has been adopted, *viz.* iron shields, casemates, and turrets. It is most interesting to examine how far this system of iron, the last alternative left, meets the three requirements of coast defence alluded to, and to see what very great difficulties had to be encountered in applying it.

The three requirements are thus recapitulated:—

1st. Strength of the battery to resist naval fire, and give sufficient protection to the men.

2nd. Power of fighting the guns with accuracy and effect, of following the enemy with ease as he moves, of being able to face him on any side from which he approaches.

3rd. Power of using the most formidable guns to advantage.

The first difficulty was to decide the matter of strength.

Now guns are becoming more and more weighty and powerful every day, and therefore the strength required to resist them is an unknown quantity.

An iron casemate of the present proposed strength costs, according to official returns, with all the battery adjuncts except the gun and carriage, about 5000*l.* or 6000*l.* for each gun. A 2-gun turret, about 25,000*l.* or 30,000*l.**

If guns of 50 tons are introduced in ships, as is proposed, these defences are at once quite inefficient, and it is not known how strong or how expensive should be the iron works to replace them. Such questions must be very embarrassing indeed to those who have to decide these matters. Besides protecting the gun and carriage from the enemy's shot, protection must also be given to the men. This is the most serious of all considerations in coast defence, for the following reasons:—

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* The price of a permanent Moncrieff battery, with magazines, &c., including the extra expense of carriages, is from 1100*l.* to 1500*l.* for each gun; an iron shield battery from 1800*l.* to 2000*l.* per gun; an iron casemate battery, from 5000*l.* to 6000*l.* per gun; a turret, from 12,500*l.* to 15,000*l.* per gun.

The best experience we have regarding naval attacks on land-works is derived from the late American war, in which a great many actions of that kind took place. It would be unwise to ignore this experience, because the increasing power of artillery only gives it more weight.

During the whole of that war very few guns were destroyed by the naval fire in earthen batteries.

At Fort Wagner only three guns were totally dismounted, although 2864 shot and shell were fired into it in forty-eight hours, and the bomb-proofs were hit 1200 times. Seventeen siege-mortars, several cohorns, and thirteen heavy pieces of artillery were incessantly employed.

At Fort Fisher the bombardment was opened at the rate of 115 shells per minute, and although the guns were mounted *en barbette*, only two of them were dismounted when the place fell.

At Fort Powell a tremendous bombardment from mortar and gun-boats (the most accurate firing being from 15-inch mortars) was maintained from 22nd of February till 2nd of March, and not a single gun was dismounted.

The success of the ships over the forts was gained by demolishing the works, and still oftener by making the service of the guns so dangerous that the men could not work them.

Rear-Admiral Porter, U. S. Navy, in his report on coast defence, states, "The new-fashioned casemates turned out to be no better than the guns *en barbette*. They were perfect slaughter-houses, and were piled up with dead and wounded. Every shell that went through the port-holes killed and wounded every man in the close casemate. This proved to me most satisfactorily that guns in casemates were no better protected from shells than those *en barbette*."

With such evidence as this before them, from men who were conversant with all the events of that great war, it was indeed a serious question to decide what was to be done. I myself cannot see how men in an iron casemate are as much exposed as in a barbette battery; but there is no doubt that if the port of the strongest casemate was as large as those referred to by Admiral Porter, it would be open in the same circumstances to the same dangers as the damage was done by entrance of shell through the port.

The protection a casemate would afford from vertical fire in such a case would be but a poor advantage if more correct and more deadly weapons than the mediæval mortar could still search out at times the exposed point of the casemate and kill every man inside.

The next requirement in a coast battery, *viz.* to be able to follow an enemy amidst clouds of smoke, and to lay the guns on him with precision and dispatch, formed a more embarrassing difficulty still.

On the one hand, the ports must be constructed for muzzle-pivoters to give protection. On the other hand, if they are made so small it is difficult to see through them, to fire correctly and quickly at different elevations, and on different sides on a moving enemy.

The battery is in the position of a knight who must either expose his vitals to his enemy's lance or put on armour that paralyzes his sword arm.

There is as much protection in the power of being able to strike as there is in being able to guard.

As naval actions are likely to be short and decisive, it must have appeared extremely doubtful whether it was worth purchasing increased safety at the expense of losing the attacking power.

The last of the three requirements in coast defence stated was the necessity of using the most powerful cannon.

This did not present the same difficulty as the other two, because the designers of our defences had been presented by my friend Captain Coles with the means of mounting the heaviest guns to fire in any required direction. When very large and valuable guns are used, it is not advisable to cramp their action and restrict it to a small area. The turret was therefore preferred to the casemate when lateral range was required; and though apparently very expensive it was in reality cheaper than casemates, because, although the mounting of the guns in this manner cost more, they were enabled to do much more work, and there was thus an economy both of guns and men.

Having thus far endeavoured to describe the extraordinary difficulties which the new improvements in artillery inevitably entailed on the engineers, I shall now direct your attention for a short time to the difficulties in which the same improvements involved the artillerymen themselves.

These difficulties, though not quite so important as the engineering ones, were very serious indeed, and have not yet been quite overcome. They consisted chiefly in the difficulty of making carriages and platforms strong enough for the new and powerful rifled guns. These pieces burnt enormous charges of powder, and hurled bolts as heavy as an old field-piece at 1000 feet a-second.

The recoil of such guns represents a violence of force the like of which man has never had to deal with before. Imagine 12, 18, or 25 tons of compact iron started in an instant into rapid motion with a violence that mocks the blow of a steam-hammer.

This force has to be controlled and restrained. It is no wonder then that, when met directly and stopped by friction, as is now done in the ordinary system, the difficulties are enormous. The horizontal strain on the platforms, pivots, and racers, is so great that it has not yet been quite successfully met: constant changes and inventions are being made to render this force more harmless.

I hope I have now conveyed to your minds some idea of the embarrassment and difficulties which have fallen upon both the artillery and engineers by the rapid improvement of these formidable engines of war; and of the persistent and able struggle which both have maintained to meet directly the terrible forces with which they have to contend.

They have both succeeded to a wonderful extent, but their success is blighted by that curse of the science they practise; the law that up to this time has existed—*viz.* that what was gained in protection was lost in efficiency, and the converse.

Happily I had the good fortune to conceive and develope an idea which abrogates this law. The very force the existence of which has been so great a difficulty in the artillery question has been compelled to perform a service that at once sweeps out of existence a great many of those other difficulties that embarrassed fortification.

When two evils co-exist, it is sometimes good policy to make them destroy each other.

I shall now refer shortly to the train of ideas that led me to think of solving the important problem in quite a different manner from that in which it had been attempted, which had led to the adoption of a most expensive class of works.

My solution gives a system capable of mounting the heaviest artillery, while it simplifies the vexed question of fortification. It gives protection without the expense of using iron, and free lateral range to the guns without exposure.

The system is indeed a simple one; it does not require either brute strength or heavy expenditure for its application; nor does it need mighty forges to weld iron walls to protect our guns and gunners; it only calls to our aid the simplest and most docile forces of nature.

Instead of trying to meet force by force, I make my guns bow to the inevitable conditions which science has imposed; and instead of wasting energy, money, and skill in attempts to raise a buttress against the new artillery, I employ the hitherto destructive force of recoil to lower the gun below the natural surface of the ground, where it can be loaded and worked in security and in comfort; and, at the same time, I have made that destructive force so much my servant that I compel it at my pleasure to raise the gun again into the fighting position whenever it is required.

In 1855, while watching the interesting operations before Sevastopol, and endeavouring, as well as I could, to understand the conditions under which the siege-artillery was used, I conceived the idea which is now realized. It was then that I saw the value of earth and the importance of simple expedients.

It was plain that the weak point of a battery was the embrasure, which formed a mark to fire at, an opening to admit the enemy's shot, and required constant repair even from the effects of its own gun, which in firing injured the revetments of the cheeks.

I also came to the conclusion in my own mind that a remedy for some of these defects could be devised. Afterwards I worked at various plans, of which sketches were made or models; but each design had defects which discovered themselves to me as my experience increased.

The real difficulty of the thing arose from the necessity of providing for the enormous strain of the recoil.

These early designs, which were sometimes excellent in other respects, broke down at this difficulty, and although some of them no doubt would answer with small guns, they were not calculated to meet the tremendous recoil of large rifled pieces.

At last I hit on a simple principle that would meet this difficulty to advantage, the interposition of a moving fulcrum between the gun and platform. Then I knew that the problem could be solved; and feeling the great importance of the subject, I resolved to devote my efforts to working it out completely.

While directing my attention to this simple and then apparently obscure matter, I was, as you may imagine, neither an idle nor disinterested watcher of the progress of artillery. Every step in advance was riveting the certainty in my mind that the system would one day be required, and with this conviction I refused to allow either discouragement or delay to make me desist. I shall now endeavour to explain shortly the system which bears my name, as far as it relates to coast defence.

It consists of three parts:—

1st. The mechanical principle of the gun carriages.
2nd. The form internal and external of the batteries.
3rd. The selection of ground for placing the batteries, and the arrangement for working them to the greatest effect; or, in other words, the *tactics* of defence for positions where the system is employed.

The principle on which the carriage is constructed is the first and most important part of the new system, because on it depends the possibility of applying the other parts. This principle may be shortly stated as that of utilizing the force of the recoil in order to lower the whole gun below the level of the crest of the parapet so that it can be loaded out of sight and out of exposure, while retaining enough of the force above referred to to bring the gun up again into the firing or fighting position.

This principle belongs to all the carriages; but the forms of these carriages, as well as the method in which this principle is applied, vary in each case.

For instance, in siege-guns, where weight is an element of importance, the recoil is not met by counterpoise.

With heavy garrison guns, on the other hand, which when once mounted remain permanent in their positions, there is no objection to weight. In that case, therefore, the force of gravity is used to stop the recoil, because it is a force always the same, easily managed, and not likely to go wrong; and as these carriages are employed for the most powerful guns, it is a great advantage to have the most simple means of working them.

It has been already mentioned that the principal difficulty arose from the enormous and hitherto destructive force of the recoil of powerful guns; and here I shall point out the manner in which that difficulty is overcome.

That part of the carriage which is called the elevator may be spoken of and treated as a lever; this lever has the gun-carriage axle at the end of the power-arm, and the centre of gravity of the counter-weight at the end of the weight-arm, there being between them a moving fulcrum.

When the gun is in the firing position the fulcrum on which this lever rests is almost coincident with the centre of gravity of the counter-weight, and when the gun is fired the elevators roll on the platform, and consequently the fulcrum, or point of support, travels away from the end of the weight-arm towards the end of the power-arm, or in other words it passes from the counter-weight towards the gun.

Notice the important result of this arrangement.

When the gun is fired its axle passes backwards on the upper or flat part of a cycloid. It is free to recoil, and no strain is put upon any part of the structure, because the counter-weight commences its motion at a very low velocity. As the recoil goes on, however, the case changes completely, for the moving fulcrum travels towards the gun, making the weight-arm longer and longer every inch it travels. Thus the resistance to the recoil, least at first, goes on in an increasing progression as the gun descends, and at the end of the recoil it is seized by a self-acting pawl or clutch.

The recoil takes place without any jar, without any sudden strain, and its force is retained under the control of the detachment to bring up the gun to the firing position at any moment they may choose to release it. The recoil moreover, however violent at first, does not put injurious horizontal strain on the platform. In my experiments at Edinburgh with a 32-pounder, I found that so slight was the vibration on the platform caused by firing, that the common rails on which the elevators rolled in that experiment, and which were only secured in the slightest manner, did not move from their position, nor even when heavy charges or double shot were used, did sand and dust fall off their curved tops.

At a still earlier experiment made with a model of a 95-cwt. gun, the model was fired on the ice with excessive charges, and nevertheless remained stationary.

This valuable concomitant of the system cannot be appreciated fully without referring to the difficulties that have been experienced, and are now felt, in getting pivots, platforms, &c., on the ordinary system strong enough to mount the new artillery, where the recoil is stopped by friction applied directly by means of what are technically called *compressors* attached to the platform.

I shall not detain you by detailing these difficulties, but will only state that the first two 12-ton guns on ordinary carriages that were fired in casemates (which happened a few months ago) at Gilkicker Fort were both *hors de combat* the first shot. This alarming event showed that with all the experience of ancient and modern artillery (and the carriages referred to were the legitimate exponents of the results of that experience), there was still room to doubt whether the problem of meeting recoil had been at that time completely solved by the existing system.

The accident referred to was serious, because it might occur in action, and in that event would disable the gun, *pro tempore*, as completely as if it had been dismounted by a shot.

Some credit may be claimed for the new system, on the ground that it provided a carriage for a heavy piece of artillery on an entirely new principle, in which not a single part was copied from anything that had been formerly used, dealing with new conditions and performing new functions that no other carriage had done, and yet this new carriage (the first complete one of its kind) has now fired *two hundred rounds*.

This practice has been carried out with only a few accidents which pointed to defects in the gearing, which were easily remedied.

By treating this violent force in the manner above described, a good deal of the strength that is required in other systems becomes unnecessary, and at the same time the recoil, however violent, can not only be met, but utilized.

Together with the carriages there are some improvements of minor importance, such as trunnion pointers, reflecting sights, graduated racers, and so on, which it would be out of place to discuss at present, but which contribute to the efficiency and completeness of the system, and are more or less required for carrying it out as a consistent whole for coast defence.

The second part of the system, *viz.* the profile of the batteries, is of the highest importance, because unless it is attended to great advantages are lost.

This, unfortunately, makes the system extremely difficult of adaptation to existing works. In order to get the full advantage of it no exterior slope of parapet should be exposed to the view of the enemy. This prevents him from being able to tell whether the fire be correct or wasted, and affords no means to him of correcting error.

The battery in fact is masked; so that at some distance, or in dull weather, a moving ship would have considerable difficulty in laying her guns on one battery, and still more difficulty if there were several batteries judiciously placed for the purpose of deceiving the eye.

It can easily be understood that the slightest error in elevation would either carry the shot harmlessly over the battery or else cause it to ricochet off the glacis or superior slope.

In fact when the gun is down the enemy has nothing to aim at but an undefined horizontal line.

In connection with this I should mention a very interesting fact, brought out by General Simmons at the last discussion of the Royal Engineers on a paper of mine.

He stated that on analyzing the range reports of the Armstrong and Whitworth competitive trials, which were very carefully conducted, he found that the mean horizontal and vertical errors were very different.

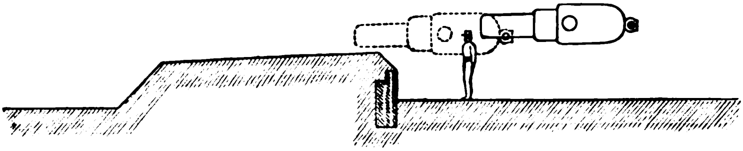
The horizontal error increased almost directly as the range, that is to say, at 400 yards it was four times as great as at 100 yards, but that the vertical error went on in a rapidly increasing progression, showing that it would be much more difficult to hit a low object than a high one of the same area.

This law has an important bearing on the subject, and should not be lost sight of in designing defensive works of any kind.

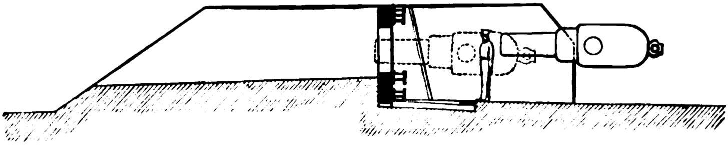
It will be observed that the interior slope of the parapet gives the most complete protection to the men, especially when the dome-form is adopted.

Sketch showing in Section Specimens of Five Methods of Mounting Heavy Coast Artillery.

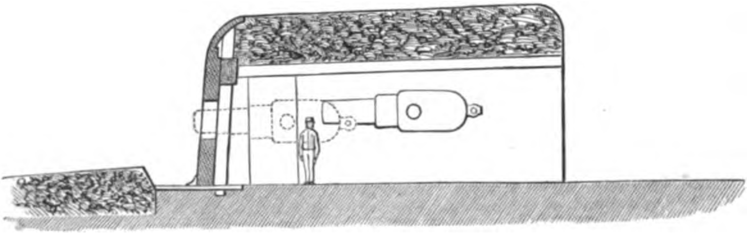
BARBETTE.



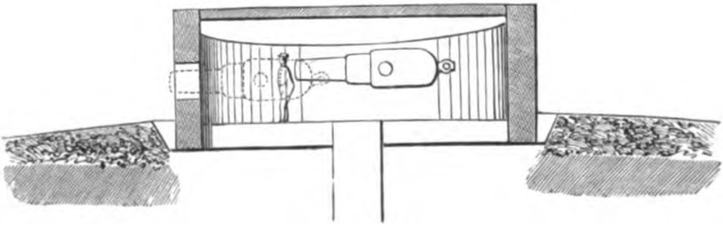
SHIELD.



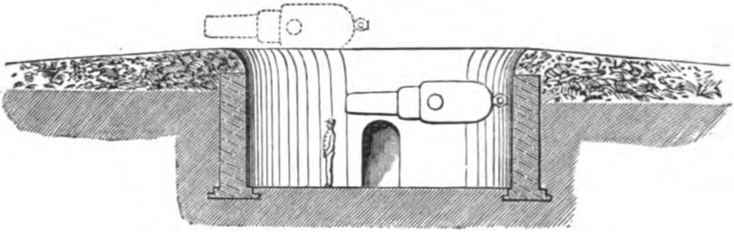
IRON CASEMATE.



IRON TURRET.



MONCRIEFF.



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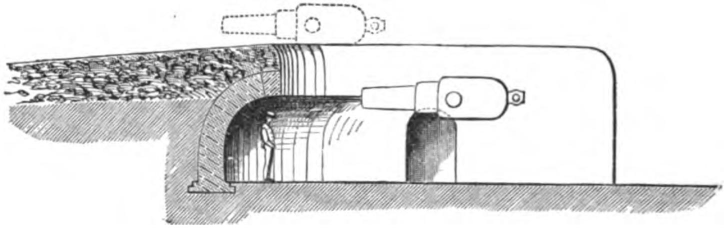
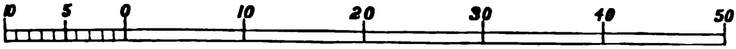




Scale of Feet.

Up to the present time the new system has only been considered as an improvement, and its value has only been estimated as an adaptation to existing forts, and there are no proposals for applying it *per se.*

I am extremely anxious to impress on you and on my countrymen that its full value cannot be seen in this manner, and that it suffers injustice by being thus treated. I trust its proper use will be fully discovered before the inevitable lesson is dictated by war, and that it may be applied in works expressly designed for it, and not merely adapted to its use.

The third part of this system consists in its application to given positions, the disposition of the batteries, and methods of working them in concert with or in support of each other.

If I might be excused for using the paradox, the system for coast defence consists in the absence of any defined system; that is to say, instead of making large regular forts, and forcing surrounding circumstances into harmony with them, every accident of the ground in this case would be seized, where available, and small batteries, consisting of a few guns, or even one powerful gun, laid down so as not to take away the natural aspect of the position.

These batteries would be well retired from the channel, and placed so as to support each other in case of attack, and should, when circumstances permit, afford flank defence to each other, in conjunction with obstacles of any character that could be conveniently employed, and with strongholds for infantry and light artillery, commanding, if possible, the sea-batteries, so as to make them untenable by an enemy, and so placed as to be in the best position for a reserve, ready to support any point attacked; the whole connected with good and sheltered roads.

In stopping the passage of a navigable river or channel, for instance, the guns, instead of being massed, would be scattered round the points where marine obstructions were placed.

These guns would be disposed in such a manner as to retain as much as possible for the defence the advantages of a free lateral range, converging fire, and different amounts of command. In other words, the method consists in placing in position the heaviest and most powerful artillery to the greatest advantage, making that the first consideration, and afterwards protecting the batteries, by separate and distinct arrangements easily devised by officers on the spot, against assault by any force that ships might land for that purpose.

When an object is to be attained, I prefer to grapple with the most difficult and important part of it first,—do that well,—and meet the other requirements afterwards, with as little loss of efficiency as possible.

The first object of coast defence is to meet and defeat the attack of powerful ships; the next is to protect the shore-batteries against landing parties.

It must not, however, be forgotten that there are positions of such importance that they might be attacked by an army on land. Such positions must either be defended by another army placed in a favourable position by such arrangements as those above referred to, or else by regular and complete earthworks thrown up in time of danger, which would enable a still smaller garrison to resist anything but regular approaches.

There are, however, few coast positions of such importance as to draw the attack of a whole army; and such positions, as a rule, are now provided with regular works of a very high order; whereas there are many positions exposed to a heavy naval attack, such as our large mercantile ports, &c. They are almost invariably centres of population, who require only fieldworks and good small arms (which are now more powerful than ever) to repel the most determined attacks of any numbers that war-ships could land.

I believe many of the *present* coast-works are defensible only against a *coup de main.*

Wherever land attack is of more importance than naval, the character and efficiency of sea-batteries must give precedence to those considerations which provide against assault. On the best provisions for meeting this I do not pretend to give an opinion. In such cases, the possibility of attack by both direct and vertical fire must be kept in view.

Where my system is employed for arming such works, one or two precautions would increase the power of resistance.

1st. The large guns for operating against ships, with traverses and *parados* to each, should be kept as far apart as space will admit.

2nd. Ample and thoroughly-complete bomb-proof cover for the whole garrison should, if possible, be supplied in the middle of the work, with arrangements for interior defence (not barracks, but places for emergency), thoroughly secure from vertical fire—good and healthy barracks for the men being made independent of the works, and by preference kept out of the way.

3rd. Howitzers and light artillery ought to be kept in reserve, in bomb-proofs constructed for the purpose, and (with the new system this can easily be done) also with the means of changing these to any required face.

The dispositions of defensive batteries such as those I have very imperfectly attempted to describe would not be complete without good arrangements for internal communications, not only by roads, but by telegraph, with a clearly laid down and simple method of working them; that is, not liable easily to go wrong, nor to lead to mistakes, and which would not require very high skill.

Such arrangements would increase the power of the defence, and indeed would be necessary with the detached system.

I have accordingly given them some attention, and designed a general plan of laying off the ranges and working the telegraphs, which will make it possible to supply simultaneous information.

The system I refer to (which has been submitted to the Director-General of Ordnance) would apply to any position, but its particular application would vary in each case.

It is extremely simple. One part of it depends on electrical instruments which I have invented for the purpose, and which, without either calculations or experience, give the range and positions of an indicated ship at every gun in the position.

Another part of it enables the officer directing the defence to deliver in one instant, by the touch of his finger, a converging volley from one or both sides of a channel on a vessel sailing past.

The possibility of delivering correct fire in this manner on a moving object, without aiming, and by an officer not even in the battery, was illustrated in one of my experiments with the 7-ton gun-carriage at Shoeburyness; and I trust I may be given some day a chance of showing to what perfection this system can be carried.

Methods of determining the distance of vessels from batteries are practised here and in some continental countries. My method is designed to be quicker, simpler, and therefore more effective. It is adapted to work in conjunction with the arrangements for submarine mines. That part of it which gives the required information for sighting the guns is of so simple a character, that the most uneducated gunner cannot make a mistake in its application.

There are many other features of the system besides those I have particularly referred to which I shall not now discuss; each requires different treatment.

Among these there are methods of mounting guns in ships, in floating-batteries, Moncrieff-carriages for heavy guns of position, adapted for locomotion, for coast-defence, siege-carriages, &c.

I may remark in passing that some of these applications are considered by officers of eminence to be quite as important as the class of Moncrieff-carriages best known.

For instance, I take the liberty of quoting from a letter I received from Colonel Brialmont, the great Belgian engineer and military writer, in November, 1868. He says:—

"I am at present engaged in publishing a great work on fortifications. I shall naturally speak of your invention in it, and if agreeable to you I shall likewise mention your proposal with regard to *barbette* system in batteries of attack. I believe this idea is destined to have a great future. This last invention will perhaps bring you less renown than the one you have experimented on at Shoeburyness, but it will have a more general and easier application."

I am most anxious to impress the national importance of this question of coast defence in relation to the system of earthworks which are now possible.

The day has gone by when the general principles of any science need be considered a mystery, and I submit that any man of intelligence, without knowing all those details which are the particular business of officers trained to apply them, may nevertheless form valuable opinions on the general principles of coast defence, and may, with care and observation, be able to arrive at sound conclusions regarding them.

The security of a country like this does not depend so much on fortresses as on the efforts that can be made by a contented, brave, and patriotic people. If it is known by those who would invade us that we have not only brave hearts, skilled hands, and powerful guns, but a system of applying our resources that is capable of making any coast position formidable to war-ships, that knowledge will have its effect.

In war-time a good general disposes of his forces in that manner which will be most embarrassing and most formidable to the enemy. In time of peace we might arrange and prepare our coast defences on similar principles.

The improved artillery applied in earthworks made thoroughly efficient on the new system, together with the facilities which the existing network of railways slightly extended would supply, should be made to go some way in meeting the corresponding advantages, that have been conferred on the power of attack by steam navies and iron-clad war-ships.

If my labours have in any degree the effect of diverting the great resources of this country from a more expensive to a cheaper and more efficient system of coast defence both in the colonies and at home, and if thereby the security from outrage and disaster is increased, the consciousness of having helped to do so will itself be to me a reward for the delays, anxieties, and trouble that it has cost me to bring this matter forward.

[A. M.]

WEEKLY EVENING MEETING,

Friday, May 14, 1869.

Sir Henry Holland, Bart. M.D. D.C.L. F.R.S. President, in the Chair.

W. H. Perkin, Esq. F.R.S.

On the Newest Colouring Matters.

Seven years ago the subject of the Coal-tar Colours was first brought under your notice in this Institution by the illustrious Dr. Hofmann, whose brilliant lecture on Mauve and Magenta must still be fresh in your memories. Since then your attention has been directed to a further development of the same subject by Mr. Frederick Field; and it may appear strange that I should invite you after so short an interval to listen to another discourse on the chemistry of the artificial colouring matters. I need not remind you, however, that in this age of progress the most remarkable advances are those which are assisted by the hand of science; and the bare statement that the progressiveness of scientific work is strikingly exemplified by the history of these colouring matters will, I trust, be deemed a sufficient justification of my review of the subject.

The first of the coal-tar colours, the "Mauve," came before the world nearly thirteen years ago; the "Magenta" appeared about two years later; and each succeeding year has seen additions to this remarkable class of products. Indeed so rapidly have these colours multiplied, that I can only notice the chief points of interest in their chemical history.

I may remind you that coal-tar is produced in the manufacture of illuminating gas by the destructive distillation of coal, and that it consists of a host of products, from a few of which our coal-tar colours are derived. Many of these colours are derivatives of *aniline*, one of the organic bases found in coal-tar. The separation of this base from the other coal-tar products is attended with much difficulty, and for this reason the whole of the aniline employed in the manufacture of colouring matters is prepared from the more volatile product *benzol*. By the action of nitric acid the benzol is converted into a dense yellow oil called *nitro-benzol*, and by the action of nascent hydrogen this new compound is transformed into aniline. The benzol of commerce, however, is invariably a mixture of benzol and *toluol*, and the product obtained from it is a mixture of the allied bases aniline and *toluidine*, both of which are required for the formation of the artificial colouring matters. The commercial aniline thus constituted is an oily liquid usually of a pale sherry colour. It dissolves readily in dilute acids, forming nearly colourless solutions, which yield when treated with bichromate of potassium a sooty black powder. This unpromising product contains that beautiful colouring matter the Mauve, which may be extracted by dilute spirit of wine.

As the Mauve is the oldest of the coal-tar colours, and a child of my own, I feel constrained to say something about its peculiarities. I may remind you in the first place that it is characterized by great stability, a quality not shared by all the coal-tar purples. The pure colouring matter contains a powerful organic base now called *mauveine*. This base dissolves in spirit of wine, forming a solution of a dingy violet colour, and the development of the beautiful mauve or purple is the result of the union of the base with an acid. The colouring matter generally used is the acetate of mauveine, a salt which may be obtained in fine crystals having a green metallic lustre. So great is the affinity of mauveine for even the most feeble acids, that the dull colour of its alcoholic solution rapidly changes to purple under the influence of the carbonic acid of the breath. Mauveine is decolorized by nascent hydrogen, but its original colour is instantaneously restored by the oxygen of the air. Ordinary indigo is similarly affected, but it does not resume its colour as rapidly as mauveine.

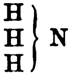
As early as 1836, Runge obtained from the products of the destructive distillation of coal a basic oil, which exhibited a remarkable blue coloration when treated with chloride of lime. This oil, which he named *kyanol*, or blue oil, was afterwards found to be aniline; and since the discovery of the mauve, the blue coloration produced by chloride of lime has often been ascribed to the formation of that colouring matter. I have lately succeeded in obtaining the product of Runge's experiment in the solid condition, and I find that it dissolves in alcohol, forming a solution of a nearly pure blue colour, which is changed to a brownish-red by the action of caustic alkali. It, therefore, differs essentially from the mauve, an alcoholic solution of which when treated with caustic alkali passes from purple to violet. The blue product, which I propose to call "Runge's Blue," undergoes a very remarkable change when subjected to the action of heat. It is rapidly converted into a purple colouring matter, which is found to be the true mauve. Indeed Runge's blue is so prone to change into the more stable mauve that its composition cannot be satisfactorily determined.

The beautiful crystalline colouring matter Magenta, and its base *rosaniline*, have been brought under your notice, with ample illustrations of their formation and properties, on previous occasions. I shall not stop, therefore, to recount the chemical history of these important compounds, but will proceed to develop the principles which elucidate the structure of the artificial colouring matters. I may remark, however, that rosaniline now occupies much the same position with respect to the coal-tar colours as aniline occupied formerly. It is now the principal raw material in the manufacture of these colouring matters.

Nearly all the coal-tar colours contain organic bases which may be regarded as representatives of *ammonia*. It is scarcely necessary to remind you that this typical body ammonia is composed of three atoms of hydrogen and one atom of nitrogen, its symbolic formula being H_3N. Chemists have found that certain groups of carbon and hydrogen atoms can take the place of single atoms of hydrogen in chemical compounds. These groups are called radicals, and many of them are distinguished by special names; thus we have the radicals

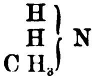
CH_3 Methyl C_6H_5 Phenyl $C_{10}H_7$ Napthyl

each of which has the combining value of an atom of hydrogen. Now we can take ammonia and by chemical means insert any one of these radicals in the place of hydrogen, so as to produce a complex form of ammonia. With the radicals I have mentioned we may get this series of compounds:



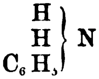
$$\left.\begin{matrix}H\\H\\H\end{matrix}\right\}N$$

Ammonia.



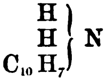
$$\left.\begin{matrix}H\\H\\CH_3\end{matrix}\right\}N$$

Methylamine.



$$\left.\begin{matrix}H\\H\\C_6H_5\end{matrix}\right\}N$$

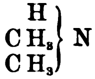
Phenylamine.



$$\left.\begin{matrix}H\\H\\C_{10}H_7\end{matrix}\right\}N$$

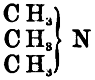
Naphthylamine.

Phenylamine is merely the systematic name for aniline; so you see we actually start with an ammonia in the preparation of our colouring matters. The process of displacing hydrogen may be continued until we get ammonias in which compound radicals are substituted for two-thirds or even the whole of the hydrogen. We have thus obtained the compounds—



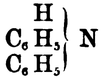
$$\left.\begin{matrix}H\\CH_3\\CH_3\end{matrix}\right\}N$$

Dimethylamine.



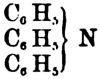
$$\left.\begin{matrix}CH_3\\CH_3\\CH_3\end{matrix}\right\}N$$

Trimethylamine.



$$\left.\begin{matrix}H\\C_6H_5\\C_6H_5\end{matrix}\right\}N$$

Diphenylamine.



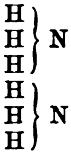
$$\left.\begin{matrix}C_6H_5\\C_6H_5\\C_6H_5\end{matrix}\right\}N$$

Triphenylamine.

In addition to those radicals, like methyl and phenyl, which displace single atoms of hydrogen, we have other groups of carbon and hydrogen atoms, called bivalent radicals, each of which can displace two atoms of hydrogen. The formulæ and names of two radicals of this class are here given, dashes being used to indicate the combining value:

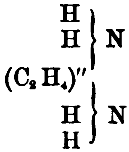
$(C_2H_4)''$ Ethylene $(C_6H_4)''$ Phenylene

Now when we attempt to displace the hydrogen of ammonia by one of these radicals, we obtain a very remarkable result. Instead of taking the place of two atoms of hydrogen in a single molecule of ammonia, the radical acts upon two molecules, displacing a single atom of each and binding together the residues, so as to produce a double ammonia or *diamine*, thus:



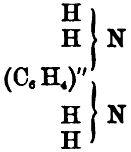
$$\begin{array}{r}\left.\begin{array}{r}H\\H\\H\end{array}\right\}N\\\left.\begin{array}{r}H\\H\\H\end{array}\right\}N\end{array}$$

Ammonia (2 mols.)



$$\begin{array}{rl}H & \\ H & \}\,N\\ (C_2H_4)'' & \\ H & \}\,N\\ H & \end{array}$$

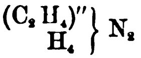
Ethylene-diamine.



$$\begin{array}{rl}H & \\ H & \}\,N\\ (C_6H_4)'' & \\ H & \}\,N\\ H & \end{array}$$

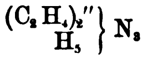
Phenylene-diamine.

By means of these bivalent radicals we may thus bind three molecules of ammonia together and obtain a triple ammonia or *triamine;* and we may even bind four molecules together and produce a quadruple ammonia or *tetramine.* The formulæ of these complex ammonias may be written in a comparatively simple manner, thus:



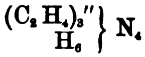
$$\left.\begin{array}{r}(C_2H_4)''\\H_4\end{array}\right\}N_2$$

Ethylene-diamine.



$$\left.\begin{array}{r}(C_2H_4)_2''\\H_5\end{array}\right\}N_3$$

Ethylene-triamine.

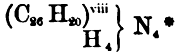


$$\left.\begin{array}{r}(C_2H_4)_3''\\H_6\end{array}\right\}N_4$$

Ethylene-tetramine.

Most of the bases of our coal-tar colours are compounds of this class.

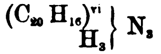
Mauveine, the base of the mauve, appears to be a tetramine in which the group $C_{26}H_{20}$ takes the place of eight atoms of hydrogen. This group really consists of several radicals, but it may be conveniently represented as an integral part of the formula, thus:



$$\left.\begin{array}{r}(C_{26}H_{20})^{viii}\\H_4\end{array}\right\}N_4{}^*$$

Mauveine.

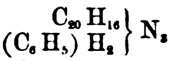
Rosaniline, the base of the magenta, is undoubtedly a triamine, in which the place of six atoms of hydrogen is filled by the group $C_{20}H_{16}$, also consisting of several radicals, which need not be written separately for our present purpose. We accordingly represent the anhydrous base by the formula



$$\left.\begin{array}{r}(C_{20}H_{16})^{vi}\\H_3\end{array}\right\}N_3$$

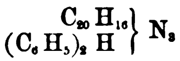
Rosaniline.

If we boil a salt of rosaniline with aniline or phenylamine we find that the radical phenyl displaces the hydrogen that is not combined with carbon, and we obtain successively salts of three new bases which may be thus formulated:



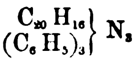
$$\left.\begin{array}{r}C_{20}H_{16}\\(C_6H_5)\,H_2\end{array}\right\}N_3$$

Phenyl-rosaniline.



$$\left.\begin{array}{r}C_{20}H_{16}\\(C_6H_5)_2\,H\end{array}\right\}N_3$$

Diphenyl-rosaniline.



$$\left.\begin{array}{r}C_{20}H_{16}\\(C_6H_5)_3\end{array}\right\}N_3$$

Triphenyl-rosaniline.

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* From recent experiments I am induced to consider the formula of Mauveine as $C_{26}H_{24}N_4$, instead of $C_{27}H_{24}N_4$.

These successive substitutions of phenyl for hydrogen are attended with remarkable alterations of colour. The beautiful red or magenta colour of the rosaniline salt is changed first to violet, then to blue violet, and lastly to a magnificent blue. The salts of triphenyl-rosaniline constitute the important colouring matter which is known commercially as "*Bleu de Lyon*" or "Opal Blue," while the salts of the other two bases form the beautiful product called "*Violet Impériale.*" The remarkable relationship of these colouring matters to rosaniline was elucidated by Dr. Hofmann.

But phenyl is not the only radical that can be substituted for hydrogen in a complex molecule, and Dr. Hofmann has succeeded in producing methyl and ethyl rosanilines analogous to the phenyl derivatives. By heating rosaniline with *iodide of methyl* we displace one hydrogen atom by the group CH_3 and obtain *methyl-rosaniline*, a base which forms salts of a red violet colour. By the further action of the iodide of methyl a second hydrogen atom is displaced, and we get *dimethyl-rosaniline*, the salts of which exhibit a bluer shade of violet. A third substitution of methyl for hydrogen gives us *trimethyl-rosaniline*, which forms salts of a very blue violet colour. The methyl and ethyl derivatives of rosaniline are the bases of the magnificent colouring matters known as the "Hofmann Violets," and extensively used by the dyer and the printer.

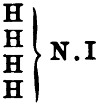
I may mention at this point that up to the present time I have only been able to effect a single substitution of methyl for hydrogen in mauveine. The most remarkable fact connected with this substitution is, that the methyl influences the colour of the product in the opposite manner to that observed in the case of rosaniline; instead of making the colour bluer it causes it to become redder.

Our theoretical considerations must now be interrupted, as I have to call your attention to a few colouring matters upon which chemistry has thrown but little light as yet.

Oil of turpentine when treated with bromine and water yields a very peculiar viscid body having the composition $C_{10}H_{15}Br_3$. I found that when this product was heated with a solution of rosaniline in methylated spirit, purple and violet colouring matters of great beauty were produced. These colours are now very extensively used by the dyer and printer, being commercially known as the "Britannia Violets." They appear to be amorphous and are easily fusible, consequently they are not very promising subjects for chemical investigation.

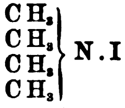
The next colouring matter that comes under our notice is another derivative of rosaniline. When experimenting with rosaniline, M. Lauth found that a solution of this base in concentrated sulphuric acid reacted with the colourless and volatile liquid called *aldehyd* to form a beautiful blue colouring matter. Unfortunately this product was characterized by great instability, a quality not generally desirable in a colour. A dyer named Chirpin endeavoured to turn this blue to practical account, but all his attempts to render it permanent were fruitless. He happened, however, to mention his difficulty to a friendly photographer, who having unbounded faith in the powers of the chemicals employed in his own art, confidently recommended hyposulphite of sodium as a fixing agent. Though the connection between the fixing of a dye and the fixing of a photograph was not sufficiently obvious to inspire hope, the dyer resolved to test the efficacy of his friend's hyposulphite. Much to his astonishment he found that the salt turned his useless blue into a splendid green, which fortunately proved to be a fast colour. This is the history of the beautiful colouring matter known as the "Aldehyd Green," or "Night Green," the latter name having been applied to it on account of its brilliancy under artificial light. Though its chemical nature is not perfectly understood, there can be no doubt that it is the salt of a colourless organic base capable of decomposing ammonia salts.

We must now resume the consideration of our compound ammonias. I have told you that all the hydrogen in ammonia may be displaced by compound radicals, and have referred to trimethylamine as a product of this complete displacement. These complex forms of ammonia were studied by Wurtz and Hofmann, but more particularly by the latter, whose magnificent researches in connection with these bodies have secured him an exalted position among modern chemists. Having succeeded in displacing the whole of the hydrogen in ammonia, Dr. Hofmann naturally thought that the substitution process could be carried no further; but disregarding preconceived ideas, he submitted his altered ammonias to the action of the iodides of methyl and ethyl. He found to his astonishment, that a compound ammonia, like trimethylamine, would unite with the iodide to produce a splendid crystalline body. These new products were found to be comparable to the *iodide of ammonium*. The methyl compound for instance may be viewed as iodide of ammonium modified by the substitution of methyl for hydrogen. We may thus indicate the relationship of the two bodies:



$$\left.\begin{matrix} H \\ H \\ H \\ H \end{matrix}\right\} N.I$$

Iodide of ammonium.



$$\left.\begin{matrix} CH_3 \\ CH_3 \\ CH_3 \\ CH_3 \end{matrix}\right\} N.I$$

Iodide of tetramethyl-ammonium.

When strongly heated the iodide of tetramethyl-ammonium splits up into the very products from which it was prepared, namely, trimethylamine and iodide of methyl.

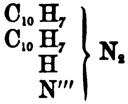
We have seen that methyl can be introduced into rosaniline by three separate displacements of hydrogen. An interesting question now arises: Will trimethyl-rosaniline combine, like trimethylamine, with iodide of methyl? It will, and the product is a beautiful colouring matter. Recollecting that the development of a blue tint in the Hofmann violets indicated successive substitutions of methyl for hydrogen, we might reasonably conclude that this new methyl compound would be still bluer than a salt of trimethyl-rosaniline. The excess of methyl, however, gives us a colour which lies beyond pure blue, our complex product being the important blue-green colouring matter known as the "Iodine Green." It is now extensively used for dyeing cotton and silk, and owing to its strong blue tinge it gives a great variety of shades when employed in combination with yellow. This green product when strongly heated splits up into the compounds from which it was formed, namely, trimethyl-rosaniline and iodide of methyl.

There is still another aniline green which is now very extensively employed for calico printing. It is a feeble organic base producing crystalline salts, but its chemical relations have not been fully studied. It is known commercially as "Perkin's Green."

I must now call your attention to a red colouring matter, which, like the mauve, is a product of the oxidation of aniline. Its shade of colour approximates to that of "safflower extract," the colouring matter of the *Carthamus tinctorius*, and is much redder than magenta. I first obtained this product several years ago, but only in small quantities. Improved methods for preparing it have been proposed, and it is already employed to a limited extent in the arts. It is commonly called "Aniline Pink." It is also known as "Safranine," but this name properly belongs to the colouring matter of saffron. I am at present engaged in an investigation of the chemical nature of this body, and from the results already obtained, I conclude that it contains an organic base which gives crimson solutions with acids and forms crystallizable salts. This base appears to be composed of $C_{18}H_{18}N_4$, and its reactions show that it is closely related to mauveine.

Last year, M. Clavel, of Basle, patented a process for producing a new colouring matter from *naphthalin*, or rather from *naphthylamine*, or, according to his own statement, from "an isomer of naphthylamine." This process consists in heating together equal quantities of the "isomeric naphthylamine," acetic acid, and nitrite of potassium to about 120° C., and then adding naphthylamine, the temperature being maintained at 120° C. until the desired colour is produced. When purified, the product is a beautiful crimson colour, specially adapted for silk dyeing.

Several years ago, Mr. Church and myself obtained by the action of nitrites on salts of naphthylamine a beautiful crystalline compound, called *azo-dinaphthyl-diamine*, consisting of two molecules of naphthylamine linked together by an atom of trivalent nitrogen occupying the place of three atoms of hydrogen. Its formula may be written thus:



$$\left.\begin{matrix} C_{10}H_7 \\ C_{10}H_7 \\ H \\ N''' \end{matrix}\right\} N_2$$

Azo-dinaphthyl-diamine.

This substance is an organic base, giving a solution of an orange-yellow colour, and producing with acids various salts, some of which are violet. Now I find that M. Clavel's new colouring matter can readily be produced from this azo-dinaphthyl-diamine by a process which appears to elucidate its chemical nature without compelling us to assume the existence of an isomeric naphthylamine. On heating azo-dinaphthyl-diamine with *ordinary* naphthylamine and an acid, we obtain the new product by a reaction which seems to be analogous to that which occurs when rosaniline is boiled with aniline. In the latter case we know that phenyl is substituted for hydrogen, and there can be little doubt that the reaction we are now considering consists in the substitution of naphthyl for hydrogen, the product being *azo-tri-naphthyl-diamine*, which may be thus formulated:

$$\left.\begin{array}{r} C_{10}H_7 \\ C_{10}H_7 \\ C_{10}H_7 \\ N''' \end{array}\right\} N_2$$

Azo-trinaphthyl-diamine.

This new dye is known in commerce as "Magdala." On boiling our azo-dinaphthyl-diamine with aniline we obtain another red colouring matter which is probably *azo-phenyl-dinaphthyl-diamine.*

The aniline pink and the magdala are characterized by a remarkable fluorescence in the green rays of the spectrum.

The last result obtained in the production of colouring matters indicates a perfectly new line of research. Nearly all the coal-tar colours contain organic bases which may be viewed as representatives of ammonia, but the discovery referred to directs our attention to products of a different class—products which do not contain nitrogen. This discovery derives additional importance from the fact that it is a precedent for the artificial production of the natural colouring matters. The product recently obtained is in fact the true colouring matter of the madder root, namely "Alizarine."

Graebe and Liebermann found that nascent hydrogen converted natural alizarine into a hydro-carbon, which proved to be *anthracene*, one of the coal-tar products. This result naturally suggested an attempt to produce alizarine from anthracene by working backwards, and the well-directed labours of the two chemists have been crowned with success. Several years since, Dr. Anderson obtained an oxygenated derivative of anthracene, having the composition $C_{14}H_8O_2$, and now called *anthrakinone.* On treating this with bromine, Graebe and Liebermann obtained *bibromo-anthrakinone*, having the formula $C_{14}(H_6Br_2)O_2$, and this compound, when digested with potash, gave them alizarine, which has the formula $C_{14}H_8O_4$. I may remind you that alizarine is one of our most important colouring matters, being extensively used for the production of Turkey red, and our lilac, pink, and chocolate prints.

I have given you a brief history of the principal artificial colouring matters, and have endeavoured to elucidate the constitution or structure of those which have been studied by chemists. Though the beautiful relations of rosaniline and its derivatives are now very evident, it must be admitted that for want of sufficient data we are compelled to regard many of our colouring matters as isolated products. We may, however, detect some slight connecting threads of chemical relationship between all the coal-tar colours. They may be said to have a common origin, as they can be generally described as phenylic derivatives. The hydro-carbons from which most of them are produced contain the radical phenyl; thus benzol is the hydride of phenyl, while toluol may be regarded as a compound of phenyl and methyl. We do not find, however, that these and other substances containing phenyl are colouring matters, but the development of colour seems to attend the removal of hydrogen from phenyl. Thus in rosaniline and mauveine we appear to have the radical *phenylene* $C_6 H_4$, which is simply phenyl deprived of an atom of hydrogen. I do not assert that this particular radical is common to all the colour-producing bases, but I have come to the conclusion that each of these bases contains a phenyl residue, that is to say, phenyl more or less dehydrogenated. The natural colouring matters have been only partially studied, but some of them undoubtedly contain phenyl residues. We thus trace a connection between the colours produced artificially and those formed in the laboratory of nature.

When I commenced my lecture, I referred to the rapid advances of applied science. The history of the production of artificial colouring matters is a striking illustration of scientific progress. The new industry which emerged from the laboratory only thirteen years ago has attained such vast proportions, that its present annual value is computed to be more than a million and a quarter. Now that we are beginning to produce the natural colouring matters from coal-tar, it is impossible to form any conception of the ultimate magnitude of this important industry.

[W. H. P.]

WEEKLY EVENING MEETING,

Friday, May 21, 1869.

John Peter Gassiot, Esq. F.R.S. Vice-President, in the Chair.

H. C. Fleeming Jenkin, Esq. F.R.S.

PROFESSOR OF ENGINEERING IN THE UNIVERSITY OF EDINBURGH,

On the Submersion and Recovery of Submarine Cables.

The speaker began by stating that his object was to explain the principles on which engineers had acted in laying and recovering submarine cables, rather than to exhibit the details of the machinery employed.

The general construction of electrical cables was first described and specimens were shown; especial attention being drawn to the deep-sea French Atlantic cable, consisting of the following parts:—A copper conductor, gutta-percha insulator, and jute serving, surrounded by ten wires of homogeneous iron, each served with five Manilla yarns saturated with tar.

TABLE I.—CONSTRUCTION OF FRENCH ATLANTIC CABLE—*Deep-Sea Section.*

	Lbs. weight per Knot.	Diameter in Inches.	Breaking Strain. Lbs.
Copper	400	·168	644
Gutta-percha	400	·463	—
Serving	234	·669	—
Homo. wires (10)	1589	·100	950
Manilla strands (50)	1091	—	550
Each served wire	268	·245	1,550
Cable	3701	1·134	16,530
Weight of cable in air	1·652 tons per knot.		
,, in water	0·753 ,,		
Strength in tons	7⅜ tons.		

Table No. 1 gives the dimensions, weights, and strengths of each of the component parts. The wire served with hemp will bear a greater weight than the sum of the weights borne separately by the wire and the strands; and, again, the ten served wires, when formed into a rope, bear a greater weight than the sum of the weights which each will bear. Moreover, while the homogeneous iron elongates less than one per cent. before breaking and the hemp elongates only 0·75 per cent., the two combined stretch three per cent. This paradoxical result is due to want of absolute uniformity in the strength of each part; when separate, each breaks at the weakest point; when combined, the weakest points seldom coincide; hence the strength of the combination is the sum of the mean strengths of the parts, necessarily greater than the sum of the minimum strengths.

The so-called spiral or helical form does not really render the cable elastic or liable to stretch, nor does it compress the core inside the sheathing, as was shown by an experiment where the core was actually withdrawn without causing the collapse of the sheathing.

The manner of coiling the cable on board ship was explained by diagrams and models; it being shown that in order to avoid putting a twist into the rope when taking it out of the hold, it was necessary to put a twist in when coiling it away. Bad coiling produces kinks or loops drawn tight, which are avoided by a cone filling the eye of the coil, and by rings or equivalent arrangements preventing the bight as drawn out of the hold from lashing out under the influence of centrifugal force.

The following table gives the dimensions and contents of the Great Eastern tanks as arranged for the Atlantic expedition. These tanks keep the cable under water on board ship to facilitate the electrical tests. They carry a weight of 5000 tons, in a bulk of 180,000 cubic feet, the tanks not being filled quite to the top.

TABLE II.

	Diameter.	Depth.	Cable knots.
Fore tank	51 ft. 6 in. ..	20 ft. 6 in. ..	728
Main tank	75 ft. 0 in. ..	16 ft. 6 in. ..	1100
After tank	58 ft. 0 in. ..	26 ft. 6 in. ..	912

Notwithstanding their enormous weight and size, these tanks occupy a very insignificant proportion of the whole bulk of the Great Eastern.

Mr. C. W. Siemens has for light cables employed a sort of reel or drum on a turn-table with partial success, instead of the fixed tank and coil.

From the tank the cable when paid out passes over a pulley and along a trough to the bréak drum, the object of which is to restrain the free exit of the cable to such an extent as is desired.

The cable is laid hold of by being passed several times round a drum, as a rope making fast a vessel may be seen to be passed round a bollard; the friction allows a slight strain at one end to prevent a very heavy pull at the other end from causing the rope to slip round the drum. The slight pull at what may be called the light end of the rope is given by a series of jockey pulleys which play the part of the hand when the rope is allowed to slip round a bollard, but in paying out a cable the rope does not slip round the drum; the drum itself turns round restrained by a friction band or belt.

It is essential that this restraining friction should be constant; a result attained by the Appold Break, which was explained by models and diagrams. In this arrangement both ends of the break strap are attached to one lever in such a manner that when the drum begins to turn it tends to lift the lever and weight hanging to it, and as the lever is lifted it slackens the break strap until the difference of tension on the two ends of the strap is equal to the weight hanging on the lever. When this is the case, the lever is no longer lifted, but remains stationary with the strap, allowing the drum to turn, restrained by a constant friction equal to the weight on the lever. If the coefficient of friction increases, the lever will be a little more lifted and the strap slackened; if the coefficient of friction diminishes, the lever and weight will fall, tightening the strap; but in any case the retarding force will be simply equal to the weight.

From the break drum the rope dips under a weighted pulley, which rides as it were suspended on a V of taut cable; if the strain increases, the rope straightens, and raises the pulley; if the strain diminishes, the weight and pulley fall; thus the height of the pulley indicates the strain. This instrument is called the dynamometer. Lastly, the rope passes over a pulley into the sea.

Having shown how the cable was treated, the speaker proceeded to show how the strains to be expected could be calculated. A cable paid out in air hangs in a catenarian curve, but in water lies in a straight line, and the strains in the two cases are wholly different. In air the rope meets with no sensible obstacle to its motion either longitudinally or in a direction perpendicular to its own length; in water, on the contrary, each foot of a cable meets with an opposition to its motion perpendicular to its length, which we may call q, and for the Atlantic cable

$$q = 0 \cdot 154\ v^2,$$

where v is the velocity of the cable normally to its own length in feet per second. Thus, as the cable weighs 0·2575 lb. per foot, it cannot sink faster than the speed given by the equation

$$0 \cdot 2575 = 0 \cdot 154\ v_1^2,$$

from which v_1, the settling velocity, is found to be 1·294 foot per second, or 0·765 knot per hour.

The result of this resistance to displacement is that the cable lies in a straight line, not in a catenary curve, supported as it were by an inclined plane of water constantly yielding at the velocity v_1.

The inclination of the straight line depends on the velocity of the ship and on v_1 not being at all affected by the tension of the rope.

The angle ϕ at which the cable will lie may be calculated as follows. Let P be the resistance of the water to displacement by each foot of the cable of the weight ω when lying at the angle ϕ,

$$P = \omega \cos. \phi;$$

let v_{11} be the velocity at which the cable moves perpendicularly to itself,

$$v_{11} = v \sin. \phi,$$

where v is the velocity of the ship.

$$\text{Also } P = \omega \frac{v_{11}^2}{v_1^2};$$

$$\text{hence } \cos. \phi = \frac{v_{11}^2}{v_1^2} = \frac{v^2 \sin.^2 \phi}{v_1^2},$$

$$\text{and } v_1 = \frac{v \sin. \phi}{\sqrt{\cos. \phi}}; \quad \ldots\ldots\ldots\ldots\ 1^\circ$$

and assuming that the resistance is proportional to the square of the velocity, we have $\omega = q\, v_1^2$, and hence $\frac{\omega}{q} = \frac{v^2 \sin.^2 \phi}{\cos. \phi}$, or

$$\frac{\omega}{qv^2} = \frac{\cos. \phi}{\sin.^2 \phi}, \text{ from which we have}$$

$$\cos. \phi = \frac{\sqrt{\omega^2 + 4\, m^2} - \omega}{2\, m} \quad \ldots\ldots\ldots\ 2^\circ$$

where $m = q\, v^2$.

From this formula, as indeed from common sense, it appears that the greater the value of q and of v, the smaller the inclination with the horizon. The rough Atlantic cable, when the ship was going at the speed of six knots per hour, lay at an angle of 6¾°, so that the inclined plane was seventeen miles long, and each foot of the cable took nearly three hours to reach the bottom.

The strain T at the top of the inclined plane, if there were no friction preventing the rope from slipping back along the plane, would be equal to the weight of a piece of cable hanging plumb from the surface of the water to the bottom, or

$$T = \omega x,$$

where ω is the weight per foot run of the cable and x is the depth in feet.

But there is a sensible friction which helps to relieve the strain precisely as when a chain is lying on a solid inclined plane; calling m_1 the coefficient of friction in lbs. per foot length of cable at the velocity v in feet per second, and assuming that $m_1 = q_1 v^2$, the experiment of the Atlantic cable showed that $q_1^2 = \cdot 00504$ (this is equivalent to 0·81 cwt. per knot of cable when slack is paid out at the rate of one knot per hour). The result is, that when slack is paid out, say at the rate of 1 knot per hour, and when $\phi = 6 \cdot 45°$, the strain is diminished by one-half, and if slack were paid out at the rate of 1·4 knot per hour, or 23½ per cent., this particular cable would require no retarding force whatever.

The strain T_1, when the velocity of the cable is v_{111}, can be found from the following formula:

$$T_1 = \omega x - m_1 \frac{\left(\frac{v_{111}}{v} - \cos. \phi\right)^2}{\sin. \phi} x \ldots\ldots\ldots \quad 3°$$

Cables of light specific gravity have a small settling velocity and lie at great length in the water, and if they are also rough, the coefficient q_1 may easily be so great as to relieve the break of most of the strain which would be necessary to lay a cable of equal weight but small bulk and smoother surface, with the same amount of slack. If no slack were laid there would be little difference between the tension required for cables of different construction but of equal weights in water. When much slack is laid, all cables will be considerably less strained than if laid without slack; and finally, the faster the ship goes the less slack is required to produce any given amount of relief.

The correctness of the above theory has been amply proved in practice. If in seas 2 miles deep the cable hung in a catenary 12¾ miles long, the weight to be carried would be 8½ tons, and the strain on the cable 29 tons; while if the cable hung in a catenary the inclination of which to the horizon at the stern was 9° 30′, the length would be 24 miles, the weight 17 tons, and the strain 102 tons instead of about 14 cwt.—the strain actually observed for the Atlantic cable when being paid out at 7 knots per hour while the ship was going at 6 knots per hour. The rise and fall of the ship, even in heavy weather, very slightly affects the strain while paying out, on account of the slight inclination of the cable to the horizon.

The margin of strength in deep-sea cables of the Atlantic type is even greater than is given in most Engineering works, since the cable will bear tenfold the strain which is found necessary in laying.

The process of grappling was next described, and the operation illustrated by dragging a miniature grapnel over the floor, so as to hook a chain lying there. When the cable is hooked, the strains on the grapnel rope are simply the weights of the bight lifted, and the length of this bight depends on the slack. Thus with 14 per cent. of slack the length of the cable lifted will be 4·89 times the depth to which it is raised. Thus in 2 miles of water about 9·8 miles of cable will be lifted, the weight on the grapnel will be 6·86 tons, but the strain on the cable will be only one component of this weight resolved in the direction of the tangent to the curve at the grapnel; this strain will be 5·5 tons. Thus it is clear that in calm weather, with 14 per cent. slack, the cable can be lifted from a depth of two miles. This was actually done upon one occasion; but owing to pitching of the ship the cable parted, and was successfully recovered by the obvious device of grappling the cable in two points about 2½ knots apart, and breaking the cable at the point furthest from land; the loose end then hung down over the other grapnel, and it is obvious that by this plan the strain on any cable in any depth can be limited to the simple weight of a length of cable hanging from the surface to the bottom. The Atlantic cables will bear five times the strain due in this manner to 2 miles of depth, and for this operation the margin of strength is also ample. The cable is hauled in by machinery very similar to that adopted for paying out; the drum being simply turned in the opposite direction by a steam engine, if only a small length is to be picked up. If many miles are required, the cable is transferred to the bow, and hauled up by a double drum to avoid the fleeting necessary on a single drum. The friction of the water during this operation adds to the strain: thus with the value of q_1 previously found, at 1 mile per hour the friction per mile would be 0·81 cwt., adding in a depth of 2 miles 1·61 cwt. to the strain due to the simple weight; besides this there is some resistance due to the displacement of the water by the bight of the rope at the bottom, and some extra weight due to the fact that the cable hangs in a catenary, not in a straight line. The length of this catenary depends on the rate at which the cable is hauled through the water; but even after allowing for all these things, the strength of the cable is from three to four times greater than the strain which in fair weather need come on the cable when being picked up from a depth of 2 miles; a margin of strength not unfrequently adopted even in permanent Engineering works.

It was by calculations like these that before the 1865 cable had been recovered in 1866, the speaker was able to write in 'The Times' of August, 1865, "If the cable retain its strength, as it probably will, it can certainly be raised;" and now that experience has confirmed theory, engineers are justified in looking forward with great confidence to the continued prosperity and extension of deep-sea telegraphy.

The following tables give some further information as to the French Atlantic cable about to be laid, which will cover 50 acres of ground, being a narrow strip, 3564 knots long, and a little more than an inch wide.

TABLE III.—LENGTHS AND WEIGHTS OF MATERIALS USED IN FRENCH ATLANTIC CABLE.

	Knots.	Tons.
Copper wire	24,918	533
Gutta-percha	3,564	549
Jute serving	—	500
Homo. wire	27,222	1872
Iron wire	9,941	2855
Total iron and homo. wires	37,163	4727
Manilla strands	136,110	1286
Clark's compound	881	652
Deep-sea cable	2,643	4366
Shallow-water cable	921	3881
Total cable	3,564	8247

TABLE IV.—LENGTHS OF EXISTING CABLES.

	Knots.
Atlantic (two)	3748
Malta, Alexandria (two)	2254
Persian Gulf	1308
Home seas	1277
Miscellaneous (approximate)	1350
Total	9937

[H. C. F. J.]

WEEKLY EVENING MEETING,

Friday, May 28, 1869.

SIR HENRY HOLLAND, Bart. M.D. D.C.L. F.R.S. President, in the Chair.

J. NORMAN LOCKYER, F.R.S.

On Recent Discoveries in Solar Physics made by means of the Spectroscope.

IN the year 1865 two very important memoirs dealing with all the telescopic and photographic observations accumulated up to that time on the subject of solar physics were given to the world. One of them was privately printed in this country, the other appeared in the 'Compte Rendu' of the Paris Academy of Sciences.

I shall not detain you with a lengthened notice of these remarkable papers. I shall merely refer to the explanation given in both of them of the reason that a sun-spot appears dark—the very keystone of any hypothesis dealing with the physical constitution of the sun.

English science, represented by Messrs. De la Rue, Stewart, and Loewy, said that a spot is dark because the solar light is absorbed by a cool, non-luminous, absorbing atmosphere, pouring down there on to the visible surface of the sun, in other words, on to the photosphere.

French science, represented by M. Faye, said that a spot is dark because it is a hole in the photosphere, and the feebly luminous and therefore radiating interior gases of the sun are there alone visible.

Now most of you will see in a moment that here was a clear issue, which probably the spectroscope, and possibly nothing else, could solve; for the spectroscope is an instrument whose special *métier* it is to deal with radiation and absorption. It tells us that the light radiated from different bodies gives us spectra of different kinds, according to the nature of the radiating body,—continuous spectra without bright lines in the case of solids and liquids, and bright lines, with or without continuous spectra, in the case of gases and vapours. It tells us also that absorption dims the spectrum throughout its length when the absorption is *general*, and dims it here and there only when the absorption is *selective*, the well-known Fraunhofer lines being, as you will readily see, an instance of the latter kind. So that we have general and selective radiation, and general and selective absorption.

Now then, with regard to the English theory, if there were more absorption in a spot than elsewhere, we might expect evidences of absorption; that is, the whole solar spectrum would be visible in the spectrum of a spot, but it would be dimmed, either throughout the length of the spectrum or in places only.

With regard to the French theory, radiating only gaseous matter to deal with, we should, according to the then generally received idea, get bright lines only in the spot spectrum.

Here then was a tempting opportunity, and one which I considered myself free to use; for, although the spectroscope had then been employed—and you all know how nobly employed—for four years in culling secrets from stars and nebulæ, there was not, so far as I know, either published or unpublished observation on the sun, the nearest star to us. The field was therefore open for me, and I was not entering into another man's labour, when, on the 4th of March, 1866, I attached a small spectroscope to my telescope, in order to put the rival theories to a test, and thus bring another power to bear on a question which had remained a puzzle since it was first started by Galileo some two-and-half centuries ago.

What I saw I will describe more fully by-and-by. It is sufficient here to mention that it was in favour of the English theory. There *was* abundant evidence of absorption in the spots, and there *was not* any indication of gaseous radiation.

Having then thus spectroscopically broken ground on the sun, a very natural inquiry was how next to employ this extension of a method of research, the discovery of which Newton had called, nearly two hundred years before, "the oddest, if not the most considerable, detection which hath hitherto been made in the operations of nature."

There seemed one question which the spectroscope should now put to the sun above all others, and it was this:—

"Assuming this absorbing atmosphere to encircle the sun, in accordance with the general idea and Kirchhoff's hypothesis, what are those strange red flames seen apparently in it at total eclipses, jutting here and there from beyond the sun's hidden periphery, and here again hanging cloudlike?"

The tremendous atmosphere, which apparently the spectroscope had now proved to be a cool absorbing one, was supposed to be indicated during eclipses by a halo of light called the "Corona," in which corona the red flames are visible. Now, as the red flames are always observed to give out more light than the corona, they were probably hotter than it; and reasoning thus on the matter with my friend Dr. Balfour Stewart one day, we came to the conclusion that they were most probably masses of glowing gas.

Now, this being so, the spectroscope *could* help us, and in this way.

The light from solid or liquid bodies, as you all I am sure know, is scattered broadcast, so to speak, by the prism into a long band of light, called a continuous spectrum, because from one end of it to the other the light is persistent.

The light from gaseous and vaporous bodies, on the contrary, is most brilliant in a few channels; it is *husbanded*, and, instead of being scattered broadcast over a long band, is limited to a few lines in the band—in some cases to a very few lines.

Hence, if we have two bodies, one solid or liquid and the other gaseous or vaporous, which give out exactly equal amounts of light, then the bright lines of the latter will be brighter than those parts of the spectrum of the other to which they correspond in colour or refrangibility.

Again, if the gaseous or vaporous substance gives out but few lines, then, although the light which emanates from it may be much less brilliant than that radiated by a solid or liquid, the light may be so localized, and therefore intensified, in one case, and so spread out, and therefore diluted, in the other, that the bright lines from the feeble light source may in the spectroscope appear much brighter than the corresponding parts of the spectrum of the more lustrous solid body. Now here comes a very important point: supposing the continuous spectrum of a solid or liquid to be mixed with the discontinuous spectrum of a gas, we can, by increasing the number of prisms in a spectroscope, dilute the continuous spectrum of the solid or liquid body very much indeed, and the dispersion will not seemingly reduce the brilliancy of the lines given out by the gas; as a consequence, the more dispersion we employ the brighter relatively will the lines of the gaseous spectrum appear.

The reason why we do not see the prominences every day in our telescopes is that they are put out by the tremendous brightness of our atmosphere near the sun, a brightness due to the fact that the particles in the atmosphere reflect to us the continuous solar spectrum. There is, as it were, a battle between the light proceeding from the prominences and the light reflected by the atmosphere, and, except in eclipses, the victory always remains with the atmosphere.

You will see, however, in a moment, after what I have said, that there was a possibility that if we could bring a spectroscope on the field we might turn the tide of battle altogether, assuming the prominences to be gaseous, as the reflected continuous spectrum might be dispersed almost into invisibility, the brilliancy of the prominence lines scarcely suffering any diminution by the process.

The first attempt was made in 1866, a Herschel-Browning spectroscope being attached to my telescope, and the first and many succeeding attempts failed; there was not dispersion enough to dilute the spectrum of the regions near the sun sufficiently, and as a consequence the tell-tale lines still remained veiled and invisible. Nature's secrets were not to be wrested from her by a *coup de main.*

The year 1868 brought us to the now famous eclipse, to see which scientific men hastened from all civilized Europe to India. To this eclipse and its results I need only refer, as they have already been dwelt on at some length in this theatre; suffice it to say that in the eclipse the spectroscope did its duty, and that the gaseous nature of the prominences was put beyond all question.

But there was a magnificent pendant to the eclipse, to which I must request your special attention. One of the observers, M. Janssen—a spectroscopist second to none—the representative, in that peaceful contest, of the *Académie des Sciences* and of the *Bureau des Longitudes*, was so struck with the brightness of the prominences rendered visible by the eclipse that, as the sun again lit up the scene, and the prominences disappeared, he exclaimed, "*Je reverrai ces lignes là!*"; and, being prevented by clouds from putting his design into execution that same day, he rose next morning long before the sun, and as soon as our great luminary had risen from a bank of vapours, he succeeded in obtaining spectroscopic evidence of the protuberances he had seen surrounding the eclipsed sun the day before. During the eclipse M. Janssen had been uncertain even as to the number of lines he had observed, but he now by this new method at his leisure determined that the prominences were built up of hydrogen, this fact being indicated by the presence of two bright lines corresponding to the dark lines C and F in the ordinary solar spectrum.

Let me show you how this result was accomplished, by throwing an enlarged photograph of my telescope and spectroscope on the screen. We have first the object-glass of the telescope to collect the sun's rays and to form an image of the sun itself on a screen. In this screen is an excessively narrow slit, through which alone light can reach the spectroscope. This entering beam is grasped by another little object-glass and transformed into a cylinder* of light containing rays of all colours, which is now ready for its journey through the prisms. In its passage through them it is torn by each succeeding prism more out of its path, till at last, on emerging, it crosses the path it took on entering, and enters the little telescope you see, thoroughly dismembered but not disorganized.

Instead now of a cylinder of light containing rays of all colours, we have a cylinder of each ray which the little telescope compels to paint an image of the slit. Where rays are wanting, the image of the slit remains unpainted—we get a black line; and when the telescope is directed to the sun, so that the narrow slit is entirely within the image of the sun, we get in the field of view of the little telescope a glorious coloured band with these dark lines crossing it.

Of course it is necessary for our purpose to allow only the edge of the sun to fall on the slit, leaving apparently a large portion of the latter unoccupied. What is seen, therefore, is a very narrow band in the field of view of the little telescope, and a large space nearly dark, as the dispersion of the instrument is so great that the atmospheric light is almost entirely got rid of, for a reason you are already acquainted with.

Mr. Ladd will now show you on the screen what is seen when the slit reaches a prominence. First a line in the red, very obvious and brilliant, next a more delicate line in the yellow, then another in the green, and two others in the violet; all these lines, with the exception of the yellow line, are in the positions occupied by known lines of hydrogen.

As the height of these bright lines must vary with the height of the prominences, and as the lines will only be visible where there is any hydrogen to depict, it is obvious that the form of the prominences may be determined by confining the attention to one line, and slowly sweeping the slit over it.

The first fruits then of this new method of working with an uneclipsed sun was to tell us the actual composition of the prominences, and to enable us to determine their shapes and dimensions.

For the next steps you must permit me to refer more particularly to my own observations.

When I was first able to obtain results in this country similar to those previously obtained by M. Janssen, though unknown to us, my instrument was incomplete; when other adjustments had been added by Mr. Browning, I found that at whatever part of the sun's edge I looked I could not get rid of the newly discovered lines. They were not so long as I had seen them previously, but there they were, not to be extinguished, showing that for some 5000 miles in height all round the sun there was an envelope of which the prominences were but the higher waves. This envelope I named the "Chromosphere," as it is the region in which all the variously coloured effects are seen in total eclipses, and because I considered it of importance to distinguish between its discontinuous spectrum and the continuous one of the photosphere. And now another fact came out. The bright line F took the form of an arrow-head, the dark Fraunhofer line in the ordinary spectrum forming the shaft, the corresponding chromospheric line forming the head; it was broad close to the sun's edge, and tapered off to a fine point, an appearance not observed in the other lines.

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* Cylindrical, that is, in the case of each pencil.

Nature is always full of surprises, and here was a surprise and a magnificent help to further inquiry lurking in this line of hydrogen! MM. Plücker and Hittorf had already recorded that, under certain conditions, the green line of hydrogen widened out; and it at once struck me that the "arrow-head" was nothing but an indication of this widening out as the sun was approached,

I will now, then, for one moment leave the observatory work to say a word on some results recently obtained by Dr. Frankland and myself, in the researches on the radiation and absorption of hydrogen and other gases and vapours, upon which we have for some time been engaged.

First, as to hydrogen, what could laboratory work tell us about the chromosphere and the prominences?

It was obviously of primary importance—

1. To determine the cause to which the widening of the F line was due.

2. To study the hydrogen spectrum very carefully under varying conditions, with a view of detecting whether or not there existed a line in the orange.

We soon came to the conclusion that the principal, if not the only cause of the widening of the F line was *pressure*.

Having determined, then, that the phenomena presented by the F line were phenomena depending upon and indicating varying pressures, we were in a position to determine the atmospheric pressure operating in a prominence, in which the red and green lines are nearly of equal width, and in the chromosphere, through which the green line gradually expands as the sun is approached.

With regard to the higher prominences, we have obtained evidence that the gaseous medium of which they are composed exists in a condition of *excessive tenuity;* and that even at the lower surface of the chromosphere, that is, on the sun itself, in common parlance, the pressure is very far below the pressure of the earth's atmosphere.

Now I need hardly point out to you that the determination of the above-mentioned facts leads us necessarily to several important modifications of the received theory of the physical constitution of our central luminary—the theory which we owe to Kirchhoff, who based it upon his examination of the solar spectrum. According to his hypothesis, the photosphere itself is either solid or liquid, and it is surrounded by an extensive cool and non-luminous atmosphere composed of gases and the vapours of the substances incandescent in the photosphere.

We find, however, instead of this compound cool and non-luminous atmosphere outside the photosphere, one which is in a state of incandescence, is therefore luminous, and which gives us merely, or at all events mainly, the spectrum of hydrogen; and the tenuity of this incandescent atmosphere is such that it is extremely improbable that any considerable atmosphere, such as the corona has been imagined to indicate, exists outside it.

Here already, then, we find the "cool absorbing atmosphere" of the theorists terribly reduced in height, and apparently much more simple in its composition than had been imagined by Kirchhoff and others. Dr. Frankland and myself have shown separately—

1. That a gaseous condition of the photosphere is quite consistent with its continuous spectrum, whether we regard the spectrum of the general surface or of spots. The possibility of this condition has also been suggested by Messrs De la Rue, Stewart, and Loewy.
2. That a sun-spot is a region of greater absorption.
3. That when photospheric matter is injected into the chromosphere, we see bright lines.
4. That there are bright lines in the solar spectrum itself.

All these are facts which indicate that the absorption to which the reversal of the spectrum and the Fraunhofer lines are due takes place in the photosphere itself or extremely near to it, instead of in an extensive outer absorbing atmosphere. And this conclusion is strengthened by the consideration that otherwise the newly discovered bright lines of hydrogen should themselves show traces of absorption on Kirchhoff's theory; but I shall show you presently that, so far from this being the case, they *appear bright actually in the very centre of the disc*, and, moreover, the vapours of sodium, iron, magnesium, and barium are often bright in the chromosphere, showing that they would always be bright there *if the vapours were always present*, as they should be on Kirchhoff's hypothesis; so that we may say that the photosphere *plus* the chromosphere is the real atmosphere of the sun, and that the sun itself is in such a state of fervid heat that the actual outer boundary of its atmosphere, *i.e.* the chromosphere, is in a state of incandescence.

With regard to the line in the orange I have nothing yet to tell. Dr. Frankland and myself are at the present moment working upon it.

I have next to take you a stage lower into the bowels, not of the earth, but of the sun.

As a rule, the chromosphere rests conformably, as geologists would say, on the photosphere, but the atmosphere (as I have just defined it) is tremendously riddled by convection currents; and where these are most powerfully at work the upper layers of the photosphere are injected into the chromosphere. Thus I have observed the lines due to the vapour of sodium, magnesium, barium, and iron in the spectrum of the chromosphere, appearing there as very short and very *thin lines*, generally much thinner than the black lines due to their absorption in the solar spectrum.

These injections are nearly always accompanied by the strangest contortions of the hydrogen lines, of which more presently. Sometimes during their occurrence the chromosphere seems full of lines, those due to the hydrogen towering above the rest.

At the same time we have tremendous changes in the prominences themselves, which I have recently been able to see in all their beauty. I attempted to accomplish this in the first instance by means of an oscillating slit, but hearing that Mr. Huggins had succeeded in doing the same thing by means of absorptive media, using an open slit, it struck me at once that an open slit was quite sufficient, and this I find to be the case. By this method the smallest details of the prominences and of the chromosphere itself are rendered perfectly visible and easy of observation, and for the following reason. As you already know, the hydrogen Fraunhofer lines (like all the others) appear dark because the light which would otherwise paint an image of the slit in the place they occupy is absorbed, but when we have a prominence on the slit, there is light to paint the slit, and as in the case of any one of the hydrogen lines we are working with light of one refrangibility only, on which the prisms have no dispersive power, we may consider the prisms abolished. Further, as we have the prominence image coincident with the slit, we shall see it as we see the slit, and the wider we open the slit the more of the prominence shall we see. We may use either the red, or yellow, or green light of hydrogen for the purpose of thus seeing the shape and details of the prominences; how far the slit may be opened depends upon the purity of the sky at the time. I have been perfectly enchanted with the sight which my spectroscope has revealed to me. The solar and atmospheric spectra being hidden, and the image of the wide slit and the part of the prominence under observation alone being visible, the telescope or slit is moved slowly, and the strange shadow-forms flit past, and are seen as they are seen in eclipses. Here one is reminded, by the fleecy, infinitely-delicate cloud-films, of an English hedge-row with luxuriant elms; here of a densely intertwined tropical forest, the intimately interwoven branches threading in all directions, the prominences generally expanding as they mount upwards, and changing slowly, indeed almost imperceptibly.

It does not at all follow that the largest prominences are those in which the intensest action, or the most rapid change is going on—the action as visible to us being generally confined to the regions just in, or above, the chromosphere; the changes arising from violent uprush or rapid dissipation—the uprush and dissipation representing the birth and death of a prominence. As a rule, the attachment to a chromosphere is narrow and is not often single; higher up, the stems, so to speak, intertwine, and the prominence expands and soars upward until it is lost in delicate filaments, which are carried away in floating masses.

Since last October, up to the time of trying the method of using the open slit, I had obtained evidence of considerable changes in the prominences from day to day. With the open slit it is at once evident that changes on the small scale are continually going on; but it was only on the 14th of March that I observed any change at all comparable in magnitude and rapidity to those already recorded by M. Janssen.

About 9h. 45m. on that day, with the slit lying nearly along the sun's edge instead of across it as usual, I observed a fine dense prominence near the sun's equator, on the eastern limb, with signs of intense action going on. At 10h. 50m., when the action was slackening, I opened the slit and saw at once that the dense appearance had all disappeared, and cloud-like filaments had taken its place. The first sketch, now exhibited, embracing an irregular prominence with a long perfectly straight one, was finished at 11h. 5m., the height of the prominence being 1′ 5″, or about 27,000 miles. I left the Observatory for a few minutes, and on returning, at 11h. 15m. I was astonished to find that the straight part of the prominence had entirely disappeared; not even the slightest rack appeared in its place. Whether it was entirely dissipated, or whether parts of it had been wafted towards the other part, I do not know, although I think the latter explanation the more probable one, as the other part had increased.

So much then for the chromosphere and the prominences, which I think the recent work has shown to be the last layer of the true atmosphere of the sun. I shall now invite your attention to spots.

Now, as a rule, precisely those lines which are injected into the photosphere by convection currents are most thickened in the spectrum of a spot, and the thickening increases with the depth of the spot, so that I no longer regard a spot simply as a cavity, but as a place in which principally the vapours of sodium, barium, iron, and magnesium occupy a lower level than they do ordinarily in the atmosphere.

I have told you before, that when these lines are observed in the chromosphere, they usually are thinner than their usual Fraunhofer lines.

I will now show a photograph of a spot spectrum on the screen. You will see a black band running across the ordinary spectrum; that black band indicates the general absorption which takes place in a sun spot. Now mark the behaviour of the Fraunhofer lines; see how they widen as they cross the spot, putting on a sudden blackness and width in the case of a spot with steep sides, expanding gradually in a shelving one. The behaviour of these lines is due to selective absorption.

We have, then, the following facts: mark them well—

1. The lines of sodium, magnesium, and barium, when observed in the chromosphere, are among those which are thinner than their usual Fraunhofer lines.

2. The lines of sodium, magnesium, and barium, when observed in a spot, are among those which are thicker than their usual Fraunhofer lines.

They show, I think, that a spot is the seat of a downrush or downsinking.

Messrs. De la Rue, Stewart, and Loewy, who brought forward the theory of a downrush before my observations of an actual downrush were made in 1865, at once suggested as one advantage of this explanation that all the gradations of darkness, from the faculæ to the central umbra, may be supposed to be due to the same cause, namely, the presence to a greater or less extent of a relatively cooler absorbing atmosphere; thus suggesting as one cause of the darkening of a spot—

1. The general absorption of the atmosphere, thicker here than elsewhere, as the spot is a cavity.

To which the spectroscope added in 1866, as you know—

2. Greater selective absorption.

I have Dr. Frankland's permission to exhibit an experiment connected with our researches on absorption which will show you that this increased selective absorption can be fairly grappled with in our laboratories. I will show you on the screen the absorption line due to sodium vapour, in one part as thin as it is in the ordinary solar spectrum; in another, almost if not quite as thick as it appears in a spot; and I accomplish this result in the following way:—Here I have an electric lamp, and by means of this slit I only permit a fine line of light to emerge from it; here the beam passes through a bisulphide of carbon prism, and there you see on the screen the glorious spectrum, due to the dismemberment of the fine line of polychromatic light. Mr. Pedler will now place a glass tube containing metallic sodium, sealed up with hydrogen, in front of the slit, and will heat it with a spirit lamp.

As the sodium vapour rises you see the dark line of absorption make its appearance as an extremely fine line, and finally you see that the light which traverses the upper layer of the sodium scarcely suffers any absorption—the line is thin; while, on the contrary, the light which has traversed the lower, denser layers has suffered tremendous absorption: the line is inordinately thick, such as we see it in the spectrum of a spot.

So much then for the selective absorption. My recent observations, to which I will shortly draw attention, show, I think, that it is of great importance, especially in connection with the fact that the passage from the penumbra to the umbra is generally less gradual than that from the photosphere to the penumbra. You see now how much is included in the assertion that the photosphere is gaseous.

You are all, I know, familiar with that grand generalization of Kirchhoff's, by which he accounted for the Fraunhhofer lines.

If we have a gas or a vapour less luminous than another light-source, and view that light-source through the gas or vapour, then we shall observe absorption of those particular rays which the gaseous vapour would emit if incandescent.

Let us confine our attention to the hydrogen Fraunhofer lines.

When I observe the chromosphere on the sun's limb, with no brighter light-source behind it, I observe its characteristic lines *bright*. But when I observe them on the sun itself—that is, when the brighter sun is on the other side of the hydrogen envelope, then, as a rule, its function is reduced—is toned down—the envelope acts as an absorber—the lines are observed black.

Now what must we conclude when I tell you that, at the present time, it is almost impossible to observe the sun for an hour without observing the hydrogen lines, every now and then, *bright upon the sun itself!*

Not only are the lines observed bright, but it would appear that the strongly luminous hydrogen is carried up by the tremendous convection currents at different pressures; and under these circumstances the bright line is seen to be expanded on both sides of its normal position. Moreover, at times there is a dim light on both sides the black line, and the line itself is thinned out, showing that, although there is an uprush of strongly luminous material, the column is still surmounted by some less luminous hydrogen, possibly separated from the other portion, which still performs the functions of an absorber. This seems established by another fact, namely that at times the lines, still black, expand on both sides, as if, in fact, in these regions there were a depression in the chromosphere; you already know that the pressure is greater at the base of the chromosphere than at the summit.

For this reason it is best to observe these phenomena by means of the green line, which expands in a more decided manner by pressure than does the red.

I now come to a new field of discovery opened out by these investigations, a branch of the inquiry which I fear you will consider more startling than all the rest—a branch, however, which I have had many opportunities of studying, and which has required me to move with the utmost caution. I allude to the movements of the hydrogen envelope and prominences at which I have before hinted.

Any one who has observed the sun with a powerful telescope, especially in a London fog—all too great a rarity unfortunately for such work—will have been struck with the tremendous changes observed in spots. Now, change means movement, and as spot phenomena occur immediately below the level of the chromosphere we may easily imagine that the chromosphere and its higher waves, the prominences, will also partake of the movements, be they up- or down- rushes, cyclones, or merely lateral motions. I have thrown on the screen a photograph of a drawing of a sun-spot observed under the clear sky of Rome by Father Secchi—a drawing I regard as a most faithful counterpart of nature.

You see how the photosphere is being driven about and contorted; how here it seems to be torn to ribbons by the action of some tremendous force, how here it is dragged down and shivered to atoms.

The spectroscope enables us to determine the velocities of these movements with a considerable approach to accuracy; and at times they are so great that I am almost afraid to mention them to you.

Let me first endeavour to give you an idea how this result is arrived at, and I must here beg your indulgence for a gross illustration of one of the most supremely delicate of nature's operations.

Imagine a barrack out of which is constantly issuing with measured tread and military precision an infinite number of soldiers in single or Indian file; and suppose yourself in a street seeing these soldiers pass. You stand still, and take out your watch, and find that so many pass you in a second or minute, and that the number of soldiers, as well as the interval between them, is always the same.

You now move slowly towards the barrack, still noting what happens. You find that more soldiers pass you than before in the same time, and, reckoned in time, the interval between each soldier is less.

You now move still slowly from the barrack, *i.e.* with the soldiers. You find that fewer soldiers now pass you, and that the interval between each is longer.

Now suppose yourself at rest, and suppose the barrack to have a motion now towards you, now from you.

In the first case the men will be paid out, so to speak, more rapidly. The motion of the barrack-gate towards you will plant each soldier nearer the preceding one than he would have been if the barrack had remained at rest. The soldiers will really be nearer together.

In the second case it is obvious that the interval will be greater, and the soldiers will really be further apart.

So that, generally, representing the interval between each soldier by an elastic cord, if the barrack and the eye approach each other by the motion of either, the cord will contract; in the case of recession, the cord will stretch.

Now let the barrack represent the hydrogen on the sun, perpetually paying out waves of light, and let the elastic cord represent one of these waves; its length will be changed if the hydrogen and the eye approach each other by the motion of either.

Particular wave lengths with the normal velocity of light are represented to us by different colours.

The long waves are red.

The short waves are violet.

Now let us fix our attention on the green wave, the refrangibility of which is indicated by the F line of hydrogen. If any change of wave length is observed in this line, *and not in the adjacent ones*, it is clear that it is not to the motion of the earth or sun, but to that of the hydrogen itself and alone that the change must be ascribed.

If the hydrogen on the sun is approaching us *the waves will be crushed together;* they will therefore be shortened, and the light will incline towards the violet, that is, towards the light with the shortest waves; and if the waves are shortened only by the $\frac{1}{10000000}$th of a millemeter we can detect the motion.

If the hydrogen on the sun is receding from us the waves will be drawn out, they will therefore be longer, and the green ray will incline towards the red.

I must next point out, that there are two different circumstances under which the hydrogen may approach or recede from the eye.

I have here a globe, which we will take as representing the sun. Fix your attention on the centre of this globe: it is evident that an uprush or a downrush is necessary to cause any alteration of wave length. A cyclone or lateral movement of any kind is powerless; there will be no motion to or from the eye, but only at right angles to the line of sight.

Next fix your attention to the edge of the globe—the limb, in astronomical language; here it is evident that an upward or downward movement is as powerless to alter the wave length as a lateral movement was in the other case, but that, should any lateral or cyclonic movement occur here of sufficient velocity, it might be detected.

So that we have the centre of the disc for studying upward and downward movements, and the limb for studying lateral or cyclonic movements, if they exist.

If the hydrogen-lines were invariably observed to broaden out on both sides, the idea of movement would require to be received with great caution; we might be in presence of phenomena due to greater pressure, both when the lines observed are bright or black upon the sun; but when they widen out, sometimes on one side, sometimes on the other, and sometimes on both, this explanation appears to be untenable, as Dr. Frankland and myself in our researches at the College of Chemistry have never failed to observe a widening out, equally or nearly so, on both sides the F line when the pressure of the gas has been increased.

You see now on the screen a diagram showing the strange contortions which the F hydrogen line undergoes at the centre of the sun's disc. Not only have we the line bright, as I have before told you, but the dark one is twisted in places, generally inclining towards the red; and often when this happens we have a bright line on the violet side. You see it, sometimes, stopping short of one of the small sun-spots; swelling out prior to disappearance; invisible in a facula between two small spots; changed into a bright line, and widened out on both sides two or three times in the very small spots; becoming bright near a spot, and expanding over it on both sides; very many times widened out near a spot, sometimes considerably, on the less refrangible side; and, finally, extended as a bright line without any thickening over a small spot.

Now the other Fraunhofer lines on the diagram may be looked upon as so many milestones telling us with what rapidity the uprush and downrush take place; for these twistings are nothing more or less than alterations of wave length, and thanks to Ångström's map we can map out distances along the spectrum from F in $\frac{1}{10000000}$ths of a millimeter from the centre of that line; and we know that an alteration of that line $\frac{1}{10000000}$th mm, towards the violet means a velocity of 38 miles a second towards the eye, *i.e.* an uprush; and that a similar alteration towards the red means a similar velocity from the eye, *i.e.* a downrush. The fact that the black line inclines to the red shows that the less bright hydrogen descends; the fact that the bright line—where both are visible side by side—inclines to the violet, shows that the more vivid hydrogen ascends; and the alteration of wave length is such that 20 miles a second is very common.

Now, observations of the lateral motions at the limb are of course made by the chromospheric bright lines seen beyond the limb. Here the velocities are very much more startling; not velocities of uprush and downrush, as you now know, but swinging and cyclonic motions of the hydrogen.

I will first show you a cyclone observed on the 14th of March, but before I do so let me make one remark. Although the slit used is as narrow as I can make it, let us say $\frac{1}{500}$th—I have not measured it—of an inch, a strip of this breadth, of the sun's image, is something considerable, as the glorious sun himself is painted by my object-glass only about ·94 inch in diameter, so that after all the slit lets in to be analyzed a strip some 1800 miles wide.

Now, suppose we have a cyclone of incandescent hydrogen some 1500 miles wide tearing along with a very rapid rotatory motion, it is clear that all this cyclone could fall within the slit; and that if the rotatory motion were sufficiently rapid the spectroscope should separate the waves which are carried towards us from those which are receding. It does this: as you see, we have an alteration of wave-length both towards the red and violet, amounting to something like 40 miles a second. Now it should be clear to you that, by moving the slit first one way and then the other, we may be able to bring it in turn to such positions that only the light proceeding from either side of the cyclone can enter it. Then we shall have changes of wave-length in one direction only, in each case precisely as you see was observed.

Now, let us suppose that instead of a cyclone, we have a motion of some portions of the prominence towards the eye; and that, moreover, the rate of motion varies excessively in some portions. What we shall see will be this. The portion of the prominence at rest will give us no alteration of wave-length; its bright line will be in a line with the corresponding black one in the spectrum. The portion moving towards the eye, however, will give us an alteration of wave-length towards the violet. You are now in a position to grasp the phenomena revealed to me by my spectroscope on the 12th instant, when at times the F line was triple! the extreme alteration of wave-length being such that the motion of that part of the prominence giving the most extreme alteration of wave-length must have exceeded 120 miles per second, if we are to explain these phenomena by the only known possible cause which is open to us.

By moving the slit it was possible to see in which part of the prominence these great motions arose, and to follow the change of wave-length to its extremest limit.

By the kindness of Dr. Balfour Stewart I am able to exhibit to you some of the Kew sun-pictures which show you how these spectroscopic changes are sometimes connected with telescopic ones.

On the 21st April there was a spot very near the limb which I was enabled to observe continuously for some time. At 7.30 A.M. there was a prominence visible in the field of view, in which tremendous action was evidently going on, for the C, D, and F lines were magnificently bright in the ordinary spectrum itself, and as the spot-spectrum was also visible it was seen that the prominence was in advance of the spot. The injection into the chromosphere surpassed anything I had seen before, for there was a magnesium cloud quite separated from the limb, and high up in the prominence itself.

By 8.30 the action had quieted down, but at 9.30 another throb was observed, and the new prominence was moving away with tremendous velocity. While this was going on, the hydrogen lines suddenly became bright on the other side (the earth's side) of the spot, and widened out considerably—indeed to such an extent that I attributed their action to a cyclone, although, as you know, this was a doubtful case.

Now, what said the photographic record? The sun was photographed at 10h. 55m. A.M., and I hope you will be able to see on the screen how the sun's surface was disturbed near the spot. A subsequent photograph at 4h. 1m. P.M. on the same day shows the limb to be actually broken in that particular place: the photosphere seems to have been absolutely torn away behind the spot, exactly when the spectroscope had afforded me possible evidence of a cyclone!

In connection with the last branches of the research I have brought to your notice, I may remark that we have two very carefully prepared recent maps of the solar spectrum, one by Kirchhoff, the other by Angström, made a few years apart and at different epochs with regard to the sun-spot period. If you look at these maps you will see a vast difference in the relative thicknesses of the C and F lines, and great differences in the relative darkness and position of the lines; and if I had time I could show you that we now may be supplied with a barometer, so to speak, to measure the varying pressures in the solar and stellar chromospheres; for, depend upon it, every star has, has had, or will have, a chromosphere, and there are no such things as "worlds without hydrogen," any more than there are stars without photospheres. I suggested in 1866 that possibly a spectroscopic examination of the sun's limb might teach us somewhat of the outburst of the star in Corona, and already we see that all that is necessary to get just such an outburst in our own sun is to increase the power of his convection currents, which we know to be ever at work. Here, then, is one cataclysm the less in astronomy—one less "World on Fire," and possibly also a bright light thrown on the past history of our own planet.

I might show you further that we now are beginning to have a better hold on the strange phenomena presented by variable stars, and that an application of the facts I have brought to your notice this evening, taken in connection with the various types of stars which have been indicated by Father Secchi with admirable philosophy, opens out generalizations of the highest interest and importance; and that having at length fairly grappled with some of the phenomena of the nearest star, we may soon hope for more certain knowledge of the distant ones.

At present, however, we may well leave speculation for those who prefer it to acquiring facts; let us rather, emboldened by the work which this new method of research has enabled us to accomplish in this country, under the worst atmospheric conditions, in seven short months, go on quietly deciphering one by one the letters of this strange hieroglyphic language which the spectroscope has revealed to us—a language written in fire on that grand orb which to us earth-dwellers is the fountain of light and heat, and even of life itself.

[J. N. L.]

WEEKLY EVENING MEETING,

Friday, June 4, 1869.

His Royal Highness the Prince of Wales, K.G. Vice-Patron, in the Chair.

William Odling, Esq. M.B. F.R.S.

FULLERIAN PROFESSOR OF CHEMISTRY, R.I.

On the Simplest Organic Compounds.

All the olefine hydrocarbons are found to have one and the same ultimate composition, or ratio of carbon to hydrogen.

$C_2 H_4$	Ethylene
$C_3 H_6$	Propylene
$C_4 H_8$	Butylene
$C_5 H_{10}$	Amylene
$C_x H_{2x}$	&c., &c.

But these hydrocarbons obviously differ in the complexity of their constitution. Some of them are gases, differing from one another in condensability; others of them are liquids, differing from one another in volatility; while others of them are solids, differing from one another in fusibility. The exact degree of complexity of each hydrocarbon is shown by its reactions. Thus the hydrogen of gaseous ethylene being experimentally divisible into four parts, and its carbon into two parts, there is deduced for it the formula C_2H_4; while the hydrogen of liquid amylene being experimentally divisible into ten parts, and its carbon into five parts, there is deduced for it the formula C_5H_{10}. Again, it is possible to extract from a given volume of ethylene gas and amylene vapour, four times and ten times respectively the actual weight of hydrogen obtainable from the same volume of hydrochloric acid gas; and also to extract therefrom two times and five times respectively the actual weight of carbon that is obtainable from the same volume of carbonic acid gas—these two gases, formulated as HCl and CO_2, containing within a given volume the smallest observed weights of hydrogen and of carbon respectively. The olefine hydrocarbons are said to be polymeric, and their different properties are satisfactorily referable to the different relative weights of their units or molecules. There exist many other series of polymeric bodies, that is of bodies having one and the same ultimate composition, but different molecular weights.

II.

Acetone, propion-aldehyd, and allyl-alcohol are entirely different substances, possessed of well-marked distinctive properties. Like the several olefines, they have the same ultimate composition as each other; but, unlike the several olefines, they have also the same molecular weight as each other, and are expressed by the same molecular formula C_3H_6O. In each of them the carbon is experimentally divisible into three parts, and the hydrogen into six parts, while the oxygen is indivisible; and it is possible to extract from any given volume of one of them exactly the same weights of carbon, hydrogen, and oxygen that are obtainable from the same gas-volume of each of the other two. These different bodies are said to be metameric, and their different properties are necessarily referable to a difference in the arrangement of their constituent elements.

III.

The existence of a determinate structural arrangement in chemical compounds is further demonstrated by a host of considerations; but the difficulty of making out the actual structure of individual compounds has hitherto proved insuperable. The facility of setting forth imaginary structure, however, is very great; and accordingly the presentation of imaginary for ascertained structure has been freely practised by chemists from the first introduction of chemical formulæ until now. But in what degree soever a determination of absolute chemical structure may hereafter be achieved, the possibility exists very generally, even at the present day, of determining relative chemical structure—of making out that in such and such a body the structural arrangement is similar to, or different from, that of some other and usually more simple body. Hence the importance of studying the structural analogies of the simplest organic bodies.

IV.

Marsh-gas is furnished by the decay of moist vegetable tissue, and in other ways. Chloride of methyl-gas is furnished by the action of hydrochloric acid upon narcotine, codeine, wood-spirit, &c. The ultimate composition of the two gases is expressed by the formulæ CH_4 and CH_3Cl respectively. Marsh-gas is transformable into methyl-chloride by the action of chlorine; and methyl-chloride into marsh-gas by the action of hydrogen. From this mutual metamorphosis, and from the parallelism of their properties, formations, and transformations, the two gases are inferred to have one and the same molecular structure, whatever that may be; and the same conclusion is applicable to the entire series of bodies formulated below:—

CH_4	Marsh-gas.
CH_3Cl	Methyl-chloride.
CH_2Cl_2	Methylen-dichloride.
$CHCl_3$	Chloroform.
CCl_4	Perchloride of carbon.

V.

Wood-spirit is usually furnished by the destructive distillation of wood, but is procurable from many other sources, and especially from essential oil of winter-green, by its decomposition with potash. The ultimate composition of wood-spirit is expressed by the formula CH_4O; and it is observable that the difference of ultimate composition between wood-spirit and methyl-chloride CH_3Cl, is the same as that between water H_2O, and hydrochloric acid HCl; as is shown more clearly by the following formulæ, in which the differential constituents of the two pairs of bodies are included in parentheses.

$$H(Cl) \;..\;..\; H(HO). \quad | \quad CH_3(Cl) \;..\;..\; CH_3(HO).$$

Further, the residue H of hydrochloric acid and residue CH_3 of methyl-chloride are transformable into water and wood-spirit respectively, and re-transformable from water and wood-spirit back to hydrochloric acid and methyl-chloride respectively, by precisely similar reactions.

From the mutual metamorphoses and parallel habitudes of the two bodies, methyl-chloride and wood-spirit, it is inferred that, with regard to their common residue CH_3, they have the same structure as one another; while, with regard to their differential constituents, (Cl) and (HO), their difference of structure is analogous to the difference between hydrochloric acid and water.

VI.

Hydrochloric acid HCl, and water H_2O, are the first and second terms of a series of bodies of which the third and fourth terms are constituted by ammonia H_3N, and marsh-gas H_4C respectively. Hence arises a question as to the existence of organic bodies bearing to methyl-chloride and wood-spirit the relation that ammonia and marsh-gas bear to hydrochloric acid and water.

VII.

Many years ago, methyl-amine was discovered by Wurtz. It is now known to occur as a product of the putrefaction of, and also of the action of alkalis upon, many animal substances. Its resemblance in properties to ammonia is most remarkable, and its differentiation therefrom somewhat difficult. Its ultimate composition is expressed by the formula CH_5N; whence it appears that the difference of ultimate composition between methyl-amine and wood-spirit and methyl-chloride is the same as that between ammonia and water and hydrochloric acid.

$$\begin{array}{llll|llll}
HCl & .. & .. & CH_3Cl & H(Cl) & .. & .. & CH_3(Cl) \\
H_2O & .. & .. & CH_4O & H(HO) & .. & .. & CH_3(HO) \\
H_3N & .. & .. & CH_5N & H(H_2N) & .. & .. & CH_3(H_2N)
\end{array}$$

Moreover, methyl-amine, wood-spirit, and methyl-chloride are mutually convertible, through exchanges of their differential constituents, by processes effecting similar mutual conversions of hydrochloric acid, water, and ammonia. From the mutual metamorphoses and parallel habitudes of the three bodies, it is inferred that, with regard to their common residue CH_3, they have the same structure as each other, while with regard to their differential constituents (Cl), (HO), and (H_2N), their difference of structure is similar to the difference between hydrochloric acid and water and ammonia.

VIII.

Wood-spirit or methyl-hydrate is producible by the action of methyl-chloride upon sodium-hydrate NaHO; and methyl-amine by the action of methyl-chloride upon sodium-amide NaH_2N. Similarly, methyl-methide is producible by the action of methyl-chloride upon sodium-methide NaH_3C. (Action of methyl-iodide upon zinc-methide.—Frankland and Kolbe.)

$$\begin{aligned} NaHO &+ CH_3Cl = CH_4O + NaCl \\ NaH_2N &+ CH_3Cl = CH_5N + NaCl \\ NaH_3C &+ CH_3Cl = CH_6C + NaCl \end{aligned}$$

This methyl-methide is now known to be identical with the hydrocarbon C_2H_6, producible by the indirect deoxidation of common alcohol C_2H_6O, and known as ethane or hydride of ethyl. Ethane $CH_3(H_3C)$, being susceptible of two distinct and similar sets of marsh-gas reactions, just as methyl-amine $CH_3(H_2N)$ is susceptible of one set of marsh-gas and one set of ammonia reactions—as before, an identity of structure between ethane and methyl-amine is inferred with respect to their common residue CH_3, and a difference of structure with respect to their differential constituents (H_3C) and (H_2N), parallel to the difference in structure between marsh-gas and ammonia.

IX.

The property of oxygen that is half-saturated by hydrogen, or of the water residue HO'', and the property of nitrogen that is two-thirds saturated by hydrogen, or of the ammonia residue H_2N''', to suffer an exchange for one proportion of chlorine or hydrogen, has been shown in the above, and might be shown in many other compounds. But oxygen, altogether unsaturated by hydrogen, has the additional property of being exchangeable for two proportions of chlorine or hydrogen; and nitrogen, altogether unsaturated by hydrogen, has the additional property of being exchangeable for three proportions of chlorine or hydrogen, as indicated by the succeeding formulæ for mercuric oxide and mercuric chloride, and for boric nitride and boric chloride respectively.

$$\begin{array}{lll} NaF' \quad .. \quad .. & HgO'' \quad .. \quad .. & BN''' \\ NaCl \quad .. \quad .. & HgCl_2 \quad .. \quad . & BCl_3 \end{array}$$

Marsh-gas compounds, in which hydrogen or chlorine has been exchanged for half-saturated oxygen and two-thirds saturated nitrogen, have been already adduced in wood-spirit CH_4O, and methylamine CH_5N; and reference has also been made to the corresponding hydro-carbon CH_6C or C_2H_6. The question now arises as to the existence of marsh-gas compounds in which hydrogen or chlorine has been exchanged for else unsaturated oxygen and nitrogen, and of hydrocarbons corresponding to these compounds.

X.

Some few years back a curious oxidation product of wood-spirit was discovered by Hofmann. It is now called formic aldehyd, and its ultimate composition is expressed by the formula CH_2O. In respect of ultimate composition its relationship to the second marsh-gas chloride CH_2Cl_2 is obviously similar to that of mercuric oxide HgO, to mercuric chloride $HgCl_2$. Both formic aldehyd and methylen dichloride, however, are imperfectly studied compounds, and their actual metamorphoses are unknown. But in the case of many other aldehyds and corresponding organic dichlorides, the relationship of mutual metamorphosis is well established, whence it is believed to exist between these two compounds also. On this assumption formic aldehyd CH_2O'', and methylen dichloride CH_2Cl_2, are inferred to have one and the same structural arrangement in respect to their common residue CH_2, and a difference of structure in respect to their differential constituents O'' and Cl_2, similar to the difference between oxide and chloride of mercury for example.

XI.

Prussic acid is a well-known organic compound producible by the action of water on bitter almond kernels, and in various other ways. Its ultimate composition being expressed by the formula CHN, the difference in ultimate composition between it and chloroform $CHCl_3$, is obviously similar to the difference in composition between nitride and chloride of boron, BN and BCl_3 respectively. Now chloroform is readily converted into prussic acid, and boric chloride into boric nitride, by the similar action of ammonia upon the two compounds, with exchange of Cl_3 for N''', thus :—

$$\begin{aligned} CHCl_3 + H_3N &= CHN + 3HCl \\ BCl_3 + H_3N &= BN + 3HCl. \end{aligned}$$

Prussic acid CHN''', and chloroform $CHCl_3$, are accordingly inferred to have one and the same structure in respect of their common residue CH, and a difference of structure in respect of their differential constituents N''' and Cl_3, parallel to the difference between nitride and chloride of boron for example.

XII.

Allowing for the different replaceable values of oxygen O″, and nitrogen N‴, the relationship of methyl-amine to prussic acid is comparable with that of formic aldehyd to wood-spirit:—

Wood-spirit	$C\,H_4\,O$	Formic aldehyd	$C\,H_2\,O$		
Methylamine	$C\,H_5\,N$		„	Prussic acid	$C\,H\,N$
Ethane . .	$C\,H_6\,C$	Ethylene . .	$C\,H_4\,C$	Acetylene .	$C\,H_2\,C$

Prussic acid and formic aldehyd are procurable from methylamine and wood-spirit respectively, by similar processes of dehydrogenation or oxidation; while methyl-amine actually, and wood-spirit analogically, are reproducible from prussic acid and formic aldehyd by the reverse process of hydrogenation. Now the difference in ultimate composition, between ethane and olefiant gas, or ethylene, is similar to that between wood-spirit and formic aldehyd; and the difference of ultimate composition, between the hydrocarbons ethane and acetylene, is similar to that between methyl-amine and prussic acid. Moreover ethane is convertible successively into ethylene and acetylene by oxidation; and acetylene is reconvertible into ethylene and ethane successively by hydrogenation.

XIII.

The experimental relationship of ethane, methyl-amine, and wood-spirit has been already considered. Formic aldehyd being a very imperfectly-studied compound, the relationship between it and ethylene is as yet analogical only; but the experimental relationship between acetylene and prussic acid has recently been established by Berthelot, who has shown that mixed acetylene and nitrogen are convertible into prussic acid, and that prussic acid is reconvertible into mixed acetylene and nitrogen, by the action of the electric spark.

$$C\,H_2\,C \text{ or } \overbrace{C\,H\,.\,H\,C + N\,N} = \overbrace{C\,H\,N + N\,H\,C}$$

XIV.

Just as the existence in methyl-chloride, in formic aldehyd, and in prussic acid of one marsh-gas residue, plus one hydrochloric acid residue Cl′, and one water residue O″, and one ammonia residue N‴, respectively, is inferable from the relationships and behaviour of the respective bodies—so is the existence alike in ethane, ethylene, and acetylene of two marsh-gas residues inferable from the relationships and behaviour of these respective bodies. As shown more especially by Kekulé, the individual marsh-gas residues of every hydrocarbon are found to be susceptible of changes exactly similar to those of marsh-gas itself, and resulting in the formation of similarly characterized bodies, as indicated below in the case of some ethane and ethylene derivatives.

Ethyl chloride	$CH_3.CH_2(Cl)$	Vinyl-chloride	$CH_2.CH(Cl)$
Alcohol . .	$CH_3.CH_2(HO)$	Vinyl-alcohol	$CH_2.CH(HO)$
Ethylamine	$CH_3.CH_2(H_2N)$	Vinyl-amine	$CH_2.CH(H_2N)$
Propane . .	$CH_3.CH_2(H_3C)$	Propylene	$CH_2.CH(H_3C)$

XV.

In any hydrocarbon or chlorhydrocarbon, the substitution of half-saturated oxygen HO'', for one proportion of hydrogen or chlorine, is productive of a compound either similar or dissimilar in its properties to common alcohol C_2H_6O or $CH_3.CH_2(HO)$. Hence, from an examination of the resulting product, and of its other modes of formation, an inference is deducible as to its genesis having resulted from a change effected in the marsh-gas residue CH_3 in the former case, and from a change effected in one or other of the marsh-gas residues CH_2 and CH in the latter case.

In any hydrocarbon or chlorhydrocarbon, the substitution of one proportion of oxygen O'', for two proportions of hydrogen or chlorine, is productive of a compound either similar or dissimilar in its properties to common aldehyd C_2H_4O or $CH_3.CHO''$. Hence, from an examination of the resulting product, and of its other modes of formation, an inference is deducible as to its genesis having resulted from a change effected in the marsh-gas residue CH_3 in the former case, and from a change effected in the marsh-gas residue CH_2 in the latter case.

The relative structure of the previously cited metameric compounds propion-aldehyd, acetone, and allyl-alcohol is thus ascertainable, and is expressed in the following structural formulæ:—

Hydrocarbon.	Metameric Derivatives.	
C_3H_8 or $CH_3.CH_2.CH_3$	$CH_3.CH_2.CHO''$	Propion-aldehyd.
	$CH_3.CO.CH_3$ ..	Acetone.
C_3H_6 or $CH_2.CH.CH_3$	$CH_2.CH.CH_2(HO)$	Allyl-alcohol.

[W. O.]

GENERAL MONTHLY MEETING,

Monday, June 7, 1869.

William Robert Grove, Esq. M.A. Q.C. F.R.S. in the Chair.

James Spencer Bell, Esq.
Honourable Henry M. Best,
Henry Davis Pochin, Esq.
were *elected* Members of the Royal Institution.

John Benjamin Marsden, Esq.
was *admitted* a Member of the Royal Institution.

The special thanks of the Members were returned for the following additions to "the Donation Fund for the Promotion of Experimental Researches":—

Sir Henry Holland, Bart. (11*th annual donation*) . .	£40
J. Carrick Moore, Esq. (6*th annual donation*) . . .	10

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

Astronomical Society, Royal—Monthly Notices. Vol. XIX. No. 6. 8vo. 1869.
Bennett, J. J. Esq. Registrar-General, New Zealand—Statistics of New Zealand, 1867. fol. 1869.
Birmingham Free Library Committee—Catalogue of the Reference Department. 8vo. 1869.
British Museum Trustees—Catalogue of Dermaptera Saltatoria, &c. 8vo. 1869.
  Guides to the Exhibition Rooms, and the Autograph Letters, &c. 8vo. 1869.
Burton, E.—Thoughts on the Separation of Church and State. (K 96) 8vo. 1868.
Chemical Society—Journal for May and June, 1869. 8vo.
Cornwall Polytechnic Society, Royal—Report for 1868. 8vo.
East India Association—Journal. Vol. III. No. 2. 8vo. 1869.
Editors—American Journal of Science, March, 1869. 8vo.
  Artizan for May, 1869. 4to.
  Athenæum for May, 1869. 4to.
  British Journal of Photography for May, 1869. 4to.
  Chemical News for May, 1869. 4to.
  Engineer for May, 1869. fol.
  Horological Journal for May, 1869. 8vo.
  Journal of Gas-Lighting for May, 1869. 4to.
  Mechanics' Magazine for May, 1869. 8vo.
  Pharmaceutical Journal for May, 1869. 8vo.
  Photographic News for May, 1869. 4to.
  Practical Mechanics' Journal for May, 1869. 4to.
  Revue des Cours Scientifiques et Littéraires. May, 1869.

Elliot, Lady—History of India as told by its own Historians. Edited from the Posthumous Papers of the late Sir H. Elliot. Vol. II. 8vo. 1869.
Geographical Society, Royal—Journal. Vol. XXXVIII. 8vo. 1869.
Catalogue of the Library. 8vo. 1865.
Geological Society—Quarterly Journal. No. 90. 8vo. 1869.
Jerwood, James, Esq. (*the Author*)—The Application of the Calculus of Probabilities to Legal Subjects. (Devon. Assoc. Trans. 1868.) 8vo.
Macdonald, William, M.D. (*the Author*)—Contributions to the History of Development in Animals. Part I. 8vo. 1868.
Macpherson, John, M.D. M.R.I. (*the Author*)—Cholera in the East. (K 96) 8vo. 1869.
The Baths and Wells of Europe. 16mo. 1869.
Mechanical Engineers' Institution, Birmingham—Proceedings, July, 1868. 8vo.
Meteorological Society—Proceedings. No. 42. 8vo. 1869.
Murchison, Sir R. I. Bart. M.R.I.—Address at the Anniversary Meeting of the Royal Geographical Society, 24th May, 1869. 8vo.
Musgrave, Rev. George, M.A. (*the Translator*)—The Odyssey of Homer in English Blank Verse. 2nd ed. 2 vols. 8vo. 1869.
Northumberland, The Duke of, M.R.I.—Incised Markings on Stone in Northumberland, Argyleshire, &c. (Privately printed.) fol. 1869.
Payne, Joseph, Esq. M.R.I. (*the Author*)—On Theories of Teaching. (L 15) 8vo. 1869.
Photographic Society—Journal. No. 205. 8vo. 1869.
Royal Society of Literature—Transactions. Second Series. Vol. IX. Part 2. 8vo. 1869.
Royal Society of London—Proceedings. No. 111. 8vo. 1868.
Société Hollandaise des Sciences—Archives. Tomes 1–3. 8vo. 1866–8.
Symons, G. J. Esq. (*the Author*)—Symons' Monthly Meteorological Magazine, May, 1869. 8vo.
Ulm and Oberschwaben Art and Antiquarian Society—Verhandlungen. Neue Reihe. Heft 1. 4to. 1869.
United Service Institution, Royal—Journal. No. 53. 8vo. 1869.
Vereins zur Beförderung des Gewerbfleisses in Preussen—Nov.-Dez. 4to. 1868.
Victoria Institute—Transactions. No. 11. 8vo. 1869.
Zoological Society—Proceedings for 1868. Part 3. 8vo. 1869.

MUSEUM.

Commissioners of the International Exhibition at Paris, 1867—Medal.
Tennant, Professor—Model of the first Gold Nugget received from Australia, 1851.

GENERAL MONTHLY MEETING,

Monday, July 5, 1869.

George Busk, Esq. F.R.S. Vice-President, in the Chair.

William Vaughan Murray, Esq.
Albert Lewis Newdigate, Esq. M.A.

were *elected* Members of the Royal Institution.

James Spencer Bell, Esq.

was *admitted* a Member of the Royal Institution.

The special thanks of the Members were returned to the Lord Vernon for his present of a copy of the Inferno of Dante, literally paraphrased, with "Documenti" and "Album," in 3 vols. folio, privately printed by his late father, George John Warren, Lord Vernon.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

Asiatic Society of Bengal—Journal. No. 152. 8vo. 1869.
  Proceedings, 1869. No. 4. 8vo.
Astronomical Society, Royal—Monthly Notices. Vol. XIX. No. 7. 8vo. 1869.
Basel Natural History Society—Verhandlungen. Theil V. Heft. 2. 8vo. 1869.
Bavarian Academy of Sciences—Sitzungsberichte. 1869. I. Heft. 1, 2. 8vo.
Chemical Society—Journal for July, 1869. 8vo.
Cornwall Polytechnic Society, Royal—Report for 1868. 8vo.
Davis, Alfred, Esq. M.R.I.—Moses Angel, the Law of Sinai. 16mo. 1868.
East India Association—Journal. Vol. III. No. 2. 8vo. 1869.
Editors—Artizan for June, 1869. 4to.
  American Journal of Science, May, 1869. 8vo.
  Athenæum for June, 1869. 4to.
  British Journal of Photography for June, 1869. 4to.
  Chemical News for June, 1869. 4to.
  Engineer for June, 1869. Fol.
  Horological Journal for June, 1869. 8vo.
  Journal of Gas-Lighting for June, 1869. 4to.
  Mechanics' Magazine for June, 1869. 8vo.
  Pharmaceutical Journal for June, 1869. 8vo.
  Photographic News for June, 1869. 4to.
  Practical Mechanics' Journal for June, 1869. 4to.
  Revue des Cours Scientifiques et Littéraires, June, 1869.
Edinburgh Royal Physical Society—Proceedings. 1862–6. 8vo.
Franklin Institute, Philadelphia - Journal. Nos. 520, 521. 8vo. 1869.
Geological Institute, Vienna—Jahrbuch: Jahrgang, 1869. No. 1. 8vo.
  Verhandlungen, 1869. No. 1. 8vo.

Grove, William R. Esq. Q.C. M.R.I. (the Author)—Address on the Study of Physical Science in Medical Education. (K 96) 8vo. 1869.
Linnean Society—Transactions. Vol. XXVI. Part 3. 4to. 1869.
Lloyd, W. Watkiss, Esq. M.R.I. (the Author)—Panics and their Panaceas: The Theory of Money, &c. (K 96) 8vo. 1869.
Macdonald, William, M.D. (the Author)—Contributions to the History of Development in Animals. Part I. 8vo. 1868.
Mechanical Engineers' Institution, Birmingham—Proceedings, Nov. 1868. 8vo.
Meteorological Society—Proceedings. No. 43. 8vo. 1869.
Photographic Society—Journal. No. 206. 8vo. 1869.
Prussian Academy of Sciences—Monatsberichte. Jan. Feb. Mar. 1869. 8vo.
Roma, Accademia de' Nuovi Lincei—Atti. Anno XXI. Sess. 1, 2, 4, 5. fol. 1868.
Symons, G. J. Esq. (the Author)—Symons' Monthly Meteorological Magazine, June, 1869. 8vo.
Tilt, Edward J. M.D. (the Author)—Handbook of Uterine Therapeutics and of Diseases of Women. Third edition. 8vo. 1868.
Vernon, The Lord, M.R.I.—L'Inferno di Dante Alighieri disposto in ordine grammaticale e corredato di brevi Dichiarazioni da G. G. Warren Lord Vernon: con Documenti ed Album. 3 vols. fol. 1858–65.

GENERAL MONTHLY MEETING,

Monday Nov. 1, 1869.

SIR HENRY HOLLAND, Bart. M.D. D.C.L. F.R.S. President, in the Chair.

The PRESENTS received since the last Meeting were laid on the table and the thanks of the Members returned for the same, viz.:—

FROM

Commissioners in Lunacy—23rd Report. 8vo. 1869.
Imperial Government of France—A. Fresnel: Œuvres, Tome II. 4to. 1868.
  Lavoisier: Œuvres, Tome IV. 4to. 1868.
  Documents Inédits sur l'Histoire de France: Appendice au Cartulaire de l'Abbaye de St. Bertin. 4to. 1867.
   Berty et Legrand: Topographie du Vieux Paris. Tome II. 4to. 1868.
   Les Familles d'Outre-mer de Du Cange. Ed. M. E. G. Rey. 4to. 1869.
   Lettres du Cardinal de Richelieu, Tome VI. 4to. 1869.
Secretary of State for India—Report of Government Astronomer at Madras (N. Pogson) on Solar Eclipse. 18th Aug. 1869. 8vo.
H.R.H. the Count of Paris, M.R.I. (the Author)—Les Associations Ouvrières (Trades' Unions). 16mo. Paris, 1869.

Académie Impériale des Sciences de St. Pétersbourg—Mémoires. VII^{e} Série, Tome XII. Nos. 4, 5. Tome XIII. Nos. 1–7. 4to. 1868–9.
  Bulletins, Tome XIII. Nos. 4, 5. 4to. 1868.
Agricultural Society of England, Royal—Journal, Second Series, No. 10. 8vo. 1869.
American Academy of Arts and Sciences—Proceedings, Vol. VII. Nos. 44–46. 8vo. 1867–8.
American Philosophical Society—Proceedings, Nos. 78, 79, 81. 8vo. 1867–9.
  Transactions, Vol. XIII. Part 3. 4to. 1869.
Asiatic Society of Bengal—Journal, Nos. 153, 154, 155. 8vo. 1869.
  Proceedings, 1869, Nos. 2, 3, 5, 6, 7. 1869. No. 1. 8vo.
Asiatic Society, Royal—Journal, New Series, Vol. IV. Part 1. 8vo. 1869.
Astronomical Society, Royal—Monthly Notices, Vol. XIX. Nos. 8, 9. 8vo. 1869.
Attfield, John, Esq. Ph.D. (the Author)—Chemistry, General, Medical, and Pharmaceutical. 2nd ed. 1869.
Author, the—Perpetual Motion. (K 97) 8vo. 1867.
Bombay Geographical Society—Transactions, 1865–67. Vol. XVIII. 8vo. 1868.
Boston Society of Natural History, U.S.—Memoirs, Vol. I. Part 4. 4to. 1869.
  Proceedings, Vol. XII. Nos. 1–17. 8vo. 1868–9.
  Harris, Dr. T. W., Entomological Correspondence. 8vo. 1869.
British Association for the Advancement of Science—Report of the 38th Meeting held at Norwich, Aug. 1868. 8vo. 1869.
Cambridge Philosophical Society—Transactions, Vol. XI. Part 2. 4to. 1869.
  Proceedings, Parts 3–6. 8vo. 1866–7.
Chemical Society—Journal for Aug.-Oct. 1869. 8vo.
Compiler, the—Discussions on Abolition of Patents for Inventions. 8vo. 1869.
Davis, Alfred, Esq. M.R.I.—Walter Dickson: Japan, being a Sketch of the History, Government, and Officers of the Empire. 8vo. 1869.
  James Greenwood: The Seven Curses of London. 12mo. 1869.
Dublin Society, Royal—Journal, No. 38. 8vo. 1869.

Editors—American Journal of Science, July to Sept. 1869. 8vo.
Artizan for July to Oct. 1869. 4to.
Athenæum for July to Oct. 1869. 4to.
Chemical News for July to Oct. 1869. 4to.
Engineer for July to Oct. 1869. fol.
Horological Journal for July to Oct. 1869. 8vo.
Journal of Gas-Lighting for July to Oct. 1869. 4to.
Quarterly Journal of Science for Aug. Sept. Oct. 1869. 8vo.
Mechanics' Magazine for July to Oct. 1869. 8vo.
Pharmaceutical Journal for July to Oct 1869. 8vo.
Photographic News for July to Oct. 1869. 4to.
Practical Mechanics' Journal for July to Oct. 1869. 4to.
Revue des Cours Scientifiques et Littéraires. July to Oct. 1869. 4to.
Essex Institute, U.S.—Proceedings, Vol V. Nos. 7, 8. 1868.
Franklin Institute—Journals, Nos. 522, 523, 524. 8vo. 1869.
Genève, Société de Physique—Mémoires, Tome XX. Partie 1. 4to. 1869.
Hill, M. D. Esq. (the Author)—On Allegiance by Birth. (K 97) 8vo. 1869.
Institution of Civil Engineers—Minutes of Proceedings, Vols. XXVII. XXVIII. 8vo. 1868–9.
Linnean Society—Journal, Nos. 46, 50, 51, and Vol. XII. 8vo. 1869.
Long, Colonel Samuel, M.R.I.—R. Stephenson, on the Isthmus of Suez Canal. (K 97) 8vo. 1858.
Lubbock, Sir John, Bart. F.R.S. M.R.I. (the Author)—Pre-historic Times, as illustrated by Ancient Remains and Modern Savages. 2nd ed. 1869.
Manchester Literary and Philosophical Society.—Memoirs, New Series, Vol. III. 8vo. 1868. Proceedings, Vols. V.–VII. 8vo. 1865–8.
Marcet, William, M.D. F.R.S. M.R.I. (the Author)—Clinical Notes on the Diseases of the Larynx. 12mo. 1869.
Mechanical Engineers' Institution, Birmingham—Proceedings, April, 1869. 8vo.
Medical and Chirurgical Society, Royal—Proceedings, Vol. VI. No. 4. 8vo. 1869.
Additions to Library, No. 12. 8vo. 1869.
Meteorological Committee of the Royal Society—Reports. 8vo. 1869.
Charts, showing the Surface Temperature of the Atlantic Ocean. fol. 1869.
Meteorological Society—Proceedings, No. 44. 8vo. 1869.
National Academy of Sciences, Washington, U.S.—Reports, 1866, 1867. 8vo.
Osborn, Captain Sherard, R.N. C.B.—Chart of North Atlantic Ocean: Deep Sea Soundings. 1865, 1866, 1869. (Portfolio I.)
Photographic Society—Journals, Nos. 207-210. 8vo. 1869.
Rae, John, Esq. M.R.I. (the Editor)—Statutes of Henry VII. in exact Facsimile, from the very rare Original one printed by Caxton in 1489. 4to. 1869.
Royal Society of London—Proceedings, Nos. 112, 113, 114. 8vo. 1869.
Greenwich Observations for 1867. 4to. 1869.
Smithsonian Institution, U.S.—Annual Report, 1867. 8vo. 1868.
Statistical Society—Journal, Vol. XXXII. Parts 2, 3. 8vo. 1869.
Stone, E. M. Esq. (the Author)—Memoir of T. A. Tefft. (L 15) 8vo. 1869.
Sylvester, J. J. LL.D. (the Author)—Theory of Reducible Cyclodes. (K 97) 8vo. 1869.
Symons, G. J. Esq. (the Author)—Symons' Monthly Meteorological Magazine, July to Oct. 1869. 8vo.
Treasurer, the—St. Bartholomew's Hospital Reports. Vol. V. 8vo. 1869.
Trinity College, Dublin: Provost and Senior Fellows.—Magnetical and Meteorological Observations. Vols. I. II. 4to. 1865–9.
United Service Institution, Royal—Journal, Nos. 54, 55. 8vo. 1869.
Van der Mensbrugghe, M. (the Author)—Sur la Tension Superficielle de Liquides. 4to. Bruxelles. 1869.
Vereins zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, Mai, Juni. 1869. 4to.
Victoria Institute—Journal, Vol. III. No. 12. 8vo. 1869.
Zoological Society—Transactions, Vol. VI. Part 8. 4to. 1869.
Proceedings, 1869. No. 1. 8vo.

GENERAL MONTHLY MEETING,

Monday, December 6, 1869.

Sir Henry Holland, Bart. M.D. D.C.L. F.R.S. President,
in the Chair.

George Henderson Gibb, Esq.
William Harbottle, Esq.
John Henderson, Esq.
Henry Musgrave Musgrave, Esq.

were *elected* Members of the Royal Institution.

The following Lecture Arrangements for the ensuing Season were announced:—

Professor Tyndall, LL.D. F.R.S. — Six Lectures *(adapted to a Juvenile Auditory)*, On Light. On December 28th, 30th, 1869; January 1st, 4th, 6th, 8th, 1870.

Before Easter, 1870.

Professor Humphry, M.D. F.R.S.—Six Lectures, On the Architecture of the Human Body. On Tuesdays, January 18th to February 22nd.

Professor Odling, F.R.S.—Twelve Lectures, On the Chemistry of Vegetable Products. On Thursday, January 20th to April 7th.

Robert Scott, Esq. M.A. Director of the Meteorological Office. — Four Lectures, On Meteorology. On Saturdays, January 22nd to February 12th.

Dr. Masters. F.L.S.—Two Lectures, On Plant Life as contrasted with that of Animals. On Tuesdays, March 1st and 8th.

Professor Rolleston, M.D. F.R.S. — Four Lectures, Deductions from the Comparative Anatomy of the Nervous System. On Tuedays, March 15th to April 5th.

Professor Max Müller, M.A. LL.D.—Four Lectures, An Introduction to the Science of Religion. On Saturdays, February 19th to March 12th.

Joseph Norman Lockyer, Esq. F.R.S.—Four Lectures, On the Sun. On Saturdays, March 19th to April 9th.

After Easter.

Professor Blackie. — Four Lectures, On the Principles of Moral and Political Philosophy. On Tuesdays, April 26th to May 17th.

Professor Tyndall, LL.D. F.R.S. — Seven Lectures, On Physics. On Thursdays, April 28th to June 9th.

Professor Robert Grant, LL.D. F.R.S.—Seven Lectures, On Astronomy. On Saturdays, April 30th to June 11th.

Professor Seeley.—Three Lectures, On History. On Tuesdays, May 24th, 31st, and June 7th.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

Her Majesty's Government, through Sir R. I. Murchison—Memoirs of the Geological Survey:

Report on the Geology of Jamaica. 8vo. 1869.

Memoirs on the Geology of the Midland Counties, Derbyshire, and Yorkshire. 3 parts. 8vo. 1869.

Mineral Statistics for 1868. 8vo. 1869. Catalogue of Publications. 8vo. 1869.

Lords of the Admiralty—Nautical Almanac for 1873. 8vo. 1869.

Andrews, J. R. Esq. M.R.I. (the Author)—Life of Oliver Cromwell to the Death of Charles I. 8vo. 1869.

Asiatic Society of Bengal—Proceedings, 1869, No. 8. 8vo.

Bavarian Academy of Science, Royal—Abhandlungen. Band X. Zweite Abtheilung. 4to. 1868.

Denkschrift auf C. F. P. von Martius. 4to. 1869.

Beobachtungen des Meteorologischen Observatoriums 1851–66. 8vo. 1868.

Verzeichniss der 6323 Telescopischen Sterne. 8vo. 1869.

Booth, Miss—Memoir of the late Henry Booth of Liverpool and Manchester Railway. By Robert Smiles. 8vo. 1869.

British Museum Trustees—Guide to Second Vase Room. 8vo. 1869.

Chemical Society—Journal for Nov. 1869. 8vo.

Clinical Society—Transactions. Vol. II. 8vo. 1869.

Devonshire Association—Report and Transactions. July, 1869. 8vo.

Editors—Academy for Nov. 1869. 4to.

Artizan for Nov. 1869. 4to.

Athenæum for Nov. 1869. 4to.

Chemical News for Nov. 1869. 4to.

Engineer for Nov. 1869. fol.

Horological Journal for Nov. 1869. 8vo.

Journal of Gas-Lighting for Nov. 1869. 4to.

Mechanics' Magazine for Nov. 1869. 8vo.

Nature for Nov. 1869. 4to.

Pharmaceutical Journal for Nov. 1869. 8vo.

Photographic News for Nov. 1869. 4to.

Practical Mechanics' Journal for Nov. 1869. 4to.

Revue des Cours Scientifiques et Litteraires. Nov. 1869. 4to.

Scientific Opinion. Nov. 1869. 4to.

Faraday, Mrs. and Dr. Bence Jones—Life and Letters of Faraday. By Dr. Bence Jones. 2 vols. 8vo. 1869.

Franklin Institute—Journals, Nos. 525, 526. 8vo. 1869.

Genève, Société de Physique—Mémoires. Tome XX. Partie 1. 4to. 1869.

Geographical Society, Royal—Proceedings. Vol. XIII. Nos. 3, 4, 5. 8vo. 1869.

Geological Society—Quarterly Journal, Nos. 99, 100. 8vo. 1869.

Geological Society of Ireland. Journal, Vol. II. Part 1, 2. 8vo. 1869.

Leeds Philosophical Society—Annual Report. 1868–9. 8vo.

Mechanical Engineers' Institution, Birmingham—Proceedings, Aug. 1869. 8vo.

Medico-Chirurgical Society, Royal—Transactions. Vol. LII. 8vo. 1869.

Musée Teyler, Harlem—Archives, Vol. II. Fascicule 3. 8vo. 1869.

Odling, W. M.B. F.R.S. (the Author)—Outlines of Chemistry. 16mo. 1869.

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INDEX TO VOLUME V.

Abel, F. A., on some Applications of Electricity to Naval and Military Purposes, 479.
Abyssinia, or Ethiopia, 404.
Aeronautics in relation to Flight, 94.
Alloys and their Uses, 335.
Animals most nearly intermediate between Birds and Reptiles, 278.
Anderson, Charles, Assistant to Faraday, 206.
Annual Meetings (1867), 144; (1868), 375; (1869), 547.
Artificial Formation of Organic Substances, 378.
Art, Good Taste in, 376.
Atlantic Telegraph, 45.

Barrett, W., Experiments on Sounding and Sensitive Flames, 7, 9.
Barrows, in Yorkshire, explored, 78.
Bain, A., on the Doctrine of the Correlation of Force in its Bearing on Mind, 157.
Baker, Sir S. W., on Abyssinia, or Ethiopia, 404.
Biot, Experiments of, 188.
Blackie, J. S., on the Music of Speech in the Greek and Latin Languages, 145.
Brain, its Unconscious Activity, 338.
Bridges, Dr. John H., on the Influence of Civilization upon Health, 470.
Breech-loading Small Arms, 62.
Brown, Dr. Crum, on Chemical Constitution and its relation to Physical and Physiological Properties, 495.
Bye-laws, Repealed or Altered, 308.

Carpenter, W. B. on the Temperature and Animal Life of the Deep Sea, 503.
—— on the Unconscious Activity of the Brain, 338.
Carruthers, W., on the Cryptogamic Forests of the Coal Period, 511.
Cavendish Henry, his Balance, presented by Felix R. Garden, 403.
Chemical Actions, Rate at which they take place, 304.
—— Constitution and its relation to Physical and Physiological Properties, 495.
Chemical Rays, and the Light of the Sky, 429.
Chemistry of the Primeval Earth, 178.
Civilization, its Influence upon Health, 470.
Classical Education, Remarks on, 30, 273.
Clifford, W. Kingdon, on some of the Conditions of Mental Development, 311.
Coal, its Uses, 329.
—— Mines, their probable Exhaustion, 328.
—— Period, Cryptogamic Forests, 511.
Coast Defences of England, 458.
—— Defence, Moncrieff System of, 550.
Colouring Matters. Newest, 566.
Correlation of Force in its Bearing on Mind, 157.
Conway, M. D., on New England, 59.
Cryptogamic Forests of the Coal Period, 511.
Crystal Palace Fire, 18.

Deep Sea, Temperature and Animal Life of, 503.
Deutsch, E., on the Talmud, 386.
Diffusion of Gases, 12.
Donors to the Fund for the Promotion of Experimental Researches, 1, 4, 24, 76, 276, 309, 370, 403, 451, 549.

Early Mental Condition of Man, 83.
Earth, Primeval, 178.
Education, Public School, 26.
Electricity applied to Naval and Military Purposes, 47.
Ethiopia, or Abyssinia, 404.
Experiments of Savart, 188.

"Faraday as a Discoverer," by J. Tyndall, 199.
  Parentage: Introduction to the Royal Institution: Earliest Experiments: First Royal Society Paper: Marriage, 199.
  Early Researches: Magnetic Rotations: Liquefaction of Gases: Heavy Glass: Charles Anderson: Contributions to Physics, 203.

"Faraday as a Discoverer"—*continued.*
  Discovery of Magneto-electricity: Explanation of Arago's Magnetism of Rotation: Terrestrial Magneto-electric Induction: The Extra Current, 207.
  Points of Character, 214.
  Identity of Electricities: First Researches on Electro-Chemistry, 216.
  Laws of Electro-Chemical Decomposition, 222.
  Origin of Power in the Voltaic Pile, 224.
  Researches on Frictional Electricity: Induction: Conduction: Specific Inductive Capacity: Theory of Contiguous Particles, 227.
  Rest needed—Visit to Switzerland, 231.
  Magnetization of Light, 233.
  Discovery of Diamagnetism—Researches on Magno-Crystallic Action, 237.
  Supplementary Remarks, 242.
  Magnetism of Flame and Gases: Atmospheric Magnetism, 245.
  Speculations: Nature of Matter: Lines of Force, 250.
  Unity and Convertibility of Natural Forces: Theory of the Electric Current, 256.
  Summary, 261.
  Illustrations of Character, 262.
Faraday, M., Decease announced, 193.
—— his Legacy of MSS. and Books, 193.
Faraday, Mrs., Books presented by, 194.
—— Pension granted to, 276.
Farrar, Rev. F. W., on some Defects in Public School Education, 26.
—— on Public School Education, 273.
Fergusson, James, on Tree and Serpent Worship, 453.
Flames, Sounding and Sensitive, 6.
Flight in relation to Aeronautics, 94.
Frankland, E., on the Water Supply for the Metropolis, 109, 346.
—— on the Source of Light in Luminous Flames, 419.
Fuller, Francis, Promoter of Crystal Palace Building, 23.
Fullerian Professors appointed:
  Chemistry, W. Odling, 424.
  Physiology, M. Foster, 549.

Gases in Metals, 159.
—— in Waters, 365.
Gladstone, J. H., on some New Experiments on Light, 371.
Graham's Experiments, 159.
—— Recent Discoveries on the Diffusion and Occlusion of Gases, 12, 159.
Greek and Latin Languages, Music of Speech, 145.
Good Taste in Art, 376.
Greenwell, Rev. W., on the Yorkshire Wold Tumuli, 78.

Hamlet, on, 295.
Harcourt, A. Vernon, on the Rate at which Chemical Actions take place, 304.
Heat of the Oxy-hydrogen Flame, 391.
Heavenly Bodies, Spectrum Analysis applied to, 475.
Herschel, A. S., on the Shooting Stars of the Years 1866–67, and on the Probable Source of certain Luminous Meteors in the material Substance of the Zodiacal Light, 164.
—— on latest Eclipse of the Sun (*no abstract*), 450.
History, Influence of the Imagination on, 394.
Holland, Sir Henry, Donations from, 186, 403, 451, 605.
Huggins, W., on some further Results of Spectrum Analysis as applied to the Heavenly Bodies, 475.
Hunt, T. Sterry, on the Chemistry of the Primeval Earth, 178.
Huxley, T. H., on the Animals which are most nearly intermediate between Birds and Reptiles, 278.

Imagination, its Influence on History, 394.

Jenkin, H. C. Fleeming, on the Submersion and Recovery of Submarine Cables, 574.
Jervois, Colonel W. F. Drummond, on the Coast Defences of England, 458.
Jevons, W. Stanley, on the Probable Exhaustion of our Coal Mines, 328.

Laboratory Fund abolished, 308.
Lecky, W. E. H., on the Influence of the Imagination on History, 394.
Leconte, Professor, observes Sensitive Gas Flames, 6.
Lectures (1866–67), 4, 24; (1867–68), 197; (1868–69), 427; (1869–70), 611.
Light of the Sky, 429.
—— in Luminous Flames, its Source, 419.

Light, New Experiments on, 371.
Lockyer, J. Norman, on Recent Discoveries in Solar Physics made by means of the Spectroscope, 580.
Luminous Meteors, probable Source of, 164.

Magneto-Electric Machine presented by N. Wilde, 1.
Majendie, Captain V. D., on Military Breech-loading Small Arms, 62.
Man's Early Mental Condition, 83.
Matthiessen, A., on Alloys and their Uses, 335.
Mayow, Rev. M. W., on Hamlet, 295.
Mental Development, its Conditions, 311.
Metals, Occlusion of Gases in, 159.
Meteorological Office, Work of, 535.
Metropolitan Water Supply, 346.
Michael's, Mount St., Cornwall, 128.
Mind and the Correlation of Force, 157.
Modern Civilization, Survival of Savage Thought in, 522.
Moncrieff, Captain A., on the Moncrieff System of working Artillery as applied to Coast Defence, 550.
Music of Speech in the Greek and Latin Languages, 145.

Naval and Military Purposes, Application of Electricity to, 479.
New England, 59.

Occlusion of Gases by Metals, 159.
Odling, W., appointed Fullerian Professor of Chemistry, 424.
—— on Mr. Graham's Recent Discoveries on the Diffusion of Gases, 12.
—— on the Occlusion of Gases by Metals, 159.
—— on some Effects of the Heat of the Oxy-hydrogen Flame, 391.
—— on the Simplest Organic Compounds, 598.
Officers elected (1867), 145; (1868), 375; (1869), 548.
Organic Compounds, Simplest, 598.
Oxy-hydrogen Flame, Heat of, 391.

Palgrave, F. T., How to form a good Taste in Art, 376.
Pengelly, W., on the Insulation of St. Michael's Mount, Cornwall, 128.
Pettigrew, Dr. James Bell, on the various Modes of Flight in relation to Aeronautics, 94.
Pepys, John, Portrait of, presented by his Son, 24.
Perkin, W. H., on the newest Colouring Matters, 566.
Physical and Physiological Properties and Chemical Constitution, 495.
Primeval Earth, Chemistry of, 178.
Public School Education, Defects in, 26, 273.

Rate at which Chemical Actions take place, 304.
Recovery of Submarine Cables, 574.
Roscoe, H. E., on Vanadium, one of Trivalent Group of Elements, 287.
Ruskin, J., on the present State of Modern Art with reference to the advisable Arrangements of a National Gallery (*no Abstract*), 187.
—— on the Flamboyant Architecture of the Valley of the Somme (*no Abstract*), 450.
Russell, J. Scott, Esq., on the Crystal Palace Fire, 18.

Savage Thought in Modern Civilization, 522.
Scott, R. H., on the Work of the Meteorological Office, Past and Present, 535.
Serpent Worship, 453.
Sewage Contamination of Water, 360–363.
Shooting Stars of the Years 1866–67, 164.
Simplest Organic Compounds, 598.
Soap, Quantities destroyed by various Waters, 358.
Solar Physics made by means of the Spectroscope, Recent Discoveries in, 580.
Sounding and Sensitive Flames, 6.
Spectrum Analysis applied to the Heavenly Bodies, 475, 580.
Speech, Music of, in Greek and Latin, 145.
Storm Warnings, 539.
Submersion and Recovery of Submarine Cables, 574.
Substances, Artificial Formation of, 378.
Stewart Balfour on the Sun as a Variable Star, 138.
Superstitions, Ancient and Modern, 87, 523.

Talmud, on the, 386.
Telegraphic Weather Intelligence, 539.
Tree and Serpent Worship, 453.
Tumuli, Yorkshire Wold, 78.
Tylor, E. B., on Traces of the Early Mental Condition of Man, 83.

"Faraday as a Discoverer"—*continued.*
  Discovery of Magneto-electricity: Explanation of Arago's Magnetism of Rotation: Terrestrial Magneto-electric Induction: The Extra Current, 207.
  Points of Character, 214.
  Identity of Electricities: First Researches on Electro-Chemistry, 216.
  Laws of Electro-Chemical Decomposition, 222.
  Origin of Power in the Voltaic Pile, 224.
  Researches on Frictional Electricity: Induction: Conduction: Specific Inductive Capacity: Theory of Contiguous Particles, 227.
  Rest needed—Visit to Switzerland, 231.
  Magnetization of Light, 233.
  Discovery of Diamagnetism—Researches on Magno-Crystallic Action, 237.
  Supplementary Remarks, 242.
  Magnetism of Flame and Gases: Atmospheric Magnetism, 245.
  Speculations: Nature of Matter: Lines of Force, 250.
  Unity and Convertibility of Natural Forces: Theory of the Electric Current, 256.
  Summary, 261.
  Illustrations of Character, 262.
Faraday, M., Decease announced, 193.
—— his Legacy of MSS. and Books, 193.
Faraday, Mrs., Books presented by, 194.
—— Pension granted to, 276.
Farrar, Rev. F. W., on some Defects in Public School Education, 26.
—— on Public School Education, 273.
Fergusson, James, on Tree and Serpent Worship, 453.
Flames, Sounding and Sensitive, 6.
Flight in relation to Aeronautics, 94.
Frankland, E., on the Water Supply for the Metropolis, 109, 346.
—— on the Source of Light in Luminous Flames, 419.
Fuller, Francis, Promoter of Crystal Palace Building, 23.
Fullerian Professors appointed:
  Chemistry, W. Odling, 424.
  Physiology, M. Foster, 549.

GASES in Metals, 159.
—— in Waters, 365.
Gladstone, J. H., on some New Experiments on Light, 371.
Graham's Experiments, 159.
—— Recent Discoveries on the Diffusion and Occlusion of Gases, 12, 159.
Greek and Latin Languages, Music of Speech, 145.
Good Taste in Art, 376.
Greenwell, Rev. W., on the Yorkshire Wold Tumuli, 78.

HAMLET, on, 295.
Harcourt, A. Vernon, on the Rate at which Chemical Actions take place, 304.
Heat of the Oxy-hydrogen Flame, 391.
Heavenly Bodies, Spectrum Analysis applied to, 475.
Herschel, A. S., on the Shooting Stars of the Years 1866–67, and on the Probable Source of certain Luminous Meteors in the material Substance of the Zodiacal Light, 164.
—— on latest Eclipse of the Sun (*no abstract*), 450.
History, Influence of the Imagination on, 394.
Holland, Sir Henry, Donations from, 186, 403, 451, 605.
Huggins, W., on some further Results of Spectrum Analysis as applied to the Heavenly Bodies, 475.
Hunt, T. Sterry, on the Chemistry of the Primeval Earth, 178.
Huxley, T. H., on the Animals which are most nearly intermediate between Birds and Reptiles, 278.

IMAGINATION, its Influence on History, 394.

JENKIN, H. C. Fleeming, on the Submersion and Recovery of Submarine Cables, 574.
Jervois, Colonel W. F. Drummond, on the Coast Defences of England, 458.
Jevons, W. Stanley, on the Probable Exhaustion of our Coal Mines, 328.

LABORATORY Fund abolished, 308.
Lecky, W. E. H., on the Influence of the Imagination on History, 394.
Leconte, Professor, observes Sensitive Gas Flames, 6.
Lectures (1866–67), 4, 24; (1867–68), 197; (1868–69), 427; (1869–70), 611.
Light of the Sky, 429.
—— in Luminous Flames, its Source, 419.

Light, New Experiments on, 371.
Lockyer, J. Norman, on Recent Discoveries in Solar Physics made by means of the Spectroscope, 580.
Luminous Meteors, probable Source of, 164.

Magneto-Electric Machine presented by N. Wilde, 1.
Majendie, Captain V. D., on Military Breech-loading Small Arms, 62.
Man's Early Mental Condition, 83.
Matthiessen, A., on Alloys and their Uses, 335.
Mayow, Rev. M. W., on Hamlet, 295.
Mental Development, its Conditions, 311.
Metals, Occlusion of Gases in, 159.
Meteorological Office, Work of, 535.
Metropolitan Water Supply, 346.
Michael's, Mount St., Cornwall, 128.
Mind and the Correlation of Force, 157.
Modern Civilization, Survival of Savage Thought in, 522.
Moncrieff, Captain A., on the Moncrieff System of working Artillery as applied to Coast Defence, 550.
Music of Speech in the Greek and Latin Languages, 145.

Naval and Military Purposes, Application of Electricity to, 479.
New England, 59.

Occlusion of Gases by Metals, 159.
Odling, W., appointed Fullerian Professor of Chemistry, 424.
—— on Mr. Graham's Recent Discoveries on the Diffusion of Gases, 12.
—— on the Occlusion of Gases by Metals, 159.
—— on some Effects of the Heat of the Oxy-hydrogen Flame, 391.
—— on the Simplest Organic Compounds, 598.
Officers elected (1867), 145; (1868), 375; (1869), 548.
Organic Compounds, Simplest, 598.
Oxy-hydrogen Flame, Heat of, 391.

Palgrave, F. T., How to form a good Taste in Art, 376.
Pengelly, W., on the Insulation of St. Michael's Mount, Cornwall, 128.
Pettigrew, Dr. James Bell, on the various Modes of Flight in relation to Aeronautics, 94.
Pepys, John, Portrait of, presented by his Son, 24.
Perkin, W. H., on the newest Colouring Matters, 566.
Physical and Physiological Properties and Chemical Constitution, 495.
Primeval Earth, Chemistry of, 178.
Public School Education, Defects in, 26, 273.

Rate at which Chemical Actions take place, 304.
Recovery of Submarine Cables, 574.
Roscoe, H. E., on Vanadium, one of Trivalent Group of Elements, 287.
Ruskin, J., on the present State of Modern Art with reference to the advisable Arrangements of a National Gallery (*no Abstract*), 187.
—— on the Flamboyant Architecture of the Valley of the Somme (*no Abstract*), 450.
Russell, J. Scott, Esq., on the Crystal Palace Fire, 18.

Savage Thought in Modern Civilization, 522.
Scott, R. H., on the Work of the Meteorological Office, Past and Present, 535.
Serpent Worship, 453.
Sewage Contamination of Water, 360–363.
Shooting Stars of the Years 1866–67, 164.
Simplest Organic Compounds, 598.
Soap, Quantities destroyed by various Waters, 358.
Solar Physics made by means of the Spectroscope, Recent Discoveries in, 580.
Sounding and Sensitive Flames, 6.
Spectrum Analysis applied to the Heavenly Bodies, 475, 580.
Speech, Music of, in Greek and Latin, 145.
Storm Warnings, 539.
Submersion and Recovery of Submarine Cables, 574.
Substances, Artificial Formation of, 378.
Stewart Balfour on the Sun as a Variable Star, 138.
Superstitions, Ancient and Modern, 87, 523.

Talmud, on the, 386.
Telegraphic Weather Intelligence, 539.
Tree and Serpent Worship, 453.
Tumuli, Yorkshire Wold, 78.
Tylor, E. B., on Traces of the Early Mental Condition of Man, 83.

Tylor, E. B., on the Survival of Savage Thought in Modern Civilization, 522.
Tyndall, J., on Sounding and Sensitive Flames, 6.
—— on some Experiments of Faraday, Biot, and Savart, 188.
—— on Faraday as a Discoverer, 199.
—— on Chemical Rays, and the Light of the Sky, 429.

Unconscious Activity of the Brain, 338.

Vanadium, 287.
Varley, C. F., on the Atlantic Telegraph, 45.
Vernon, Lord, Present of Dante from, 607.

Water Analyses, 113, 122, 359.
Water Supply for the Metropolis, 109, 346.
Wilde, H., presented Magneto-Electric Machine, 1.
Williams, C. G., on the Artificial Formation of Organic Substances, 378.
—— on the Female Poisoners of the Sixteenth and Seventeenth Centuries (*no Abstract*), 470.

Zodiacal Light, Substance of, 164.

END OF VOL. V.

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